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# JOURNAL

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# Institution of Electrical Engineers.

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At a Special General Meeting of Members, held at the Institution of Civil Engineers, 25, Great George Street, Westminster, on Thursday, January 10th, 1889 — Mr. Edward Graves, late President, in the Chair—

The notice convening the meeting was read by the Secretary.

The Chairman moved that the following resolution, passed at the Special General Meeting of Members on the 20th December, 1888, be now confirmed, viz.:—

"That the Regulations of the Society of Telegraph-Engineers "and Electricians, as contained in their Memorandum and "Articles of Association, be altered by substituting the name "'The Institution of Electrical Engineers' for 'The Society of "'Telegraph-Engineers and Electricians,' and also by sub-"stituting the word 'Institution' for the word 'Society,' "wherever the same respectively occur in the regulations."

The motion, having been seconded by Professor D. E. Hughes, Past-President, was carried unanimously.

The One Hundred and Eighty-third Ordinary General Meeting of the Institution was held at the Institution of Civil Engineers, 25, Great George Street, Westminster, on Thursday, January 10th, 1889—Mr. Edward Graves, Past-President, in the Chair.

The Chairman: In order to shorten the formal proceedings this evening, we propose to defer reading the minutes of the Annual General Meeting of December 13th. They are not particularly novel or interesting, but are necessarily rather long, on account of the numerous votes of thanks recorded therein. With your permission, therefore, we will defer the reading of them until the next meeting.

The names of new candidates for election into the Institution were announced and ordered to be suspended.

The following transfers were announced as having been approved by the Council:—

From the class of Associates to that of Members—

James Thomson Bottomley, F.R.S.
George Fuller.
William Geipel.
Walter Thomas Goolden, M.A.
William P. Granville.
Frank King.
William M. Mordey.
Lieut.-Col. M. T. Sale, C.M.G., R.E.
Albion T. Snell.
Henry Upton.
T. Fred. Toft.
John B. Verity.

From the class of Students to that of Associates—

Edgar T. Gideon.
John Rance, jun.
C. P. Sparks.
Edward Wythe Smith.
Percival S. Tasker.
Ernest George Tidd.
Walter R. Underhill.

The Chairman: I have now to announce that the last legal steps have been taken in reference to the change of name of the Society, and that in future the "*Institution of Electrical Engineers*" will take the place of the "Society of Telegraph-Engineers and Electricians."

Before resigning the chair I have one more agreeable duty to

perform, and that is to present the premiums awarded by the Council; but I will first call upon the Secretary to read a letter which has been received from Professor Silvanus Thompson.

The SECRETARY: Members will recollect that the principal premium of the Institution was awarded to Professor Silvanus Thompson for his paper on "The Influence Machine, from 1788 "to 1888." In reply to my letter announcing that award, he writes as follows:—

20, ARUNDEL GARDENS, W.,
*December* 15*th*, 1888.

DEAR MR. WEBB,—I am in receipt of yours of the 14th inst., announcing that the Council of the Society of Telegraph-Engineers and Electricians have awarded to me the "Society's Premium" for 1888. I need not say that I gratefully acknowledge the honour which the Council have done me in selecting me to be the recipient of this award. I should value highly such an addition to my library, and deeply appreciate the expression of the Council's goodwill.

That the original intention of the Society in establishing this premium was to offer it for the encouragement of the efforts of the junior members of the Society is, however, obvious; and I, having already once served on your Council, viz., in 1886, and having just been again elected, cannot but feel that, though technically I may be entitled to the honour, the interests of the Society will be best served if the Council will revert to the original intention of the award.

I therefore ask the Council to allow me most respectfully to decline the premium.

Believe me, dear Mr. Webb,
Yours very truly,
(Signed) SILVANUS P. THOMPSON.

F. H. WEBB, Esq.

The CHAIRMAN: I do not doubt that you will highly appreciate, as the Council do, the consideration shown by Professor Silvanus Thompson in his decision; and I think also that the course recommended by the Premium Committee under the circumstances, and which has been adopted by the Council, will meet with your approval, viz., that Professor Silvanus Thompson having declined to receive the premium awarded to him for the reasons given by him, Mr. A. C. Cockburn, who was to receive the second prize, should receive the first; that Mr. E. Stallibrass, who was to receive the third prize, should receive the second; and that Mr. E. O. Walker, whose communication, "Earth Currents "in India," was highly commended, should receive the third prize.

The meeting having signified approval, the following prizes were presented by Mr. Graves, viz.:—

To Mr. A. C. Cockburn, Member, the "Society's Premium," value £10, consisting of a chemical balance, Prescott's "Dynamo-Electricity," and Prescott's "Electricity and "Magnetism."

To Mr. Edward Stallibrass, Member, the "Paris Electrical "Exhibition Premium," value £5, consisting of Spon's "Dictionary of Engineering," in four volumes.

The Chairman: Mr. E. O. Walker, to whom the "Fahie Pre- "mium" has been awarded, is in India, and the Secretary has not yet had time to ascertain in what form he wishes to receive it.

The Chairman: My remaining duty is to introduce to you as your President Sir William Thomson, in whose favour I vacate this chair.

The President, Sir William Thomson, then took the chair, and delivered the following Inaugural Address:—

## ETHER, ELECTRICITY, AND PONDERABLE MATTER.

Gentlemen,—My first duty is to give you my warmest thanks for the great honour you have done me in electing me to be the first President of the Institution of Electrical Engineers. Fourteen years ago, when the three-year-old Society of Telegraph Engineers honoured me by appointing me to be their President, the Society numbered 570 members; it now numbers 1,500. It is gratifying to us to think how that young Society has grown, and how successful it has been.

But while we think with pleasure of the great increase of our numbers, that pleasure is saddened by the thought that a great many of the old members are gone, and especially the first two Presidents—Sir William Siemens and Mr. Frank Scudamore. The genial presence of Sir William Siemens is a happy recollection to many of us—I think I might say to nearly all present—and the thought that he is no more with us is certainly a very great grief to all who have known him, and a loss to Science and

to England. We look back upon Mr. Scudamore's presidency with great satisfaction, considering all that he did for the Society, and his loss is deeply felt. Three of the Presidents since I had the honour to serve are also gone—Mr. Walker, Sir John Bateman-Champain, and, within the past year, Sir Charles Bright. The first Atlantic cable of 1857 gave me the happiness and privilege of acquaintanceship with Sir William Siemens. In the course of the work Sir Charles Bright was my colleague. He was engineer to the company, and during the thirty-three days when we were out of sight of land in the ever-memorable "Agamemnon" expedition of 1858, Sir Charles Bright was with us, full of vigour and enthusiasm. To his vigour and earnestness and enthusiasm, in a great measure was due the existence of that cable—the temporarily successful cable of 1858—and all the great consequences which followed from it, even although that cable itself had a very short life, having only three months of working time, and a still shorter time of really useful, practical work. Still, we must always feel deeply indebted to Sir Charles Bright as a pioneer in that great work, when other engineers would not look at it, and thought it was absolutely impracticable; and we must always look upon our late colleague, lost within the last year, as having done much indeed for the subject of the Society of Telegraph Engineers.

The Society of Telegraph Engineers has grown not only in membership, but in the extent of its province, from the time of its foundation until now. It became the "Society of Telegraph-"Engineers and Electricians" a few years ago, and now the more properly representative title of the "Institution of Electrical "Engineers" has been adopted. The original name included only telegraphy, but that was not then the only application of electricity to engineering. There was electro-metallurgy. Electro-metallurgy and telegraphy were the two, and I think the only two, branches of practical science to which electricity had then been applied; but since that time we have a vast augmentation of the field. We have telephony; we have electric transmission of power. The Society of Telegraph Engineers will recollect Sir William Siemens's introduction to it of that great subject of the electric transmission of power on

a scale of practical usefulness. I think we may safely feel that to the Society of Telegraph Engineers in a large measure is due that practical development of electricity. We have now power electrically transmitted through factories to drive separate machines by separate motor dynamos; we have electric haulage and electric tramcars; we have the application of electricity to naval and military purposes; and last, not least, we have the application of electricity to electric lighting. With all these grand subjects of applied practical science for our province, I think the Institution of Electrical Engineers may feel that it has a great and noble dominion.

But we must not forget that the province of the electrical engineer necessarily touches upon that of the civil engineer. When fourteen years ago I gave my Inaugural Address, I endeavoured to impress upon engineers and architects that architects made a great mistake in not being engineers—in not qualifying themselves as engineers, and doing the work of engineers — that architects do not do their duty to their clients in not being engineers and understanding the engineering of their own works, and making engineering science, particularly the dynamics of engineering, an essential part of the training of an architect. It is not necessary to make any animadversion, I think, upon electrical engineers in this respect. Electrical engineers know well that they must, before all, be engineers. They must be engineers, and they must learn electricity.

To young persons who have a taste for electrical machines (and who that is young has not a taste for electrical machines, and sparks, and flashes, and aurora borealis artificially made, and the smell of ozone—sulphur and phosphorus we used to call it!—which is one of the pleasing reminiscences of one's youth in working with electrical machines?) a word of advice may be useful. Every young person who has a fancy for electricity thinks he would like to be an electrical engineer. They think electrical engineering is all ether and electricity. Now I have continually to impress upon anxious fathers and mothers that their boys must condescend to learn something of gross ponder-

able matter, and that electrical engineering is not confined to ether and electricity, but mechanics also is an essential part of the subject. It is, I think, an important practical point this —that the electrical engineer, or the youth or aspirant to that honourable profession, ought to learn mathematics and dynamics after having obtained the elements of a good general education. He ought to learn mathematics and dynamics well. Then a good deal of chemistry and regular mechanical and civil engineering should all be learnt; and electricity learnt besides. It may be said juvenile life is too short. I do not think it is. I think if the other subjects are well learnt, electricity may be learnt in a few months. I am perfectly sure that if the youth is qualified in other departments, the mere addition of electricity to the education of a competent engineer will not take so very long a time as might be imagined, and that the merely educational part of the work will not be protracted unduly by adding electricity to the branches learnt in general engineering. I do not mean to say that if electrical engineering is the branch adopted, there is not an endless and prodigious field of electricity proper in which the worker will learn every day of his life, though he lives for more years than any person present. I wished just to make these few remarks in the beginning, because they do seem to me of some practical importance, and worthy, therefore, of being put in the front of the Address I have to offer as first President of the Institution of Electrical Engineers.

Now these remarks have suggested to me a subject for such a scientific exposition as I could possibly attempt to give in this opening Address—"Ether, Electricity, and Ponderable Matter." The demand for something like a mechanical explanation of electrical phenomena is not new, but it is growing in intensity every year. The proceedings of recent meetings of the British Association—and especially of the last meeting of the British Association—illustrate the growing desire to know something below the surface; to know something of the internal relations connected with the wonderful manifestations of force and energy which are put before us in the action of the magnet, in the working even of a common electrical machine, and in electro-magnetic phenomena.

The Addresses of Past-Presidents of the parent Societies of Telegraph Engineers, and Telegraph-Engineers and Electricians illustrate also the growing desire to know something of the molecular theory or the dynamical theory of electricity and magnetism. Mr. Preece, in his Address of 1880, pointed out how Maxwell had shown the velocity of light to be related to electricity in such a way that we can scarcely doubt but that the propagation of electro-magnetic disturbance through space, which we have every reason to believe does exist—which, in fact, from known laws we may say certainly does exist—is effected with a velocity equal to that of light, and that the propagation of electrical disturbance and of light may perhaps be identical. In support of these remarks, Mr. Preece alluded to the disturbances at the sun's surface and the simultaneous magnetic disturbances which had been observed in the telegraphs and in other operations of an electro-magnetic character on the surface of the earth.

In 1883 Mr. Willoughby Smith described shortly some experiments which I consider to be very beautiful and very instructive, with which he was then engaged. Those experiments demonstrated and illustrated the screening effect of sheets of different kinds of metal upon electro-magnetic and electrostatic inductions. Electric induction, simply, we may say, because we begin to fail to distinguish between electrostatic induction and electro-magnetic induction. In Willoughby Smith's subsequent work he gave an exceedingly beautiful set of experimental investigations of the screening effect of lead, copper, and iron, of which, as I have said, a slight sketch was given in his Presidential Address. A little earlier the subject was mathematically worked out with great power by several mathematicians, but perhaps most notably by Horace Lamb. I feel it almost invidious to mention names when there are so many thorough workers who touched upon the same subject very closely. Charles Niven,* almost simultaneously with Lamb,† went through very much the same kind of work—

* "On the Induction of Electric Currents in Infinite Plates and Spherical "Shells," *Phil. Trans. Roy. Soc.*, 1881, p. 307 (read Jan. 29th, 1880).

† "On Electrical Motions in a Spherical Conductor," *Phil. Trans. Roy. Soc.*, 1883, p. 519 (read April 5th, 1883).

in fact, obtained the same solutions of some important and interesting problems regarding electric currents in spherical conductors. I specially mention Lamb's name because the subject of screening is more particularly developed in his mathematical paper.

In the memorable Presidential Address of Professor Hughes, another allied branch of electro-magnetic induction was very admirably illustrated by experiments which are now more or less familiar to us all, but which have been of an immensely suggestive and stimulating character, both to mathematicians and to experimental workers. The very criticisms by mathematicians upon some of the experiments and modes of statement by Professor Hughes have, with Professor Hughes's own experiments, given a very large body of electric knowledge and electro-magnetic knowledge which, without such stimulus and such mathematical and experimental scrutiny as it has led to, might have been wanting for many a year.

One of the earliest problems in which electric induction had to be considered was that of the submarine telegraph. The subject of induction in telegraph wires presented itself in a peculiarly perplexing way to the first workers in that department. There was the general knowledge of electro-magnetic induction between two wires, which had been worked out by Henry and Faraday in a very full manner. That was the only kind of induction which was thought of by some of the pioneers of submarine telegraphy. Another kind of induction was more thought of by others. That was the electrostatic induction due to the Leyden-jar charge of the insulated wire. Faraday, in this department, as in the other department, was the origin of nearly all that we now know. He explained in a very beautiful and clear way the electrostatic charge of the submarine cable, and showed how the electricity conducted through the cable from one end, to give what of potential is necessary in the middle or the other end of the cable—in the middle of the cable for any mode of working, at the other end of the cable for modes of working in which the other end is insulated—gave rise to the Leyden-jar charge. He pointed out (without going into any of these details,

however) the doctrine of the conduction of electricity through the wire to supply the Leyden-jar charge which the wire must have, in the course of working, in order to be raised to the difference of potential from the earth required to cause the signal current to pass through it. Cromwell Varley made very important advances in that direction. At the meeting of the British Association in 1854, at Liverpool, he brought forward some important developments of Faraday's doctrine. And then came on the great Atlantic Cable question. I always remember how that question came upon me. I see in Professor Stokes's presence with us this evening a reminder of the circumstances. I was hurriedly leaving the meeting of the British Association, when a son of Sir William Hamilton, of Dublin, was introduced to me with an electrical question. I was obliged to run away to get to a steamer by which I was bound to leave for Glasgow, and I introduced him to Professor Stokes, who took up the subject with a power which is inevitable when a scientific question is submitted to him. He wrote to me on the subject soon after that time, and some correspondence between us passed, the result of which was that a little mathematical theory was worked out, which constituted, in fact, the basis of the theory of the working of the submarine cable. In that theory, electro-magnetic induction was not taken into account at all. The leaving it out of account was justified by the speed of signalling which the circumstances of a cable exceeding 200 or 300 miles in length dictated. For a cable more than 200 or 300 miles long the speed of working was essentially limited by these electrostatic considerations—limited so much that the electro-magnetic induction certainly could have no sensible effect. But the possible speed of working in a cable of 20 miles or 50 miles, or even 100 miles, was so great that in short lengths like that the electro-magnetic induction might well come into play. I worked out the subject partially myself. I found it necessary to do so to satisfy myself that the doctrine upon which the Atlantic Cable project, then growing up, was ultimately founded, was thoroughly trustworthy. I found it necessary to investigate the question of electro-magnetic induction. This question was

further forced upon me by communications that I had with my friends, Lewis Gordon, and the two brothers, Charles Wm. Siemens and Werner Siemens, with reference to Mediterranean cables. It was imagined that electro-magnetic induction alone was operative—that embarrassment in working through the submarine cable was due to electro-magnetic induction alone. On its being demonstrated that electro-magnetic induction could have no sensible effect on the signalling through proposed Mediterranean cables, the proposal to have two thin wires close together in order to obviate electro-magnetic induction was given up. Experiments in Germany had shown considerable electro-magnetic induction on short lengths of cable, and it had been supposed that there would be embarrassment from this cause in the working of the cables, which would be diminished by using wires very close together. But this diminution of the electro-magnetic inductive influence would produce a corresponding increase in the electrostatic inductive influence; and when it was pointed out that the electro-magnetic inductive influence would be absolutely imperceptible at the highest speeds of working of the proposed cables, and that it would be the electrostatic induction which would limit the speed, the idea of making them of thin twin wires—two pairs of wires close together in metallic circuit—was given up, and the present type of submarine cables was adopted. But now it is very interesting to us to find that old question revived. I had myself laid it aside in some corner of my mind and in some slight corners of my note-books for forty years. Within the last forty days I have really worked it out to the uttermost, merely for my own satisfaction. But in the meantime it had been worked out in a very complete manner by Mr. Oliver Heaviside; and Mr. Heaviside has pointed out and accentuated this result of his mathematical theory—that electro-magnetic induction is a positive benefit: it helps to carry the current. It is the same kind of benefit that mass is to a body shoved along against a viscous resistance. Suppose, for instance, you had a railway carriage travelling through a viscous fluid. Take a boat on wheels, so that the more massive body will not sink deeper in

the fluid than the less massive body. Take a boat on wheels in a viscous fluid. We will shove off two boats with a certain velocity —the boats of the same shape; but let one of them be loaded to ten times the mass of the other: it will take greater force to give it its impulse, but it will go further. That is Mr. Oliver Heaviside's doctrine about electro-magnetic induction. It requires more electric force to produce a certain amount of current, but the current goes further. It is a very crude way in which I am putting it. I am not doing justice, of course, I know, to his statement in one short sentence. The whole question is treated in the most complete mathematical way. The effect of electro-magnetic induction and electrostatic induction taken together (and they cannot be separated) is fully worked out. One thing that was known of old is made a point of in Mr. Heaviside's treatment of the cable problem—that is, the beneficial effect of leakage in respect to clearness of signals. Old telegraphists remember that. They always used to say three or four good leaks in a cable, if they would but kindly remain constant, and not introduce extra trouble by earth currents, would make the signalling more distinct. That used to be well known, and the reason used to be fairly well known; mathematical theory had pointed it out. Now Mr. Oliver Heaviside has taken up that subject again and included it in his work. He has included it along with electro-magnetic induction, and this point he has particularly accentuated. It is a practical point of importance that the question of clearness of signals is not simply or even very importantly this—How much is the current attenuated at the remote end of the cable? how much is the amplitude of the electric current in one mode of working, or of the variation of electric potential in another mode of working, altered in transmission through a thousand miles or two thousand miles of cable? A certain range is given at the sending end; and what is the range at the receiving end? That is an important question, but it is not the most important question with reference to clearness of signalling; in fact, we might almost say it is not an important question at all. It is not the smallness of the signals at the receiving end that is the real difficulty in a submarine cable just now at all; it is the running of one signal

into another; it is the want of correspondingly definite distinctions of single signals or of a group of signals at the receiving end and at the sending end.

Now in the mathematical theory there are two things to be considered in respect to the distortion (as Heaviside called it) of the signals in passing through the cable. One thing to be considered is the retardation of phase; another is the diminution of amplitude. If the retardation of phase were the same for alternating currents of all periods, then this retardation of the phase would be of no consequence whatever—it could not diminish the distinctness at all. Again, if the diminution of the amplitude were precisely in the same proportion for alternating currents of all periods, then when we come to make non-periodic signals we should find that the signals would be transmitted with perfect sharpness. In fact, if we compare the transmission of electric signals through a wire with the transmission of sound through air, we have in the course of transmission of sound through air great attenuation by distance—inversely as the square of the distance, in fact—but the same for all notes; and, again, retardation of phase depending upon the velocity of the sound, the same for all notes. The result is that speaking, and musical performances, and signals of all kinds in air, lose none of their clearness by distance. It is just a question whether at the very greatest distance at which a sound can be heard there is any want of clearness due to different attenuations of the different notes or of the different elements forming the compound sound, or to difference of retardations of phase. I must not occupy you too long with this subject, but it is one of large practical importance. Heaviside points out that electro-magnetic induction causes a less great difference in the attenuation of different periods than there is without it; and that electro-magnetic induction (as we knew forty years ago) tends to reduce the retardation of phase to the same for all different notes—that is, to the retardation equal to what would depend on a velocity not very different from the velocity of light—if the signals have but sufficient frequency. That velocity was then and is still known

as the velocity which is the conductance in electrostatic measure, and the resistance in electro-magnetic measure of one and the same conductor. But its relationship to the velocity of light was brought out in a manner by Maxwell to make it really a part of theory which it never was before. Maxwell pointed out its application to the possible or probable explanation of electric effects by the influence of a medium, and showed that that medium—the medium whose motions constitute light—must be ether. Maxwell's "electro-magnetic theory of light" marks a stage of enormous importance in electro-magnetic doctrine, and I cannot doubt but that in electro-magnetic practice we shall derive great benefit from a pursuing of the theoretical ideas suggested by such considerations. In fact, Heaviside's way of looking at the submarine cable problem is just one instance of how the highest mathematical power of working and of judging as to physical applications, helps on the doctrine, and directs it into a practical channel.

The telephone—one of the added subjects of the Institution of which we are members—illustrates very splendidly these developments of the theory of the transmission of signals through the submarine cable. The telephonic signals have, in fact, sufficient frequency to make electro-magnetic induction very sensibly influential. The frequencies in telephony correspond to from 250 periods per second, up to two, or three, or four times that; being the frequencies involved in speaking in the human voice—tenor and soprano—and in the quality of the voice as affected by the over-tones. I say frequencies of from 250 per second to 1,000 or 1,500 periods per second, are concerned in the fundamental notes, and in the characterising over-tones, of the sounds transmitted by the telephone. Now there seems no doubt but that the clearness of the telephone through great distances is to a large degree due to the circumstance which Heaviside has pointed out—that we have much less of difference of attenuation and difference of phasal retardation, for different notes, with the actual frequencies of the notes in sounds transmitted through the telephone wire, and the practical dimensions of the telephone wire, than we should have without electro-magnetic induction.

I cannot speak on this subject without just touching upon a question which I do not at all propose to enter upon to-night, and that is, the relative efficiencies of iron and copper as telephonic conductors. Information given to me by Mr. Bennett, the engineer of the National Telephone Company in Scotland, leaves no doubt whatever but that there is a very considerably greater loss of clearness in speaking through an iron wire—such, for instance, as the first metallic circuit iron wire of the Post Office between Glasgow and Edinburgh—than there is in speaking through the present copper wire circuit of the company between the same stations. I shall say nothing more of this just now. It is an exceedingly difficult and complicated subject. But Heaviside's mathematical work, and Lord Rayleigh's experimental investigations on the susceptibility of iron to very small differences of magnetic force—differences of magnetic force superimposed on even a powerful magnetic force or a large residual magnetism—put us, I think, in a very hopeful condition. The subject is in good hands, mathematical and practical, and I think before many months pass over the Institution of Electrical Engineers, we may have an absolutely clear understanding of telephony through iron wire as compared with telephony through copper wire.

Leaving all questions of submarine telegraphy, and of telegraphy or telephony, in which—whether from the greatness of the distance through which the communications are made, or smallness of distance between insulated conductor and sheath, or between the twin wires when insulated metallic circuit is used—the effects of electrostatic capacity give rise to sensible difference of strength of current in different parts along the length of the conductor—I wish to call your attention to the differences of current-density across different parts of the cross section, which are produced when alternate currents are sent through a wire. Consider a copper wire, and a copper tube surrounding it for return. Or consider what is, after all, one of the very simplest cases—two parallel copper wires. If the distance between them is a large multiple of the diameter of each, as is the case in practical telegraphy and telephony, the problem is the same as

the problem of a single copper wire in the centre of a cylindrical tube of infinitely conductive metal, and of radius equal to the distance between the wires. The distribution of current within the solid conductor depends only on the period of the alternations, and on the diameter and the specific resistance of the metal; and is quite independent of the surroundings, provided only they be symmetrical all round, or provided, if the case be that of two parallel wires, the distance between the two wires be a large multiple of the diameter of each, so that the current in each is not sensibly disturbed, by the influence of the other, from being arranged in co-axial cylindric layers of equal current-density. For this problem the mathematical theory gives us a remarkably interesting and very useful practical result; and I really, in proposing to speak upon such a very abstruse and uninteresting subject as "Electricity, Ether, and Ponderable Matter," wish to try to give one little piece of practical information to-night. It will be no information to some, but it may be information to others. The solution, expressed in a formula, and a table of numerical results calculated from it, I hope, will appear complete in the report of this Address. I think I might give you just now two or three of the numbers that are rather interesting. Take 80 periods per second as the frequency — that is about what is adopted in the alternate-current system of distribution for electric light; at all events in one great system I know—the Grosvenor Gallery installation—that is the frequency of the period; and I believe it is pretty much the same generally. Let us take, then, as an example, the 80 periods per second. First, consider copper wire of 1 centimetre diameter: the ohmic effective resistance is greater than for steady current through the same wire, but not as much as $\frac{1}{100}$ per cent. greater. Take, now, copper wire of $1\frac{1}{2}$ centimetres diameter: the ohmic effective resistance is $2\frac{1}{2}$ per cent. greater than the resistance for steady current. Next, take copper wire of 2 centimetres diameter: the ohmic resistance is 8 per cent. more for the alternating current than for the steady current. In round copper rod of 4 centimetres diameter, the ohmic resistance is 68 per cent. more for the 80 periods per second alternate currents than for steady currents. In round copper bar of 10

centimetres diameter, the ohmic resistance is 3·8 times what it would be for the steady current. In a solid copper cylinder of 100 centimetres diameter the ohmic resistance is 35 times greater than for steady currents. From 10 centimetres diameter upwards the ohmic effective conductance—that is, the reciprocal of the ohmic effective resistance—increases scarcely more than as the diameter simply, and not as the square of the diameter. The conductance for steady currents is as the square of the diameter all through. The effective conductance for alternate currents follows a law which can only be expressed by aid of Fourier-Bessel functions till we get to very great diameters. When we get to so great a diameter that the shell, or outer portion, of the wire into which the current is practically confined, is moderate or small in proportion to the diameter of the wire, then, for diameters exceeding that, you can all see perfectly without calculation, that the conductance is in simple proportion to the circumference, and therefore in simple proportion to the diameter. This very imperfect explanation of the results may give some idea which, I think, is of rather an interesting and important kind, but the figures will speak for themselves. With quadruple frequency, the same figures apply to wires of half diameter. There we get the telephone problem. Four times 80 is 320, which is among the frequencies for telephonic notes; and for the 320 frequency, take the figures I have given, but with half the linear magnitudes. Thus, for instance, for copper wire of 1 centimetre diameter, transmitting musical notes of 320 periods per second, the ohmic resistance is 8 per cent. greater than the resistance for steady currents; for a copper wire 2 centimetres diameter, and frequency of musical note 320 per second, the ohmic resistance is 68 per cent. greater than the resistance for steady currents; and so on.

Another important development from this theory is, that there is much less importance in the conductivity of the metal for telephonic work through extreme distances, than for ordinary electric work. The formula and figures showing the kind of relations between ohmic effective resistances for different speeds are, as we have seen, a little complicated; but I will only say

that ultimately it is the square root of the resistance that we have to deal with instead of the simple resistance. Quadruple resistance is only twice as bad, speaking roughly, when the frequency is so great as to cause the effect of the ohmic resistance to be very much greater than the resistance for steady currents. The moral of this is, not that you may choose wire of bad conductivity; on the contrary, take the best conductivity you can get, whether for telephones or for electric light conductors, but shape the conductors so that the ohmic resistance shall not be too much augmented by the unequal distribution of the current.

In respect to electro-magnetic theory. We have a very fine analogy with viscous fluid motion, which has been obvious, more or less, from the time the known laws of electro-magnetic induction were put into formulæ in the beautiful manner in which Maxwell put them,—we have a very fine analogy, I say, with the diffusion of laminar motion into a viscous fluid, and its analogue in the diffusion of heat by conduction through a solid, first pointed out by Professor Stokes. The actions concerned in the distribution of alternating electric current through a conductor such as copper, and the distribution of the motion of water in a viscous fluid disturbed by periodical tangential motions of its surface, follow identically the same law. Mr. Heaviside, referring to this, has well said that this analogy is very useful, because we can see the motions in a viscous fluid, and understand them, and picture them to our minds, while it is much more difficult to fancy we see the distribution of electric current in a wire. Take now definitively, this analogy for the distribution of electric current in a round copper wire through which alternate currents of electricity are sent. Take a viscous fluid in a tube, in place of the conductor: move the tube to and fro with a regular alternating motion—a simple harmonic motion. In order that we may fulfil at all approximately what I am speaking of, the length of the tube must be very great in comparison with the diameter, and the place in which we consider the motion of the fluid must be at a distance of many diameters from the ends, which we may suppose to be closed by frictionless pistons,

limiting the fluid at its two ends. In the first place, if the fluid were not viscous—if it were perfectly liquid—you might move the tube to and fro, but the fluid inside of it would remain at rest. Water, however, would move; oil would move; the more viscous the fluid is, the more liable it would be to experience motion in that way. Now there is a perfect analogy between the alternating motion of the fluid transmitted inwards from the surface, and the distribution of the electric current in a wire through which the effect of the alternating current machine is being conveyed.

Another very interesting analogy in which exactly the same law holds, is the change of temperature of a conducting solid, due to variations of external temperature. Imagine a column of rock or stone or metal, and, instead of moving our tube to and fro longitudinally, let the atmosphere surrounding our column be periodically varied in temperature: the law of the inwards progress of changes of temperature, the law of the maximums and minimums and zeros of temperature, is identical with the law of the corresponding features of electric currents and of fluid motion. In each case we have a propagation inwards, with diminishing amplitude. In each case the rate of diminution of amplitude corresponds to the retardation of phase according to exactly the same law. I need not attempt at this time to state the law—mathematicians know it perfectly well. Now take another case. Here the thermal analogy absolutely fails us, but the fluid motion analogy still holds. Take a tube of fluid and give it an alternating motion—a periodically varying motion round its axis which gives a tangential drag to the fluid in the inside. Now you can all see that the inwards penetration of the tangential drag, if the alternations of the motion be very quick, will follow the same law for the to and fro motion of the cylinder and for the rotatory motion of the cylinder. The question is this, Does the variation penetrate sensibly to a large distance in, from the outside or not? If, for example, it penetrates in only the one-hundredth of the radius, then it is obvious that we shall have sensibly the same law of penetration inwards for the disturbance, whether for the case of the rotatory motion of the

cylinder round its axis, or of longitudinally to and fro motion. Exactly the same thing holds with reference to electro-magnetic induction. The one case of electro-magnetic induction that I mentioned first is the most important, being the telegraph and telephone case; but another very interesting case, and not at all without practical importance, is the penetration of induced currents into a copper or other metallic core within a solenoid. Take a common helix or solenoid: send an alternating current through its coil—you know what it does. It produces magnetic force, with lines of force parallel to the axis, in the interior of the solenoid. But alternating magnetic force, through the copper, induces electric currents in circles perpendicular to the direction of the force. Thus we have currents induced, as you all very well know, in a solid metal core of a solenoid. A metallic core other than iron, is a subject for investigation of an exceedingly easy kind. The Fourier-Bessel functions come in here just as they do in the other cases in which we are concerned with circular cylinders. If we have, instead of copper, an iron core, we must take into account its inductive magnetisation. This presents no *mathematical* difficulty if we suppose the magnetic susceptibility constant; and the same law of amplitudes and phasal retardation holds as for copper or other non-magnetic metal. The difficulties, both experimental and mathematical, to take into account, are the enormous differences of the inductive quality of iron with different degrees of magnetisation, and with reversals of magnetisation; and the great complications of the inductive effects on account of the "magnetic friction" in the iron, introduce corresponding complications in the theory of the induced currents, and they are complications of a kind that are very formidable.

Now I can only just go on to say two or three words about an extension of that viscous fluid theory that allows us to take into account all that goes on both in air and in metal, and in different metals, whether in contact with one another or separated by air. For illustration, consider our two simple cases—parallel wires with alternating currents through them, and the cylinder rotated with a periodic motion of rotation alternately in opposite direc-

tions. The analogy is simply this: To represent different metals, densities of fluid in simple proportion to the electric conductivities must be taken; the viscosity must be the same in all. The representative of an insulator in this analogy is a massless fluid. By "massless" I mean devoid of inertia—perhaps I ought to say an "inertialess" fluid, because people attach other ideas to "mass" sometimes than "inertia," but in the strictest dynamical language "mass" is taken as the measure of inertia. An inertialess viscous fluid must take the place of air or other non-conductor; a viscous fluid of a certain density, but the same degree of viscosity, must take the place of lead. A fluid of twelve times the density of lead would take the place of copper, the conductivity of copper being, say, twelve times the conductivity of lead.

Time does not allow me to pursue the subject further in the way of illustration at present, but I must return to the second case later on, because I am going to speak of iron and rotation.

Now, with reference to the electrostatic effect, the hopeless—I must not say "hopeless:" that is too large a word; we are never without hope in science—I was going to use another word, "despair"—well, I feel it desperately difficult; I feel the probability of my seeing the solution of it *is* hopeless. To merely introduce into the analogy electrostatic effect is very simple. Simply imagine an interface between the two fluids, and give it such stiffness against change of shape as is required to cause it to fulfil the conditions which electrostatic knowledge and our knowledge of the laws of electric and electro-magnetic influence, dictate to us. I say, put in at the interface the requisite normal force, and you can extend the analogy to include the complete problem of the submarine cable, in which electro-magnetic and electrostatic induction are both taken into account. But it is only by putting in, and in an arbitrary manner, a force at the surface to fulfil the requisite conditions, that we can complete the analogy.

The analogy I have just sketched cannot be considered as being in any respect a physical analogy. In it the analogue to

electric current is not velocity of the fluid; it is not the molecular rotation of the fluid; it is a quality derived from the simple motion of the fluid, mathematically by the operation known as the "laplacian" operation—the laplacian of $(u,v,w)$ is the electric current. Another way of putting it is the rotation of the rotation of $(u,v,w)$, or the spin-flow of the spin-flow of $(u,v,w)$. The word "curl" was introduced by Clifford and Maxwell, and has, I grieve to say, been adopted by Mr. Oliver Heaviside. It is the curl of the curl. I object to "curl," because "rotation" or "spin" is good, and "curl" is bad; I object to it on that ground, but not only on that ground. I object to it also because it is connected with a kind of mathematical symbolism which seems to me not desirable and not instructive, and, above all, not convenient for any practical use in mathematics. This "laplacian" is too difficult a subject to explain, but I would by a case try to illustrate it. Take in the viscous fluid analogue what corresponds to the steady current in a wire. Think of the tube with viscous fluid and pistons as before. At one end of the tube press a piston in with a uniform motion, continued long enough to cause the fluid throughout the tube to come to a state of steady motion. In the neighbourhood of the piston the motion is disturbed by the rigidity of the piston; but go to a distance of ten or twenty diameters from the piston, and the motion of the fluid takes a perfectly regular character. [*Illustrating on blackboard as Fig.* 1]. Suppose *that* to be the inner surface of the tube. This dotted line represents a portion of the liquid which at one time is plane. A little later, while the fluid in contact with the containing surface remains unmoved, in the doctrine of viscous fluid, as given by Stokes—there is absolutely no slip at the containing surface—this portion of the fluid which was plane becomes the paraboloid of revolution, which you see shown in axial section in the diagram. The velocity of the fluid is nothing at the bounding surface, and it is a maximum at the centre. Well, we have two functions derivable from the consideration of that distribution of velocity. The first is the rate of shearing of the fluid; the second is the rate of change per

unit-change of distance from the axis of the rate of shearing.* The rate of shearing represented graphically is equal to the tangent of the inclination (T P N) of this curve to the transverse surface, the tangent of the inclination of the curve being the angle which is represented by the letter $i$. Now the rate of change from point to point of the rate of shearing is the analogue to the strength of the current, and that is uniform. So in this analogy of a viscous fluid forced through a tube, we have not the fluid velocity equal to the electric current, but something else, quite intelligible; and the reason for it, in our analogy, is clear enough. But there is something interesting, perhaps, in this idea—that we have a super-subtle mathematical definition of electric current which is not fluid velocity. Well, now, perhaps

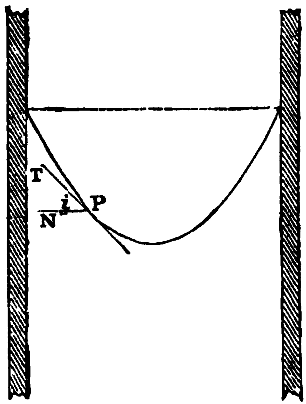


FIG. 1.

someone will say, "Had not we better get an analogy in which a "fluid velocity is equal to the velocity of the electric flow?" Well,

* Suppose the parabola in the drawing to represent the fluid which lay along the dotted line a unit of time earlier. The distance of P from the dotted line is equal to the velocity of the fluid ($v$) at the distance ($r$) of P from the axis. We have $u = c\,(a^2 - r^2)$, where $a$ denotes the radius of the tube and $c$ the fluid velocity along its axis. We have $\frac{-du}{dr} = 2\,c\,r$, which is the rate of shearing; and $\frac{-d^2 u}{dr^2} = 2\,c$, which is our representative of the electric-current density. The whole strength of the electric current is $2\,\pi\,c\,a^2$.

I do not say whether we had better do so or had better not, but we do not, otherwise than in the way I have defined, get the working analogy; and there is an advantage in this analogy. It gives us a motion of which the rotation is the magnetic force. Mr. Heaviside rather overlooks that. He objects to Maxwell's vector-potential. I do not agree with him in his objection. If he had confined the objection to the "vector" and the "potential" ("vector" wholly bad, and "potential" bad in connection with the present subject), I would heartily have agreed with him, because I think it is an unhappily chosen name; but Maxwell's use of the thing (which he unhappily calls "vector-potential") is most happy and most instructive, as it seems to me. Maxwell does not translate this into realities of motion, but he puts down in his formulæ, as the foundation from which one step leads to magnetic force and the next step to electric current, something which, translated into realities of motion, gives us a motion of which the rotation is the magnetic force; and here it seems to me that if we are ever to have a real theory it must be founded upon this view. The hand of the clock warns me time is going on so rapidly that we must leave this analogy absolutely unfinished. Perhaps it is well to be obliged to leave it now, because the more we look at it the less we like it, if we wish to see and to like a true mechanical explanation of electro-magnetism. The work is done in the wrong place. In the dense liquid, work is done and heat generated in proportion to the square of the rate of shearing. In the electric analogue, the work is done and heat generated uniformly throughout the conductor. We have work done and heat generated in the viscous massless fluid taking the place of the dielectric in our fluid analogue. We must discredit that absolutely; but the reason for judging the analogy worth so much notice as even it has had to-night, is that it is a perfect mathematical working analogy, and an exceedingly useful and instructive kind of analogy, and a very potent one to help us in guessing out, and in thinking out, and estimating results in practical problems of electro-magnetic induction in dynamos and in alternate-current machines, and in telephones and in electric instruments of great varieties of shape and mutual relations.

But now there is another line of thought in connection with this subject, and that is the elastic solid idea. Will you allow me to read a very short statement which was published in the *Cambridge and Dublin Mathematical Journal* for the year 1847? It is dated Glasgow University, November 28th, 1846. It was written after I had been twenty-eight days at work in my professorship, and it is as follows:—"Mr. Faraday, in the 11th "series of his 'Experimental Researches on Electricity,' has set "forth a theory of electrostatical induction which suggests the "idea that there may be a problem in the theory of elastic solids "corresponding to every problem connected with the distribution "of electricity on conductors or with the forces of attraction and "repulsion exercised by electrified bodies. The clue to a similar "representation of magnetic and galvanic forces is afforded by "Mr. Faraday's recent discovery of the affection with reference to "polarised light, of transparent solids subjected to magnetic or "electro-magnetic forces. I have thus been led to find three "distinct particular solutions of the equations of equilibrium of "an elastic solid, of which one expresses a state of distortion, such "that the absolute displacement of a particle in any part of the "solid represents the resultant attraction at this point produced "by an electrified body. Another gives a state of the solid in "which each element has a certain resultant angular displacement, "representing in magnitude and direction the force at this point "produced by a magnetic body; and the third represents in a "similar manner the forces produced by any portion of a galvanic "wire; the directions of the force in the latter cases being given "by the axes of the resultant rotations impressed upon the "elements of the solid." Then come the mathematics, in three pages, and then comes the last sentence: "I should exceed my "present limits were I to enter into a special examination of the "states of a solid body representing various problems in elec- "tricity, magnetism, and galvanism, which must, therefore, be "reserved for a future paper." As to this last sentence, I can say now, what I said forty-two years ago—"*must be reserved for a* "*future paper!*" I may add that I have been considering the subject for forty-two years—night and day for forty-two years.

I do not mean all of every day and all of every night; I do not mean some of each day and some of each night; but the subject has been on my mind all these years. I have been trying, many days and many nights, to find an explanation, but have not found it.

Let there be an elastic solid body of exceedingly small density, and let there be a tubular portion of it porous, but with the same aggregate rigidity as that of the continuous elastic matter around it. Let the pores be filled with a dense viscous fluid, and let this fluid be forced, by aid of a piston or otherwise, to move through the tube. The pull of the fluid upon the porous solid will produce static rotational displacement exactly proportional to the continued rotatory motion which we had in the case of the viscous fluid. Some of the most interesting practical problems of electro-magnetic induction can be dynamically realised, as it were, in model, by following out this idea; in fact, if we had nothing but electricity and ether, the thing would be done. If it were not for the gross ponderable matter that we are forced to consider, I should be perfectly satisfied with the problem of electro-magnetic induction, by taking the electricity as a viscous fluid, and ether an elastic solid, porous in some places, and continuous or non-porous elsewhere.

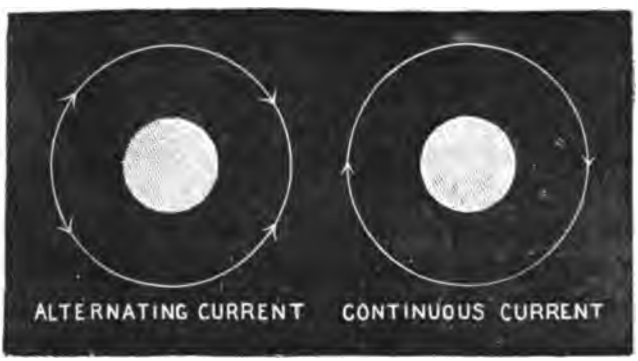


FIG. 3. FIG. 2.

Now, if you will pardon me, though it is very late for introducing another topic on which to speak—I shall confine myself to one, and that is magnetism. I must return to the rotational case. Imagine this (Fig. 2 or Fig. 3) to be the section of an

ordinary helix or solenoid with a solid copper core. Imagine a continuous electric current (Fig. 2) or an alternating electric current (Fig. 3) of electricity sent through it. Whatever the current of electricity may be, I believe *this* is a reality: *it does pull the ether round* within the solenoid. I do not think this is a dream of electro-magnetic theory; I believe it to be a reality. Whatever ether is, we move through it—the earth moves through it. Astronomers and opticians do not cry out and make their lives miserable because of the aberration of light. Fresnel and Professor Stokes have done all that man, up to the 9th of January, 1889, has been able to do to explain the dynamics of the aberration of light. It may be not beyond man's range to complete the solution—how the earth can tear through this elastic solid ether and yet the waves of light be propagated through it as they are. The aberration of light is still an absolute mystery. Yet people who deal with optics and astronomy are not expected to be miserable for life because they have that difficulty ever before them. Well, are we to be absolutely unhappy because, while we see a mobile wire caused, in virtue of an electric current through it, to move by electro-magnetic force, we cannot see any possibility of explaining how a medium capable of the "magnetic stress" can allow it to move? After all, great as the mystery there is, there is a mystery greater than that. The act of free-will with reference to the laws of matter is a greater mystery than anything that has ever been suggested or imagined in the dynamics of ether, and electro-magnetism, and light. Somehow or other, however it is, the ether is pulled round, the ether does get a turning motion in the interior of a solenoid; somehow or other it does give a turning motion to ether within our supposed copper core; and somehow or other there is a motion following these very laws we have been speaking of, and illustrated by the viscous fluid analogy.

But now for the iron. And now, instead of an alternating current through the helix, take a constant current through it. What can it do? One thing or the other it does: either a constant current through this helix drags the ether round and

round inside, or it drags it round to a certain angle proportionate to the strength of the electric current, and brings it to static equilibrium so turned. It does either one or other of those things. Now, how on earth can iron differ, in the principle of the interfacial law, from copper? Our interfacial law depending on equal viscosities is quite clear, but when you introduce iron you introduce an interfacial difference depending on rotation, without anything that could possibly be a cause of any viscous action, or a cause of any elastic action. Elastic action (unless of compression or rarefaction, and these are not of our present subject) requires distortion. You have no elasticity of an incompressible elastic solid without distortion. Now if, by applying a tangential force all round the space within a cylinder, you keep turning the circumference, you will keep turning the contents. Ultimately the whole fluid within will go round with the same angular velocity as the circumference of the viscous fluid within it. Thus our viscous fluid analogue works out perfectly well for the magnetic force within a solenoid having any non-magnetic material within it, and illustrates the fact that it is the same for conducting and non-conducting matter. But with iron the case is something quite different. Our viscous fluid analogue is called on to give us a greater permanent angular velocity, or a greater static rotational displacement, in the space occupied by iron in the magnetic analogue, than in this surrounding space! Thus the primary phenomenon of the magnetisation of a bar of iron within a helix, absolutely leaves us behind, cuts the ground from under us, both as to our viscous fluid analogy and our elastic solid analogy. If it is to be a fluid going round and round, we must have an action between the portions of fluid on the two sides of the interface, depending, not on distortion, but on rotation. Or if we take our elastic solid analogue, we must have static equilibrium of the elastic cylinder, with the inner part turned through a greater angle than the rotational part of the displacement of the surrounding matter. An irrotational circular displacement subtracted from this, procures fulfilment of the *no slip* condition at the interface. The distortion due to this irrotational displacement gives rise to a torque, tending to turn

the matter within the interface! Hence we must have an arrangement of matter in which a constant torque produces a constant angular displacement in a body, and does not produce continued rotation. The only thing that can do that is an inherent rotation existing in the molecules of matter. This seems the only thing that can do it, and this *can* do it certainly. But consider this—that the gyrostat shows us the thing done; and I will just conclude, if you will allow me, by a simple gyrostatic experiment—a very well known old gyrostatic experiment—and I want to accentuate the application of it.

I am going to show by this illustration, with reference to the idea of a medium, a medium which has the properties of an incompressible fluid, and no rigidity except what is given to it gyrostatically. Here is, so to speak, a molecular skeleton that can give us such a fluid—a set of rigid squares with their neighbouring corners joined by endless flexible inextensible threads, running frictionlessly through holes in the corners, or round pulleys mounted in the corners (Fig. 4). Here is a model

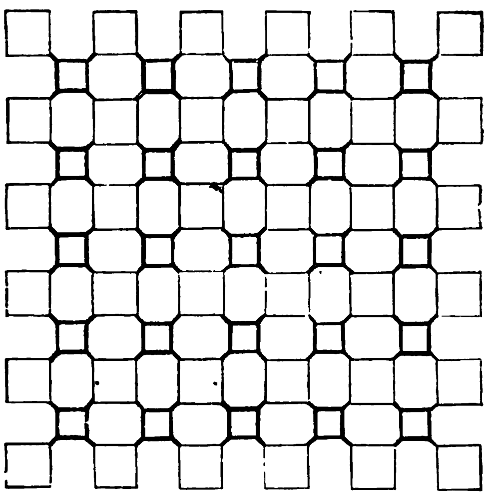

Fig. 4.

thus constructed—sixteen rigid squares and nine endless cord segments connecting the corners in this pattern, forming a kind of web. Now, if we take an ordinary cloth web, and pull it in

different directions: in the direction of the warp and the direction of the woof, you cannot stretch it; but at 45 degrees from the warp and woof you can stretch it very freely. We all understand that. You know how the surgeons take advantage of it in their diagonally cut bandages. Now here is a web which is equally easily stretchable in all directions, and yet which is of constant area—a constant area for infinitesimal displacements, not a constant area for very great displacements. The circumference of each rigid and of each flexible square is given. Well, now, if you infinitesimally alter the square into a not-square rectangle, or into a rhombus, the area remains sensibly unchanged. The first change of the area is a diminution in whatever direction you stretch it; but that is proportional to the square of the strain, so that you may say, in language of infinitesimals, the area is unchanged. The constancy of the periphery, then, of each of these figures gives rise to and entails the condition of an approximate constancy of the area. Here, then, we have in this skeleton a two-dimensional working model of a medium which is unchangeable

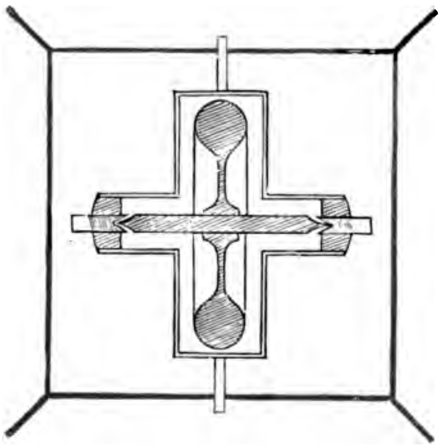

Fig. 5.

in area, but is freely extensible in any direction, provided you allow it to shrink proportionately in the perpendicular direction. Well, now, let us put a gyrostat into each of those squares (Fig. 5), and you have all that is wanted to fulfil the strange—almost inconceivable—condition for a dynamical model of electro-

magnetic induction in iron which I have put before you. I will just make an experiment illustrating that, if it is not occupying too much time. [*Sir William Thomson then spun the gyrostat.*] I turn azimuthally the square frame by which I hold it—first in one direction, and the red end of the bearing of the axle of the fly-wheel turns up; I turn the other way, and up comes the blue end. The gyrostat is mounted in a square frame, as you see, which I hold in my hand. The rigid case bearing the axle of the fly-wheel is, as you see, free to turn round the axis of these trunnions, mounted horizontally on bearings in opposite sides of the square frame which I hold in my hand. The axis of these trunnions is perpendicular to the axis of the fly-wheel. I shall walk round and round to right, to keep the red side up; I walk round to the left, and it keeps the blue side up. It is rather a curious thing. There are three little objects on a tray, as it were. Imagine this to be a butler's tray, with wine-glasses on it represented by these india-rubber corks. As long as I turn ever so little to my left all goes well; if I go straight forward, it is doubtful; but if I turn by an infinitesimal angle to my right, over it goes and everything falls off it.

Look, now, at the gyrostat resting in this position on its trunnions, the axes of the trunnions and of the fly-wheel being both at present horizontal. The outer square frame seems immovable in azimuth. When I apply a couple tending to move it in azimuth it does not move. It does not move in azimuth till the gyrostat turns round its trunnion axis and brings its fly-wheel axis to be perpendicular to the plane in which I am trying to turn the square frame. And I must apply a couple whose time-integral is equal to double the moment of momentum of the fly-wheel, before I can get the gyrostat from the position with the blue end up, to the position with the red end up. Before I succeed in turning the square frame even a degree in azimuth I must have applied a torque whose time-integral is equal to twice the moment of the momentum of the fly-wheel.

This closed brass case, with a rapidly rotating fly-wheel mounted on bearings inside it, is called a gyrostat because in virtue of rotation it stands, however you place it, with any of its

edges resting on a hard, smooth table. You see, place it as I will, it cannot fall. If I place it with its centre of gravity above the supporting point, it stands at rest. With its centre of gravity *not* vertically over the bearing point, it goes round in azimuth, *but it does not fall.*

Now imagine mounted in each one of the rigid squares of this web a gyrostat exactly as this one is in the square frame which I hold in my hand. If the fly-wheel speed be great enough, each of those rigid squares is practically immovable in azimuth. I do not say it is immovable, but I say you may make it practically immovable by making the velocity of the fly-wheel sufficiently great.

Thus we have a skeleton model of a special elastic solid with a structure essentially involving a gyrostatic contribution to rigidity. Now do not imagine that a structure of this kind, gross as it is, is necessarily uninstructive. Look at the structures of living things; think of all we have to explain in electricity and magnetism; and allow, at least, that there must be some kind of structure in the ultimate molecules of conductors, non-conductors, magnetic bodies, and non-magnetic bodies, by which their wonderful properties now known to us, but not explained, are to be explained. We cannot suppose all dead matter to be without form and void, and without any structure; its molecules must have some shape; they must have some relation to one another.

So that I do not admit that it is merely playing at theory, but it is helping our minds to think of possibilities, if by a model, however rough and unpractical, we show that a structure can be produced which is an incompressible frictionless liquid when no gyrostatic arrangement is in it, and which acquires a peculiar rotational elasticity or rigidity as the effect of introducing the gyrostats into these squares. Imagine a corresponding model in three dimensions, with rigid cubes instead of the rigid squares which you see in the model before you. Instead of the endless flexible cords which you see, you may imagine elastic threads stretched between neighbouring corners of the cubes. In each cube mount three gyrostats, with their trunnion axes perpendicular to the three pairs of its faces. The gyrostatic domination

thus provided, causes the cubes to be practically immovable in rotation, but leaves them perfectly free to take translatory motion. There you have a body, then, that you could not distinguish from an ordinary elastic solid in respect to any irrotational distortion, or in respect to translational motion of the whole, but which if you try to turn it, will not absolutely resist, but will only admit of turning by stretchings of the connecting elastic bands. It will not be immovable in respect of the turning, but it will be balanced by a constant couple with a constant degree of rotatory displacement. Thus upon this solid, the effect of a constant couple is not to produce continued rotation, but to produce and balance a constant displacement; and that balance might last for any time, however long, if the rotational moment of momentum of the fly-wheels is but great enough.

Now, lastly, I should just explain briefly that the rotational contribution to rigidity of ether in iron must be enormously less than in copper or in air. The total effective rigidity of the ether due to elastic action and gyrostatic effect in all non-magnetic bodies is the same. In iron there is less gyrostatic contribution, with equal elastic contribution, to the total rigidity. These conditions fulfil exactly what we want for the relation of ether between air and iron inside the helix of an electro-magnet. But, alas! we are only led on to inscrutable difficulties. How much does our elastic solid go towards the explanation, when in the very fundamental fact of the mutual motions by which electro-magnetic forces are made manifest to us, we have a force as of a strained solid between the bodies (magnets or wires) whose motions revealed to Œrsted and Ampère the existence of electro-magnetic force? Why is it that those strains do not simply balance themselves in the solid? How can there be a solid capable of giving rise to that wonderful condition which we have in the air between the poles of an electro-magnet—for instance, such that a piece of copper will fall down through it at the rate of, perhaps, a quarter of a centimetre per second? Look on the subject as engineers, and think of the "strength of materials" wanted for ether in air, with the molecules of the air itself tearing through it in all directions at speeds averaging 500 metres

per second, or more or less according to temperature. Think of the forces, amounting to 110 kilogrammes weight per square centimetre, with which two bars of iron magnetised to 1,700 C.G.S., with faces separated by a thin space of air, and with Ewing's 46,000 C.G.S. of magnetic force in the air around the bars, are urged towards one another. How can it be that these prodigious forces are developed in ether, an elastic solid, and yet ponderable bodies be perfectly free to move through that solid? Now I simply say, all that has been done to think out this subject merely gives us a dynamical theory on one part of it. I have absolutely—not ignored, because I have spoken of it two or three times—but I have left out in the cold, the electrostatic part, the thing we knew first. Our first love was electrostatics. That is absolutely left out in the cold; we do not touch it. We do not get near to explaining the mutual force between two electrified bodies, in any of these illustrations or attempted explanations; we do not even get near the mutual attraction between the iron of an electro-magnet, or the steel of a permanent magnet, and its armature or keeper; we do not get near to explaining the possibility of the motions of the bodies that demonstrate the forces. We only try to explain for a quiescent system of conductors and insulators, the variable distributions of electric currents which from mathematical theory and experimental observation we know to exist.

And here, I am afraid, I must end by saying that the difficulties are so great in the way of forming anything like a comprehensive theory, that we cannot even imagine a finger-post pointing to a way that can lead us towards the explanation. That is not putting it too strongly. I only say we cannot now imagine it. But this time next year,—this time ten years,—this time one hundred years, probably,—it will be just as easy, as we think it is to understand that glass of water, which seems now so plain and simple. I cannot doubt but that these things, which now seem to us so mysterious, will be no mysteries at all; that the scales will fall from our eyes; that we shall learn to look on things in a different way—when that which is now a difficulty will be the only common-sense and intelligible way of looking at the subject.

I ask you to pardon me for leading you up to so impotent a conclusion as that we really know nothing below the surface of this grand subject which constitutes the province of the Institution of Electrical Engineers.

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## APPENDIX.

*Alternate Currents through a Straight Conductor of Round, or Rod of, Non-Magnetic Material.*

Let $\sigma$ denote the specific resistance in square centimetres per second (or the "specific resistance C.G.S.");

$a$ ,, the radius of the wire;

R (S) ,, the value of $\sigma\, l \div \pi\, a^2$ [or the resistance (in centimetres per second) of any length ($l$) of the wire, with steady current through it];

R (N) ,, the effective ohmic resistance of the same length ($l$), with alternate current of N periods per second through it.

$c$ (N) ,, the current-density at distance $r$ from the axis, and at time $t$.

C (N) ,, the current-density in the axis at time $t$.

We have $c\,(\mathrm{N}) = \mathrm{C}\,(\mathrm{N})\,(\mathrm{ber}\, q \cos.\, \theta - \mathrm{bei}\, q \sin.\, \theta)$,

where $q$ denotes $\left(2\,\pi \sqrt{\frac{2\,\mathrm{N}}{\sigma}}\right) r$;

,, $\theta$ ,, $(2\,\pi\,\mathrm{N})\, t$;

,, ber and bei denote two functions defined as follows:—

$$\mathrm{ber}\, q = 1 - \frac{q^4}{2^2\, 4^2} + \frac{q^8}{2^2\, 4^2\, 6^2\, 8^2} - \text{\&c.};$$

$$\mathrm{bei}\, q = \frac{q^2}{2^2} - \frac{q^6}{2^2\, 4^2\, 6^2} + \text{\&c.}$$

And if $p$ denote the value of $q$, with $r = a$, we have

$$\frac{\mathrm{R}\,(\mathrm{N})}{\mathrm{R}\,(\mathrm{S})} = \tfrac{1}{2}\, p\, \frac{\mathrm{ber}\, p\, \mathrm{bei}'\, p - \mathrm{bei}\, p\, \mathrm{ber}'\, p}{(\mathrm{ber}'\, p)^2 + (\mathrm{bei}'\, p)^2},$$

where the accents denote differential coefficients.

The following table of numerical results has been calculated for me by Mr. Magnus Maclean, official assistant to the Professor of Natural Philosophy in the University of Glasgow:—

| $q$. | ber $q$. | bei $q$. | ber′ $q$. | bei′ $q$. | $\dfrac{\text{bei}'\,\text{ber} - \text{ber}'\,\text{bei}}{\text{ber}'^2 + \text{bei}'^2}$. | $\frac{1}{2} q \times \dfrac{\text{bei}'\,\text{ber} - \text{ber}'\,\text{bei}}{\text{ber}'^2 + \text{bei}'^2}$. |
|---|---|---|---|---|---|---|
| 0·0 | 1·0000 | 0·0000 | 0·0000 | 0·0000 | ∞ | 1·0000 |
| 0·5 | ·999 | 0·0625 | − 0·0078 | 0·24992 | 4·0000 | 1·0000 |
| 1·0 | ·9844 | ·2496 | − ·062446 | ·499947 | 2·00014 | 1·0001 |
| 1·5 | ·9211 | ·5576 | − ·210011 | ·730251 | 1·3678 | 1·0258 |
| 2·0 | ·7517 | ·9723 | − ·4931 | ·9170 | 1·0805 | 1·0805 |
| 2·5 | ·3999 | 1·4571 | − ·9436 | ·9983 | ·9398 | 1·1747 |
| 3·0 | − ·2214 | 1·9376 | − 1·5698 | ·8805 | ·8787 | 1·3180 |
| 3·5 | − 1·1936 | 2·2833 | − 2·3361 | ·4853 | ·8526 | 1·4920 |
| 4·0 | − 2·5634 | 2·2927 | − 3·1347 | − ·4911 | ·8389 | 1·6778 |
| 4·5 | − 4·2991 | 1·6859 | − 3·7537 | − 2·0526 | ·8279 | 1·8628 |
| 5·0 | − 6·2301 | ·1160 | − 3·8442 | − 4·3538 | ·8172 | 2·0430 |
| 5·5 | − 7·9735 | − 2·7902 | − 2·9070 | − 7·3729 | ·8069 | 2·2190 |
| 6·0 | − 8·8584 | − 7·3348 | − ·2931 | − 10·8462 | ·7979 | 2·3937 |
| 8·0 | 20·9739 | − 35·0167 | 38·2944 | − 7·6615 | ·7739 | 3·0956 |
| 10·0 | 138·8405 | 56·3704 | 51·373 | 135·23 | ·7588 | 3·7940 |
| 15·0 | − 2969·79 | − 2952·33 | 86·648 | − 4089·2 | ·7431 | 5·5782 |
| 20·0 | 47583·7 | 11500·8 | 24325·1 | 41491·5 | ·7325 | 7·3250 |

For copper we have $\sigma = 1{,}610$ square centimetres per second. Hence, with $N = 80$ we find

$$q = 1{\cdot}98\, r = 2\, r.$$

Thus in respect to the ohmic resistance of the whole wire, we may for copper take the column headed $q$ as the diameter of the wire, and in respect to the distribution of the current through the wire (expressed by the ber bei formula above) we may take $q$ the diameter of the cylindric shell in which the current-density is to be calculated.

Professor G. Forbes: I have an extremely pleasing duty to perform in rising to propose—"That a hearty vote of thanks be "given to our President for the admirable, instructive, and highly "interesting Address which he has given to us, and that he be "asked to permit of its publication in the Journal of the Institu-"tion." I think everyone will agree with me when I say that we have this evening had one of those treats which it rarely comes to us to enjoy—to hear an exposition from a man of Sir William Thomson's experience and position, unfolding to us the result of the labours of his life, putting before us not only his personal recollections in connection with the advance of the science in which we are all so much interested, and the recollections of others who have worked with him, but also giving, as the result of this, advice to the younger members of the profession and others as to what subjects to pursue, and so forth. With the advancement and development of our science Sir William Thomson has been so intimately connected, that all of us feel that we are being more closely brought into contact with its past history when it is dwelt upon by him in such an Address as he has just given to us. But I think everyone will agree that what we have to thank him for mostly, is the manner in which he has inspired us with some of the enthusiasm which he himself possesses in investigating the deeper problems of electrical science, and the interest which he has made every one of us feel in trying to understand those brilliant analogies between physical phenomena which are tolerably comprehensible and electrical phenomena which are rather obscure to us. I believe Professor Forbes.

Professor Forbes.

I am the only member present who had the felicity of attending the three weeks' course of lectures which Sir William Thomson delivered at Baltimore on the undulatory theory of light, and I can give you some conception of the feeling of those who were fortunate enough to attend those lectures, when I say that for three weeks we had an uninterrupted exposition of a similar mode of treating the subject as we have had this evening; and it is a thing so rare that I feel everyone will have appreciated the treat which we have had in listening to the Address this evening. Among the audience this evening there are some very young members — some who have but little knowledge of mathematics—and there are also present among us the greatest mathematicians of the country, and yet I venture to say that there is not one, in the long range of abilities included by the audience, who has not learnt a great deal, and had a great deal of new light thrown upon the subject, in the course of this evening; and I am quite sure that those who have not hitherto appreciated the benefits of mathematical investigation into the theory of such phenomena will be able to see from this Address the way in which a mathematician can, by trying to get deep into the sources of things, be able eventually to actually apply such knowledge to practical phenomena. I remember very well in 1874, just after the appearance of Clerk-Maxwell's book, Professor Tait—who took, perhaps, a too sanguine view of the work—considered, in fact, that it settled the whole question as to what electricity was—Professor Tait said: "Some twenty-five years ago Sir William Thomson "said to me, 'If you will tell me what electricity is, I will tell "'you everything else;' and," continued Professor Tait, "Clerk-"Maxwell has given us this information; we must now apply to "Thomson for the rest." This evening we have been listening to Sir William Thomson in his attempts to explain, so far as it is possible at present, what electricity is, and what everything else is too. I feel quite sure that the vote of thanks which I am proposing will be most heartily agreed to, and that everyone in this room feels, as I do, that we have been listening to an Address of a quality which it is almost impossible to expect that we can ever listen to again. We must, however, also thank

Professor Forbes.

Sir William Thomson for having come this long journey from Glasgow especially to give us this Address, and to have taken the trouble and submitted to the fatigue of travelling all night for our edification. But it is not surprising that he should have taken this labour upon himself when during the hour he has occupied in delivering to us his Address he has been trying to compress the subject which he has been working out, almost continuously, night and day, for forty-two years. I beg to move the resolution which I have already read out.

Professor Ayrton.

Professor W. E. AYRTON: It is my good fortune—for I feel it to be a good fortune—to be asked to second this vote of thanks, which I am sure you will all accord with the greatest enthusiasm, to our new President. Within the last few months we have had the inestimable privilege of hearing our new President—of seeing our new President, I may say—in this room in two totally different capacities. A few months ago it was as an electrical engineer that he showed and described to us his most beautiful measuring instruments; to-night it is in a totally different capacity—as a theoretical electrician—that he has charmed us for so long a time; and it is due to the marvellous power that he possesses of combining theory with practice that he has been enabled to accomplish the work that he has already done, and which we all hope he will live for a long time to continue carrying on. We have in this country many eminent engineers; we have also many competent mathematicians; but I venture to think that we have only one who can combine, in the marvellous way that our President has combined, theory with practice; in fact, I may shortly say that we have but one Thomson. On the previous occasion, when as an electrical engineer he was addressing us, he pointed out how important it was to study theory. To-night as an electrican he commenced, with the diffidence that has marked all his work, by telling us that if we only first became competent engineers, the additional knowledge of electricity that was necessary could be acquired in a few months; and I am sure this thought will be a consolation for all engineers here to-night to sleep on—that if they only come into this room well armed with engineering knowledge, all

Professor Ayrton. this purely electrical science they will be able to grasp in a few months. I say it is a consolation, and I should be very sorry to remove that consolation from them. At the same time I cannot but fear that this previous knowledge of engineering, if it were sufficiently complete to satisfy Sir William, would be something so vast and so comprehensive in its character as to appal the ordinary practical man.

You all know what a deep debt of gratitude is owed by electrical engineers, and indeed I may say by the whole world, to the wonderful mathematical investigations that our President has to-night so lightly touched on—that investigation which made submarine telegraphy commercially possible. How Sir William's pianoforte wire has sounded the deep sea—the ocean I mean, of course, not the musical note—is quite familiar to you all; and how his ship's compass has guided vessels to their journey's end is well known to all the world. Well, now, I daresay, on some occasions ships have started from a port armed with his compass—have started on voyages of exploration—not perhaps quite knowing where they were going, or what they would find; and I think to-night he has supplied us with another compass—a mental compass—and started us all on a voyage of exploration; and because he cannot tell us what is the country that we shall arrive at, it may be fifty or a hundred years hence, it does not make the compass that he has provided us with—that mental compass—the less valuable, or diminish our unbounded feeling of gratitude for its gift.

The motion was carried by acclamation.

The President. The PRESIDENT: Gentlemen,—I thank you all very warmly for the kind manner in which you have received the motion which has been put before you. I thank the mover and seconder for the too kind appreciation which they have shown of my imperfect efforts to speak to you in proper terms this evening. I feel that your electing me to be the first President of "The Institution of "Electrical Engineers" is really to myself a very great personal kindness, because I believe that it was understood that, living as I do at so great a distance, I should not be able to do my duty

properly as President—to be at all the meetings, as I would like to be, or at the greater proportion of meetings, as I certainly ought to be, but only at a comparatively small number. I live 400 hundred miles away, and, as you know, have incessant occupation at that distance in Glasgow, so that it is practically impossible for me to be here at many of the meetings. I must therefore ask you to kindly excuse my inability to do myself the pleasure of being with you continuously, or even frequently, and I must trust to the kind consideration of the Vice-Presidents to take the place that I ought to occupy in the Presidential chair. I hope to be present at all the meetings of the Institution after the end of our Glasgow University session, and if possible I shall be at some before that time, but I am afraid it can be but at very few; and I must ask you to kindly excuse me, and to understand that it is simply owing to the necessity of the case. It was necessary, in fact, for me to let it be known before you did me the honour of electing me that this would be my case with reference to my duties as President of this Institution. I must say that, as regards the management of its business, I think none of us need have any concern at all when we know that it is in the hands of our excellent Secretary, Mr. Webb. He is always alive to the interests of the Institution, always attentive in carrying out its objects; and with the very great ability which he has shown we may feel that the management is in excellent hands. Then with the Council here, in whom you have all such perfect confidence, I think you will feel that the Institution loses nothing whatever by my absence; but I have deemed it right that I should make my apology, as you have been so kind to me this evening. The President.

Major-General C. E. WEBBER: Sir William and gentlemen,—On behalf of the members of the Institution, and in a few words, I have the great pleasure to give expression to our gratitude to Mr. Graves, the last President of the Society of Telegraph-Engineers and Electricians, for the splendid way in which he has filled that chair during the past year. All old members are sorry that he has done so to-night for the last time. Mr. Graves, we know, has distinguished himself long ago as the Engineer-in-Chief of the Post Office, and there Major-Gen. Webber.

Major-Gen. Webber. is nothing I can say which could add to his reputation; but during the past year he endeared himself to us by the careful and friendly way in which he has presided over us on every occasion when we have met together. I will not at this late hour occupy your time by any further remarks on the subject, but ask "that you will accord, as members of the Institution of "Electrical Engineers, your cordial thanks to Mr. Edward Graves "for the admirable manner in which he has discharged the duties "of President of the Society of Telegraph-Engineers and Elec-"tricians during the past year."

Mr. Spagnoletti. Mr. C. E. SPAGNOLETTI: I rise with very great pleasure to second the proposition of a vote of thanks to our retiring President, Mr. E. Graves. I have had the pleasure of working with him for many years, and therefore I can speak with some personal experience of his excellent business qualifications, of the very ready way with which he deals with matters and surmounts difficulties, and of the very courteous way in which he always performs his onerous duties. On behalf of the Council I am sure I may say that we are very much indebted to him for his constant attendance, and for the way in which he has conducted the affairs of the Society during his year of office; and I think I may also say the same on behalf of the members of the Institution.

The motion was carried unanimously.

Mr. Graves. Mr. E. GRAVES: When you did me the honour of electing me to occupy the chair which I have so recently vacated, I said it was a case of the wrong man in the wrong place, and I have not at all altered that opinion; in fact, if I had, the Address to which I have listened to-night would have corrected the error. But I told you also that I would do my best. I have striven to do so. We have had many meetings during the year—many meetings of the Society, many meetings of the Council, many meetings of Committees, especially on extraneous matters such as those connected with the Lord Mayor's Committee of the Paris Exhibition; I have attended them all, and now, at the end, I feel myself a sort of chief mourner for the Society of Telegraph-Engineers, and I resign to Sir William Thomson the much pleasanter task of acting

as godfather to the Institution of Electrical Engineers which takes its place. Mr. Graves.

The SECRETARY announced that the next meeting would take place on January 24th, when a paper would be read on "The "Insulation Resistance of Electric Light Installations," by Professor Andrew Jamieson, Member.

A ballot for new members took place, at which the following were elected :—

*Foreign Member :*

José Savall y Salvat.

*Members :*

John Marshall Gorham.
William Edwin Heys.
Captain R. Hippisley, R.E.
Professor Oliver Joseph Lodge, F.R.S.
Anthony Reckenzaun.
Professor Arthur Schuster, Ph.D., F.R.S.
Illius Augustus Timmis, M. Inst. C.E. & M.E.
James Wimshurst.

*Associates :*

Sydney William Baynes.
Samuel Joseph Coxeter.
William Fulton.
Charles Frederic Heywood.
Edmund Philip Jackson.
Alexander Login Lineff.
Lieutenant W. Luard, R.E.
David McNeill.
Edward Thomas Mercer.
Edwin Oldroyd.
Henry Francis Sharman-Crawford.
Thomas Thomas.
Charles Turner.
T. M. Winstanley Wallis.
John C. Waltham.
Michael Scaler Warton.
George Richard Webb.
Charles Aspull Wells.
Ebenezer Clarence Wood.
Reginald Frederick Yorke.

*Students :*

John F. Coote.
Wilmot Ernest Lane.
Max Salmony.
Josiah Sayers.
Ernest Waltham.

The meeting then adjourned.

The One Hundred and Eighty-fourth Ordinary General Meeting of the Institution was held at the Institution of Civil Engineers, 25, Great George Street, Westminster, on Thursday evening, January 24th, 1889—Professor W. E. AYRTON, F.R.S., Vice-President, in the Chair.

The minutes of the Annual General Meeting held on December 13th, 1888, and of the Ordinary General Meeting held on January 10th, 1889, were read and approved.

The names of new candidates for election into the Institution were announced and ordered to be suspended.

The following transfers were announced as having been approved by the Council:—

From the class of Associates to that of Members—

Léon Drugman.

From the class of Students to that of Associates—

F. W. Chapman.
J. Mountjoy Elliott.
W. Clark Fisher.
E. Loraine Heelis.

Donations to the Library were announced as having been received since the last meeting from the Astronomer Royal; the Electrical Society of Japan; Rev. G. T. Carruthers; P. A. Scratchley, Esq.; Messrs. Whittaker & Co. (Publishers); the Commander F. Salvatori, Foreign Member; Professor K. E. Zetzsche, Foreign Member; Wm. Ellis, Member; W. T. Hancock, Member; and E. March Webb, Member.

The SECRETARY announced that Mr. W. H. Preece, Past-President, had presented an electric clock for the use of the Library of the Institution.

A hearty vote of thanks was accorded to the donors for their presentations to the Library, and also to Mr. W. H. Preece for his useful and valuable gift.

The CHAIRMAN: In the unavoidable absence of Professor Jamieson in Glasgow, the Secretary will read the paper announced for this evening.

The SECRETARY then read the following paper:—

# THE INSULATION RESISTANCE OF ELECTRIC LIGHT INSTALLATIONS.

By Professor A. JAMIESON, F.R.S.E., M. Inst. C.E., Member.

*Insulation Resistance of Land Lines and Submarine Cables.*—In the early days of telegraph land lines and of submarine cables, *insulation resistance* was neither definitely specified for, nor accurately measured during the various processes of manufacture and of maintenance. A few failures, however (in some cases dearly bought), soon impressed electricians with the necessity for paying more attention to this important quality, and they were not long in demanding from contractors the minimum of insulation resistance which would enable their lines to be worked with success. Not only are careful insulation tests now made during the manufacture of insulators and the erection of the land lines under the British Postal Telegraph Department, but "daily tests" are taken of all the more important lines. The minimum insulation resistance which each insulator should have depends upon the length of the line. For a line of 100 miles long each insulator is expected to give a resistance of 500 megohms, whereas for a line of only 10 miles 50 megohms is considered sufficient. When a line has been erected and put into working order, it is usual to fix upon a minimum standard insulation resistance per mile. If the measured value at any time falls below this standard, then the line is considered faulty, and steps are taken to remove the fault. The Post Office standard is 200,000 ohms per mile. In the cases of subterranean lines and of submarine cables an even still more rigid and careful system of inspection and of testing is now carried out during the manufacture of the cores and cables. The results are invariably reduced to an insulation resistance per unit of length at a

standard temperature. The following extract from the specification of a successfully made and laid cable will serve to illustrate this:—"The insulation resistance per knot of the gutta-percha core "shall not be less, when tested at 75° Fah., than 300 megohms, "14 days after manufacture and after two minutes' electrification. "If the resistance of *any* portion of the gutta-percha (reduced to "75° Fah.) at any period of the manufacture fall below the above "specified limit, the cable to be rejected." Throughout the whole of the operations of coiling and uncoiling a cable into the shore and ship's tanks, during the laying, and subsequently during its existence, the insulation resistance is periodically measured and tabulated.

*Results of Neglecting Insulation Resistance in Electric Lighting.*—We have been induced thus to particularise the extreme care and attention which is now considered absolutely necessary in the case of land lines and of submarine cables, because the industry of electric lighting has just passed through a similar experience to that which these industries met with in their earlier days. When the industry of electric lighting was first beginning to take root in this country, electric light engineers pooh-poohed the idea of accurately measuring the insulation resistance of any part of their work, and they invariably turned a deaf ear to the timely warnings of telegraph-engineers and electricians. Experience has, however, taught them, as it did their elder brothers, that good uniform insulation resistance throughout an installation is of immense importance, and that it cannot be obtained without careful attention to details and a rigorous system of testing during manufacture and erection, and also after completion. If an installation is not well insulated throughout, leakage of the current takes place, and consequently waste of power and money; electro-chemical action sets in where damp or liquids are present, and the conductor is thereby eaten through or a short-circuit occurs, which for a time stops the whole of the lighting. Discredit thus not only falls upon the contractor, but others are frightened from employing the electric light, to the detriment of the industry as a whole.

*Necessity for Discussing a Standard of Insulation Resistance.*—The terms usually employed in regard to the insulation resistance of an electric light installation are, that it shall be "good" or "perfect," according to the fancy of the party purchasing the plant or of his adviser. These terms have, however, no specific meaning, and are therefore very variously interpreted. No doubt the Society has had this subject before them in committee when drawing up their set of rules for the "prevention of fire risks;" but the Institution as a whole has never had an opportunity of discussing it, and thereby of impressing its members and the public generally with its importance, and the necessity for observing some minimum standard of insulation resistance. The writer had his attention directed the other day to a communication read before the International Society of Electricians, Paris, in November last, by M. Picou, wherein that gentleman ably analysed this question, and proposed a rule based upon tests of short lengths of cotton-covered insulated wire. He has not had an opportunity, however, of learning the extent and the nature of the discussion upon M. Picou's paper, and he can only, therefore, place his rule before the members, along with a few other rules, and leave them to form their own opinions, guided by their practical experience.

*General Propositions.*—The following points will no doubt be readily admitted in regard to insulation resistance of electric lighting installations:—

1. That, owing to the comparatively low resistance of the conductor circuit of ordinary electric light installations on the parallel incandescent system, the minimum standard of insulation resistance does not require to be nearly so high per unit of length as in the case of submarine cables.
2. That it should be directly proportional to the electromotive force of the generator, or difference of potential between the conductor and earth or between the forward and return conductors.

3. That it should be inversely proportional to the total length of insulated conductor, or to the total current, or to the number of lamps in circuit.
4. That, whatever rules or standards of insulation resistance may be adopted, they should be applied, not only to all the conducting wires, but also to the dynamo, the lamps, and all the fittings, or, in other words, to the *whole* circuit.

*Rules.*—1882 *Set of Rules by Society of Telegraph-Engineers and Electricians.*—In May, 1882, in the first set of "Rules and "Regulations for the Prevention of Fire Risks arising from Electric "Lighting," issued by the Society of Telegraph-Engineers, no standard of insulation resistance or of minimum leakage is recommended. All that we find there on this head is: "The "difficulties that beset the electrical engineer are chiefly internal "and invisible, and they can only be effectually guarded against "by testing or probing with electric currents;" "All wires "used for indoor purposes should be efficiently insulated," &c.; "The insulation of dynamo coils and conductors should be "practically perfect;" "The value of frequently testing the "apparatus and circuits cannot be too strongly urged," &c.

Five years ago even, the writer is not aware that any published rule existed, or that any specification contained any definite rule for the insulation resistance of any part of any electrical installation.

*Admiralty Rule.*—About 1883 or 1884, however, the Admiralty officials requested that the insulation resistance of the dynamos supplied to them should withstand the test of stroking the free end of a conducting wire (the other end being connected to either pole) over any part of the framing without showing any signs of sparking when the machine was driven at its normal speed. The writer does not know whether they still insert this clause in their specifications, or whether they have modified or improved it; but at best it is only a rough-and-ready test, and gives no indication whatever as to the precise locality of a fault or its value in ohms.

*Workman's Tests.*—The only test applied by contractors when wiring an installation was, and often still is, that of simply inserting an ordinary lineman's detector or galvanometer, G, between a battery, B (of one or two cells), and the leads, with the other end or ends freed, as in Fig. 1. If the galvanometer

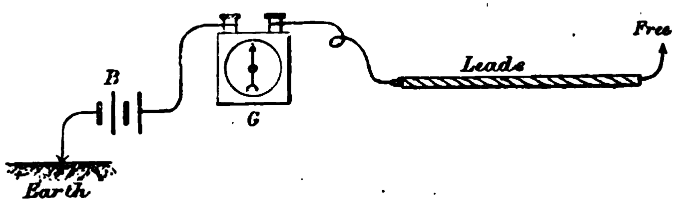


Fig. 1.

gives no deflection, or a very small one, then the section is passed as all right.

This is such an exceedingly simple and handy test for workmen that it can scarcely be improved upon, but it gives no precise indication whatever of the resistance in ohms, of the dielectric, or of the fittings. It simply informs the observer whether there is a bad fault or not. He can also find out by it whether the continuity of the conductor is complete, by earthing the further end; and he can also ascertain whether there is a short-circuit or

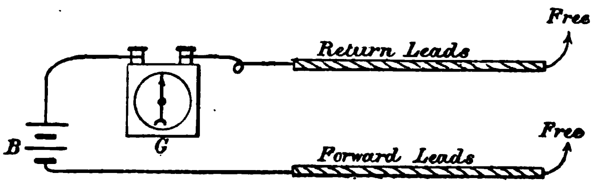


Fig. 2.

not between any two or more "forward" and "return" leads by connecting up his apparatus as shown in Fig. 2.

In 1884 Messrs. Wm. Denny & Bros.' electrician used a small portable magneto-electric machine and sensitive bell—the same as telephone companies employ—instead of the above-mentioned galvanometer and battery. If the bell rang, a fault existed, and its position had to be found out; if it did not, then all was

supposed to be right. He, however, in addition to this merely workman's test, applied a more exact test by Wheatstone bridge, and thus obtained the insulation resistance of the whole or of any desired portion of the circuit and the fittings in ohms.

*Professor Jamieson's Rule.*—In 1884, whilst testing the electric light wires and fittings of some ships, the writer first drew up his rule, which has been published in the last three editions of Munro and Jamieson's "Pocket-Book of Electrical Rules and "Tables." He had previously (since 1881) tested, by Wheatstone bridge, &c., the insulation resistance of a number of land and ship installations, and was rather surprised at the variety and lowness of the dielectric resistance, for his ideas of insulation resistance had been somewhat magnified by seven years' experience with submarine cables. At first, he could not get contractors to come up to his rule, but he was much gratified about this time by the enthusiasm and interest which Messrs. Wm. Denny & Bros.' electrician took in the matter, and by finding such good results in his tests of their ships' lighting leads, which results so far surpassed the demands of his rule that he decided to adhere to it. Since then, he has had much less difficulty, and when care and attention to cleanliness in jointing, to the fixing of wires and fittings, as well as in the manufacture of dynamos, are observed, he has frequently found that the insulation exceeded the demands of his rule, which is as follows:—"A careful insulation resistance "test of each circuit, and finally of the whole of the circuits "(including all switches and terminals, but not necessarily the "lamps) joined up together, should be taken by the method "described above,* and not passed unless the resistance is at least "equal to ·1 Ω per lamp for every volt employed. The insulation "resistance of the dynamo coils should be equally good."

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* See Munro and Jamieson's "Pocket-Book of Electrical Rules and Tables," p. 179, at foot. This rule applies only to the dynamos, leads, and fittings for incandescent lamps arranged on the single parallel system. Should the lamps be arranged in parallels, with two or three or more lamps in series in each parallel, they count as but one for each parallel.

Put into formula shape, the rule is—

$$R_I = k \frac{E}{N_L},$$

where $R_I$ = the total insulation resistance of the whole or any part of the lamp circuits or of the generator in ohms;

$k$ = a constant ($\cdot 1\ \Omega = \frac{1}{10}\ \Omega = 100{,}000\ \omega$) found from actual tests of several well-erected installations;

E = E.M.F. of dynamo or installation in volts;

$N_L$ = number of lamps (16-c.p.) on each circuit or on the whole circuit.

The insulation resistance is therefore here taken to be *directly* proportional to the normal E.M.F. of the dynamo, and *inversely* proportional to the number of 16-c.p. lamps in circuit. The difficulty of ascertaining the precise length of the mains, sub-mains, leads, and dynamo windings led the writer to substitute the number of lamps in circuit for what might at first sight appear to be the more exact term in the denominator, viz., the length of conducting wires employed. Although not stated in the above quotation of his rule, the writer usually applies the same test for the resistance or percentage leakage of current between the whole system of forward and return wires; also between the dynamo armature with field-magnet coils in circuit and the frame or body of the machine, as well as between the series and shunt-magnet coils if the dynamo is of the compound or series-shunt type.

The following example and table will make the application of this rule clearer:—

Suppose that we have an installation of 100 lamps of 16 c.p. each, and that the normal difference of potential between the terminals of the dynamo is 100 volts, then the insulation resistance should not be less than

$$\begin{aligned} R_I &= k \frac{E}{N_L} \\ &= 100{,}000\ \omega \frac{100\,v}{100\,L} \\ &= 100{,}000\ \omega = \cdot 1\ \Omega. \end{aligned}$$

*Table of Insulation Resistances required by Jamieson's Rule.*

*For* 16-*c.p. Lamp Circuits and Dynamos.*

| Number of Lamps in Circuit. | 50-Volt Lamps. | 65-Volt Lamps. | 80-Volt Lamps. | 100-Volt Lamps. | 110-Volt Lamps. |
|---|---|---|---|---|---|
| | Ohms. | Ohms. | Ohms. | Ohms. | Ohms. |
| 1 | 5,000,000 | 6,500,000 | 8,000,000 | 10,000,000 | 11,000,000 |
| 10 | 500,000 | 650,000 | 800,000 | 1,000,000 | 1,100,000 |
| 20 | 250,000 | 325,000 | 400,000 | 500,000 | 550,000 |
| 40 | 125,000 | 162,500 | 200,000 | 250,000 | 275,000 |
| 50 | 100,000 | 130,000 | 160,000 | 200,000 | 220,000 |
| 100 | 50,000 | 65,000 | 80,000 | 100,000 | 110,000 |
| 150 | 33,333 | 43,333 | 53,333 | 66,666 | 73,333 |
| 200 | 25,000 | 32,500 | 40,000 | 50,000 | 55,000 |
| 300 | 16,666 | 21,666 | 26,666 | 33,333 | 36,666 |
| 400 | 12,500 | 16,250 | 20,000 | 25,000 | 27,500 |
| 500 | 10,000 | 13,000 | 16,000 | 20,000 | 22,000 |
| 1,000 | 5,000 | 6,500 | 8,000 | 10,000 | 11,000 |

The leakage allowed by this rule in the case of 100-volt lamps, using ·64 ampere of current per lamp, is

$$C = \frac{E}{R} = \frac{100\ v}{10{,}000{,}000\ \omega} = \frac{1}{100{,}000}\text{ ampere of leakage}$$

for every lamp in circuit. Each lamp, however, requires ·64 ampere; consequently the leakage $= \frac{1}{64{,}000}$ part of the total current.

Or, the percentage leakage $= \frac{1}{640} = \cdot 0016\ {}^{\circ}/_{\circ}$.

Since by the above rule the dynamo requires to have the same insulation resistance as the lamp circuit, the insulation resistance of the *whole* circuit, including the dynamo, is half of that given by the formula and tables, and therefore the leakage is double, or $\frac{1}{32{,}000}$ part of the total current, or $\frac{1}{320}\ {}^{\circ}/_{\circ} = \cdot 0032\ {}^{\circ}/_{\circ}$.

*Phœnix Fire Office Rules.*—In May, 1888, in the thirteenth edition of the Phœnix Fire Office Rules, Mr. Heaphy recommends that "in any electric light installation in which the current is con-" tinuous, and has an electro-motive force of 200 volts or under, the " insulation resistance over the whole circuit inside any buildings " should never run below 10,000 ohms. . . . With currents of " 1,000 volts the insulation resistance should never drop below

"50,000 ohms, no matter how wet the weather may be. For "alternate currents the minimum insulation resistance should be "twice the above number of ohms respectively."

In November, 1888, in the fourteenth edition, Mr. Heaphy is more precise, and he has added a table based upon similar proportions to the writer's rule.* His Rule No. 27 reads as follows:—"In "any electric light installation in which the current is continu-"ous, and has an electro-motive force of 200 volts or under, the "insulation resistance over the whole installation should not be "below the following:—

| | | | |
|---|---|---|---|
| "Installations of | 25 lights | ... | 500,000 ohms. |
| " | 50 " | ... | 250,000 " |
| " | 100 " | ... | 125,000 " |
| " | 500 " | ... | 25,000 " |
| " | 1,000 " | ... | 12,500 " |

"When the lights are proportionate between the above numbers, "then the insulation resistance should be correspondingly pro-"portionate. The insulation resistance of separate circuits of the "installation should also be taken, and should be in accordance "with the above table. For alternate currents the minimum "insulation resistance should be twice the above number of ohms "respectively."

*Objections to Phœnix Rule.*—The following objections may be offered in regard to this last rule:—

1. It ties down a contractor to one and the same insulation resistance whether the E.M.F. of the installation lies *anywhere* below 200 volts. Surely a 50-volt installation requires but half the insulation resistance of a 100-volt one, and but one-fourth that of one with 200 volts?
2. It is somewhat doubtful (owing to a previous note to Rule No. 20) whether the words "over the whole installa-"tion" include the dynamo as well as all the wires, fittings, and lamps?

* It will be observed that the above table corresponds identically with the writer's rule for an E.M.F. of 125 volts; *e.g.*, take 100 lamps: then

$$R_I = k\frac{E}{N_L} = 100,000 \omega \frac{125\,v}{100\,L} = 125,000 \text{ ohms};$$

and so on in proportion to the number of lamps.—A. J.

3. No distinction is made between circuits with one, or two, or more lamps in each parallel.
4. Arc lamps of 2,000 c.p. or more in parallel, and large incandescent lamps of 500 c.p. or more, which require strong currents, come under the same general words, "any electric light installation in which the current "is continuous," just as much as small incandescent lamps of, say, 5 c.p., which require only a fraction of an ampere.

A question naturally arises here for discussion—Should not the insulation resistance in such extreme cases bear some definite proportion to the current?

*Westminster Fire Office Rule.*—In 1888 the Westminster Fire Office issued a set of rules for guiding their officers in regard to electric light installations, in which they demand that the insulation resistance shall in no case be less than 150,000,000 ohms per mile!!! Surely they have been blindly making extracts from a submarine cable specification! No contractor could be expected, under ordinary circumstances, to complete his work (including dynamo and fittings) to such a high standard.

*M. Picou's Rule.*—On November 7th, 1888, M. Picou communicated his rule to the International Society of Electricians, Paris.* Using the same symbols as in the writer's rule, it is—

$$R_I = k\frac{E}{C},$$

where $R_I$ = the total insulation resistance of the circuit in ohms;
$k$ = a constant (500) found by experiments with short lengths of wire covered with three layers of cotton wound in reverse layers and pressed between metal plates;
$E$ = maximum E.M.F. of the generator in volts;
$C$ = total current generated or passing through circuits in amperes;
$\frac{E}{C}$ = the resistance in ohms of the whole circuit when the installation is in full working order.

The differences between M. Picou's rule and the writer's lie in

* See *Electrical Review*, Nov. 23, 1888.

the different values given to the constant $k$, and in the substitution by M. Picou of the total current, C, for the total number of lamps, $N_L$, in the writer's rule. There is no doubt an advantage in thus substituting C for $N_L$, since the constant $k$ thereby expresses directly the number of times the insulation resistance is of the apparent working resistance, $\frac{E}{C}$. And its reciprocal, the proportion which the leakage current bears to the total current. For, let $c$ = the leakage current, and adopting the same symbols as above for the other terms, then

$$c = \frac{E}{R_I} = \frac{E}{k\frac{E}{C}} = \frac{C}{k},$$

$$\therefore \frac{1}{k} = \frac{c}{C} = \frac{\text{leakage current}}{\text{total current}}.$$

We, however, consider that M. Picou's constant, 500, is by far too low to ensure good and lasting work. By it, a circuit of 1,000 lamps of 16 c.p. each, requiring 100 volts and ·64 ampere each, the total insulation resistance of the circuit would only need to be 78·1 ohms, instead of 10,000 ohms by the author's rule (or 5,000 ohms if the dynamo be included).

*The Society of Telegraph-Engineers and Electricians' Last Rule.*—In April, 1888, this Society issued its second pamphlet of "Rules and Regulations for the Prevention of Fire Risks," and Rule 16 states that "the insulation of a system of distribution "should be such that the greatest leakage from any conductor to "earth (and in the case of parallel working from one conductor to "the other when all branches are switched on, but lamps, motors, "&c., removed) does not exceed $\frac{1}{5000}$ part of the total current "intended for the supply of said lamps, motors, &c.; the test being "made at the usual working electro-motive force." This rule includes all kinds and systems of electrical installations, and it is very neatly and concisely expressed. Some persons have, however, a difficulty in understanding how the $\frac{1}{5000}$ part of the total current is to be tested for or ascertained, and others do not see exactly how the test is to be applied to different parts of the circuits at the usual working E.M.F. *before* the installation is put into working order.

If the rule had also been put into the following form, with a table giving the insulation resistances required for a few of the more common electro-motive forces and currents (or number of lamps), it would no doubt have recommended itself more readily to those unaccustomed to deal with the form of expression used in the rule. Using the same symbols as before, we have—

$$\frac{1}{5{,}000}\ C = c = \frac{E}{R_I},$$

$$\therefore R_I = 5{,}000\ \frac{E}{C} = k\ \frac{E}{C},$$

which is the form of expression used before, and most easily applied. This gives a constant ten times as great as M. Picou's, and about one-tenth that of the writer's rule. Which of these constants is most in accordance with good practice?

*Table of Insulation Resistances required by the Society of Telegraph-Engineers and Electricians' Last Rule,**

*viz.:—The leakage of any part of an installation not to exceed $\frac{1}{5000}$ of total current supplying that part;*

*or,* $R_I = 5{,}000\ E/C$.

| Number of Amperes in Circuit. | For 50-Volt Lamps. | For 65-Volt Lamps. | For 80-Volt Lamps. | For 100-Volt Lamps. | For 110-Volt Lamps. |
|---|---|---|---|---|---|
| | Ohms. | Ohms. | Ohms. | Ohms. | Ohms. |
| 1 | 250,000 | 325,000 | 400,000 | 500,000 | 550,000 |
| 10 | 25,000 | 32,500 | 40,000 | 50,000 | 55,000 |
| 20 | 12,500 | 16,250 | 20,000 | 25,000 | 27,500 |
| 40 | 6,250 | 8,125 | 10,000 | 12,500 | 13,750 |
| 50 | 5,000 | 6,500 | 8,000 | 10,000 | 11,000 |
| 100 | 2,500 | 3,250 | 4,000 | 5,000 | 5,500 |
| 150 | 1,666 | 2,166 | 2,666 | 3,333 | 3,666 |
| 200 | 1,250 | 1,625 | 2,000 | 2,500 | 2,750 |
| 300 | 833 | 1,083 | 1,333 | 1,666 | 1,833 |
| 400 | 625 | 812 | 1,000 | 1,250 | 1,375 |
| 500 | 500 | 650 | 800 | 1,000 | 1,100 |
| 1,000 | 250 | 325 | 400 | 500 | 550 |

* If we include the dynamo as well as the whole of the lamps, motors, &c., will the insulation resistance of the *whole* installation be half of the above figures?

*Different Tests.*—The ordinary Wheatstone bridge test is the handiest and easiest method of ascertaining the actual resistance in ohms of the conductors as well as of the insulation resistance. Should the insulation, however, be too high for the battery power and the galvanometer, then the direct deflection substitution method may be adopted. These tests are so fully described in every book on testing, and are, besides, so well known, that there is no need to explain them here. The battery power need only be a few Leclanché cells (10 or 12 at most) if the galvanometer used is of the sensitive-mirror Thomson type.

When the installation has been completed, then the whole of the results may be obtained by means of ordinary ampere and volt meters.

First, join up the circuit as follows, in Fig. 3:—

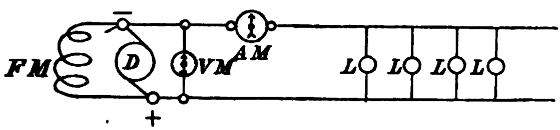


Fig. 3.

Work the installation at its normal power and observe the total current, C, by the ampere-meter, A M, and the difference of potential, E, by the voltmeter, V M. This gives us $\frac{E}{C}$.

Second, join up the voltmeter as in Fig. 4, with one side to earth, having previously ascertained its resistance, *r*, ohms. Also note, C, on the ampere-meter, and see that it is the same as before.

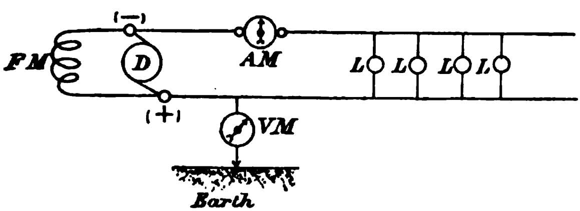


Fig. 4.

If any leakage to earth exists in the dynamo or in any part of the circuit, a deflection on the voltmeter, V M, will be produced

corresponding to the leakage current, $c$, passing through it, and is represented by, $e$, volts difference of potential between its terminals.

Then, $\frac{e}{r} = c$, the total leakage current.

But $\frac{C}{c} = k$, the actual constant or coefficient of the insulation resistance of the system.

Then, if this value is greater than $k$, our standard constant, the installation may be considered all right as far as insulation resistance is concerned. Of course this does not inform us if any leakage is taking place between the forward and the return system of wires, or between the shunt and the series windings of the dynamo.

To ascertain the former of these, disconnect all the lamps and insert the voltmeter first in the forward lead, and secondly in the return lead, as in Fig. 5, and note if we get any leakage

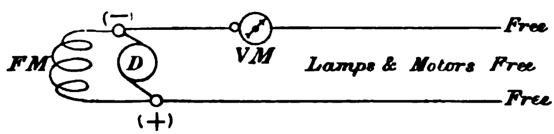


FIG. 5.

current, $c$, when the dynamo is run at its normal speed. In regard to leakage between the shunt and series windings of the dynamo magnets, recourse must be had to the Wheatstone bridge test.*

No doubt some of the members will suggest other practical tests, as well as give their experiences of how the insulation resistance of installations with which they happen to be intimately acquainted compare with the various rules herein mentioned, as well as how they stood the test of time, wear and tear of every-day work. What is chiefly wanted is to ascertain the minimum standard of insulation resistance compatible with good honest work and ordinary careful usage.

* For other tests when the installation is at work, see Munro & Jamieson's "Electrical Rules and Tables," 5th edition, pp. 215, 216.

P.S.—Since correcting the proof-sheets of this paper the writer thought that it might be interesting and instructive if he obtained the insulation resistance of an installation that had been working for some years, because members and others might say that some of the foregoing rules were all very well for *new* work, but were quite inapplicable to *old* work. He accordingly went to-day (January 16th, 1889) to Messrs. Graham's large East India offices and warehouses in Cathedral Street, Glasgow, with his testing apparatus, and took careful tests of the insulation resistance of the whole circuit, with the results shown in the following table:—

| | |
|---|---|
| Original date of fitting up installation ... ... ... | 1881–82 |
| Date on which most of the wiring was renewed ... | 1883 |
| Date of test (day wet—Glasgow weather) ... | Jan. 16th, 1889 |
| Total number of lamps in circuit of 16 c.p. each ... | 281 |
| E.M.F. at brushes of dynamo, in *volts* ... ... ... | 108 |
| Current total from dynamo for all lamps, in *amperes* ...(*about*) | 160 |
| Insulation resistance of dynamo armature and magnet coils alone, in *ohms* ... ... ... ... ... | 40,000 |
| Insulation resistance of the *whole* circuit, including the dynamo, *all* leads, switches, cut-outs, and lamps, in *ohms* ... ... ... ... ... ... | 21,400 |
| Insulation resistance to pass Jamieson's rule, including *dynamo* as well as the *whole* lamp circuit, in *ohms* | 19,217 |
| Insulation resistance to pass Society of Telegraph-Engineers and Electricians' rule (if their constant includes *dynamo* as well as the *whole* lamp circuit) in *ohms* ... ... ... ... ... ... ... | 3,375 |

This old installation (the oldest large incandescent light one in Scotland), which has been in full daily working order for over five years (since the wiring was renewed) when tested, without warning or any special preparation, and on a very damp day, gave a better insulation resistance than that demanded by the writer's rule, and over six times that required by the Society of Telegraph Engineers' rule (taking it in its broadest sense).

It is worth mentioning that the President, Sir William Thomson, has taken a special interest in this installation from its very commencement. In 1881, when so few electricians knew

practically how incandescent lighting should be carried out, and when the Messrs. Graham were erecting their large new premises, they consulted Sir William Thomson as to whether or not they could rely upon the electric light. Upon his recommendation they fitted their premises throughout with the electric light, without introducing gas-burners into any of their offices and warehouses. The actual carrying out of the present wiring of the building has been entirely done by one of the writer's students, who has had charge of the installation for the last six years. The Messrs. Graham have been so pleased with the care and attention which he displayed that they have sent him to their new premises which they are at present erecting in Oporto, Portugal, to superintend the building and put up an electric light installation there.

The dynamo which the writer tested, and which gave a total insulation resistance of 40,000 $\omega$, against framing and dead earth, is a Manchester shunt-wound machine by Messrs. Mather & Platt, put down about two years ago to replace the direct lighting of the building by three of the oldest shunt-wound dynamos in existence. These three machines are now employed in charging accumulators which serve to feed the lamps used in the darker parts of the building during the daytime. The writer tested the insulation resistance of one of these old machines, and found that it gave for armature and shunt coils just over 10,000 ohms, as against the frame of the machine.

In addition to the above tests, that indicated by Fig. 4 was applied, and no perceptible leakage current was observable on the voltmeter, even although the sensitive coil, which reads to less than a volt, was inserted.

The writer might give the results of several new installations which have surpassed his rule, and which therefore far exceeded the demands of the Society's rule. He therefore thinks that the constant of the latter might be raised from 5,000 to 10,000 with perfect fairness, and with beneficial results to the electric lighting industry as a whole.

The SECRETARY: The following telegram arrived at the office of the Institution this afternoon, addressed to Professor Jamieson. I have since telegraphed to him to know if I might read it, and he has replied in the affirmative. It is from Mr. Geipel, at Edinburgh, and is as follows:—"Tested Conservative Club to-day, "insulating dynamo and circuit from earth. 140,000, or three to "four times your standard. 65 volts, 147 lamps." The Secretary.

The following is a letter received from M. Picou, Foreign Member, in consequence of his having had a copy of Professor Jamieson's paper sent to him:—

COMPAGNIE CONTINENTALE EDISON, 45, RUE DU PARC,
*January* 23*rd*, 1889.

To the Secretary of the Institution of Electrical Engineers.

Referring to Professor A. Jamieson's paper, I should like to make a remark on the value $k = 500$ in my formula.

I quite agree with him that this figure is very low, and that in practical industrial work higher values of insulation are easily obtained than those resulting from this formula. I did not intend to give the figure that the insulation of the installation should have, to be considered as good, but rather the minimum standard, under which it should be considered as faulty. At this minimum the fault must be searched for and corrected.

It is generally personal judgment which decides whether insulation is practically good or not. But the determination of the minimum insulation under which there is risk of fire or destruction of material, offers an interest of an impersonal and absolute nature, which is of very great importance.

That limit may be advantageously fixed by the discussion of Professor Jamieson's paper, and I am happy to have, for my part, contributed in raising this discussion, which will be extremely profitable to electrical work.

R. V. PICOU, Foreign Member.

The CHAIRMAN: Perhaps Mr. Crompton will favour the meeting with his remarks on this paper? As a member of the Committee of the Institution which drew up the electric lighting rules referred to, he may probably be able to defend the principles upon which they were based. The Chairman.

Mr. CROMPTON: I trust that in this discussion some of the younger members of the profession who are engaged in superintending the wiring of houses may take a principal part. Its value will be increased in proportion to the number of facts such gentlemen can bring forward this evening. I confine my remarks, Mr. Crompton.

Mr. Crompton.

therefore, to classifying the sub-heads under which it might be convenient to discuss Professor Jamieson's paper.

The insulation resistance of complete installations may be subdivided as follows:—1st, the dynamo; 2nd, the distributing apparatus or switch-boards; 3rd, the insulated conductors themselves; 4th, the various fittings, such as switches, cut-outs, and lamp attachments.

Dealing with the first matter, I must point out that Professor Jamieson has dealt with the subject as if the great majority of installations contained dynamo machines, and most of his methods of testing relate to such installations. Now I must point out that, thanks to the increased number of central stations either actually at work or projected, the number of houses wired without a dynamo are twenty-fold of those with a dynamo. The total insulation of a self-contained installation containing a dynamo need not be nearly so high, and consequently need not be treated so carefully, as that of one forming part of a central station system, as it is obvious that a leak which might be of slight moment in the case of detached installations may, on account of the addition of many other similar leaks, become of serious moment, and exceedingly embarrassing to the engineer in charge of the central station. Your President has just said that I am one of the members of the Committee responsible for the rule contained in the Institution's pamphlet relating to insulation. First, as to the wording of that rule, which Professor Jamieson considers is most obscure. I cannot agree with him on this point. It is surely a simpler matter to prescribe that the leakage of current to earth shall not exceed a fixed percentage of the total current required for the whole of the lamps for which the installation is wired, than to follow Mr. Jamieson's rule, which takes into account varying E.M.F.'s; but I am disposed to agree with Professor Jamieson that the 5,000th part allowed in our rule, although it sufficiently protects detached installations, is not sufficient to protect an installation forming part of a central station, and I am quite prepared to adopt his suggestion to reduce the leakage to $\frac{1}{10,000}$th part.

Returning to the question of the insulation of dynamos, it is

Mr. Crompton.

no longer a difficult matter to obtain high insulation on every part of dynamo machines. Now that we dynamo makers have had our attention directed to the fact that there are far better varnishes than shellac, there is no difficulty in obtaining any desired degree of insulation on the dynamo itself from the first moment that it is wound. In the days when we used shellac it was impossible to obtain a high degree of insulation until the whole of the spirit contained in the shellac varnish had evaporated. There are other objections to its use which I need not particularise here, as it is foreign to the subject under discussion, but which have forced us into the use of other varnishes having a high insulating quality even while they are still moist.

On the subject of the dividing apparatus for switch-boards I have little to say. As has been before pointed out, the use of slate, unless very carefully selected, has been a very common source of trouble; but this difficulty can be avoided, as has been already pointed out here, by carefully selecting the slate, or by coating it with paraffin or varnish.

Next in order comes the insulating wires themselves. The manufacture of these has so greatly improved during the last few years that the insulating resistance of the covering is so high, even in the case of second-class wire, that I think they leave little to be desired; so that there is no excuse for any contractor using a wire having a covering of low insulating quality. When I speak of second-quality wire, I mean wire of which the insulation is of pure rubber, as most makers vulcanise their first quality. So long as our coil of wire remains uncut, the insulation may be measured by megohms; it is only after it has been cut into lengths, jointed up, and connected on to the various fittings, that the practical difficulty of ensuring high insulation commences. The question of obtaining good joints I will leave to others who are every day dealing with this subject in a practical manner; but I wish to dwell somewhat strongly on the fourth sub-head, viz., the insulation of the fittings that we are forced to employ.

I say "forced to employ," because the use of fittings made of glass, glazed porcelain, earthenware, slate, and similar materials

Mr. Crompton.

has been so strongly prescribed by the Insurance Offices that we have no option in the matter; but I would point out that the use of these fittings, having a highly glazed or cold surface, hence liable to condense on to them a film from our moist London atmosphere, renders it an extremely difficult matter to obtain a high insulation to earth for a large London installation. So long as we were able to use wood which could be rendered practically fire-proof, and could be varnished in such a manner that it did not condense on to it these films of moisture, it was a comparatively easy matter to obtain a high insulation, and the question arises whether we have not purchased the advantages we gain by using these fire-proof fittings at too high a cost. No doubt for some situations their use is absolutely necessary, but in most other cases their advantage is questionable.

As to the methods of testing proposed by Professor Jamieson, the lineman's detector, if used with a source of practically constant E.M.F., and carefully calibrated with that E.M.F. so as to give a certain deflection for each fraction of an ampere passing through it, is quite the most convenient instrument for use by ordinary workmen or foremen. Such an instrument is quite as portable as the voltmeter he proposes, and is generally wound so as to be much more sensitive. When such an instrument is used for testing dynamos, as, for instance, at our own works, we use it with the standard E.M.F. of 100 volts of our lighting circuit, and only pass our work if the deflection is below a certain limit, which limit is dependent on the magnitude of the work tested; but when testing installations connected with a central station, it is obvious that we cannot use the E.M.F. of the mains, as in that case the existing leakage on the mains would trouble us. For such work, therefore, it is better to provide the constant source of E.M.F. in the shape of a portable hand dynamo machine which can be driven at a certain speed, and, consequently, give the E.M.F. within the required percentage of accuracy. At Kensington we use a portable battery of accumulators. Messrs. Siemens, of Berlin, make the portable dynamo apparatus mentioned above, and I believe it is used universally throughout the Berlin central stations. I have also seen it used in other Continental towns.

In conclusion, I agree with Professor Jamieson that it is quite easy, if due precautions are taken, to obtain excessively high insulation on installations of considerable magnitude. In one case, in London, where we had to work to a very severe specification—which has become rather celebrated through a discussion which took place on it in the technical journals—we obtained a resistance of 439,000 ohms, which is about four times in excess of the requirements laid down by Professor Jamieson's rule for the number of lamps, viz., 130. In other cases we have got results sixfold in excess of that required by Professor Jamieson. It is unnecessary to quote these. Mr. Crompton.

Mr. ALEXANDER SIEMENS: I do not think I have much to say, but my ideas of insulation are different from those which have been brought forward. I will not say anything of our own installations, but we had to examine an installation of about 111 lights because a central station in London raised the question of insulation, and on measuring the insulation resistance we found it somewhat over 5 megohms, and the insulation resistance of the dynamos—I forget the maker—was over 1 megohm. This is very far in excess of any of these rules, and was something like what it should be. I think Professor Jamieson is wrong when he says that the resistance does not require to be nearly so high per unit of length as in the case of submarine cables. Why should we be content with a much lower insulation resistance in these installations than in submarine cables? I daresay we cannot get it quite as good, but we should try to get as near as possible. Mr. Siemens.

The CHAIRMAN: There must be a great many members present who have had experience in this subject of insulation resistance of electric lighting installations. Of course there is no definite rule given by Nature as to whether the standard should be 5,000 ohms or 5,000,000, or anything else. The insulation resistance asked for in submarine cables is simply the result of years of experience as to what engineers can get by more or less fighting with the contractors; and consequently, in the same sort of way, the insulation that will be obtained for the house installations will have to be settled by actual practice. Therefore the results of The Chairman.

The Chairman. experience are what we particularly want; in fact, I presume the object that Professor Jamieson had in communicating this paper was to elicit information rather than to give it. I hope, therefore, that some of the members who have had experience will continue the discussion.

Mr. Human. Mr. H. Human: I wish just to correct an error that the author has fallen into in referring to the Westminster Fire Office rules. The same error was made by Mr. Preece last year in dealing with this question, and on my calling his attention to it, he admitted that he had misunderstood the rule. The rule to which the author refers as showing that the electrical resistance should be 150 megohms per mile, has no reference whatever to the insulation resistance of an installation as a whole. It simply refers to the electrical resistance of the dielectric or covering of the conductor. Now the authors of those rules were very careful to get the opinions of experts on this question of insulation. They first of all thought of drawing up a rule defining what should be the leakage allowed in an installation, basing it upon the E.M.F.; but they were advised, and I think wisely so, to leave the matter alone, and let the electricians themselves deal with it, they having a greater interest in the subject than the Fire Offices. But having specified what the nature of the coating should be, it became also necessary to specify what its minimum electrical resistance should be, and we found that in practice a cable having an insulation resistance of 150 megohms per mile would, in nine cases out of ten, be sufficient. Therefore this 150 megohms, as referred to by the author, has nothing to do with the subject he is dealing with. I feel bound, on the part of the authors of those rules, to make this correction.

As to incombustible bases, Mr. Crompton truly remarks that the Fire Offices were originally satisfied with wood bases, and of course the results were good so far as mere leakage was concerned; but I may tell him this as the experience of the majority of the Fire Offices—that whenever a failure happened or an accident occurred it was invariably through those fittings, *i.e.*, either with regard to the cut-outs or the switches. We found, therefore, that it became absolutely necessary to prohibit what was then the

common practice, of putting them upon very inferior wooden blocks, and we thought that the best thing under the circumstances was to specify for incombustible bases. Well, I am pleased to hear from Mr. Crompton that, in one case at all events, he has had no difficulty in obtaining, even under these conditions, very high insulation. We have gained by experience in these matters, and when we find an accident, or the cause of an accident, we make a note of it, and are careful that in future it shall be provided against. These are the reasons the Fire Offices insist upon incombustible bases to fuses and switches. If I remember rightly, Mr. Cockburn last year, in his admirable paper dealing with fuses, pointed out that it was objectionable in many cases to use wood; that in the ordinary fuse, where there was considerable heating of the fuse itself before absolute breakage, it was in some cases possible to ignite the base. These are probably the exception, but still they are liable to occur, and it is only right that we should provide against them. It must not be forgotten that when an accident does happen someone has to "pay the "piper," and the Offices are justified in protecting themselves against such cases. But I am quite certain that no one would unnecessarily impose conditions that would retard the introduction of the electric light. The fact is, we have faith in the electric light, and we are convinced that in the future we shall derive great benefit from it. At present, of course, our experience is limited, and we can only judge by results. We are still, as it were, suffering from the use of two modes of lighting—that is to say, by gas and electricity in combination. Our experience has been very good so far as electric lighting alone is concerned, but it has not yet sufficiently gained the confidence of the public to enable them to dispense with other forms of lighting. I am not, of course, speaking of its danger, but of its stability or reliability. The public still keep gas as a stand-by in case of a breakdown, with the result that the gas fittings are frequently either put to improper uses or neglected, and the joints become impaired. Thus, when it happens—as unfortunately it will happen at times—that there is a temporary breakdown of the electric light, and the gas fittings are again brought into requisition, a leakage may occur, Mr. Human

Mr. Human. with a probable explosion following. So that we are suffering indirectly in some respects from the use of electricity for lighting purposes. When, however, public confidence is restored—which depends entirely upon electrical engineers—and when other forms of lighting are entirely dispensed with, then we shall doubtless better appreciate the advantage of the electric light than we do at present.

Mr. Evershed. Mr. SYDNEY EVERSHED: I fully agree with Mr. Crompton that the author's treatment of insulation testing is rather archaic. In the first place, one cannot carry a mirror galvanometer about for use in houses which are half-finished. Nearly all the houses which are now being wired in London are only just built, and in many cases the wiring and internal fittings are being put in together. I inspected some eight or ten houses a few months ago which were completely fitted with electric bells and speaking tubes, gas pipes, water pipes, and electric light wires. All these things were buried in the plaster. This seems very bad policy so far as the latter are concerned, considering the effect damp plaster will probably have on the insulating material of the cables.

Mr. Alexander Siemens made some objection to the use of a voltmeter in testing. Of course the method described in the paper looks very pretty in a diagram, where the voltmeter is put on to an apparently good earth; in practice, however, it will be difficult to find a good earth. But Mr. Siemens was mistaken when he said this test would burn the voltmeter coil, for it cannot possibly get more volts on it than the dynamo will give, and presumably it will stand that. However, ordinary voltmeters are useless for this method, as their resistance is so low that you get no reading from line to earth. I imagine Professor Jamieson was thinking of a Thomson graded voltmeter, which has a resistance of perhaps 10,000 ohms. Very small leaks can be measured with these voltmeters, but I need hardly say that no such instruments are made sufficiently portable for testing work.

Mr. Carpenter. Mr. W. LANT CARPENTER: With your permission, Sir, I should like to put a question and make a suggestion. As we have so

many gentlemen here to-night, I would ask whether there is any well-authenticated instance of a melted tin wire in a safety fuse having actually caused a fire by inflaming wood or anything of the kind? The suggestion that occurs to me rather supplements what you, Sir, and Mr. Crompton have said—that there may be many gentlemen here who have had wide experience of testing insulation resistance, and who may perhaps feel some delicacy in giving their experience because they may feel, to some extent, that their information belongs to their employer rather than to themselves. If there be any such here to-night, I would venture to suggest to them that any information that they may be disposed to put at the service of the Institution might quite well be given without a name, or other means of identification of any sort or kind, being mentioned in connection with it. It would be very valuable to the Institution, and it is a great pity that we should not have it, more particularly as I fully agree with Professor Ayrton's remark that the object of this paper was quite as much to elicit information as to give it. I know that that was the opinion of the Council in looking over the paper and in speaking of it downstairs just now, and I therefore venture to hope that we shall have some valuable information given. Mr. Carpenter.

Mr. W. B. Esson: I should like to call attention to the two tables on pages 52 and 56, which seem to be of an entirely different character. In Jamieson's table, page 52, you will observe that the insulation resistance is supposed to increase with the voltage, and that the percentage of current leaking goes up with the E.M.F.; while in the Society's table, given on page 56, the ideal condition of a constant percentage of leakage is set forth. In actual installation work for incandescence lamp circuits neither of these conditions obtain, the insulation resistance being, as a matter of fact, approximately the same whether the E.M.F. is 50, 100, or 200 volts, and the percentage of current leaking increasing, therefore, as the *square* of the volts. Mr. Esson.

Mr. Bernard Drake: I should like, Sir, to make a remark in reference to Mr. Crompton's opinion of pottery for bases of cut-outs. He is undoubtedly right in saying that leakage is more Mr. Drake.

Mr. Drake. generally due to switches and cut-outs than to the cable, but I maintain that the Insurance Companies are quite justified in insisting on pottery or incombustible materials. I have seen several bases of wood with unmistakable signs of burning from leakage through moisture absorbed by the wood.

Now this could not result with pottery, the only objection to which is condensation and surface leakage. This, I believe, will be found to vanish if the current is put on for an hour previous to testing for insulation. In support of this I may mention that on a recent occasion I had to test the leads at Lord Armstrong's private house, and found apparently a dead earth on the mains. These consisted of bare copper rods supported on pottery slabs placed in a wooden trough which was lying on the ground. I believe this system was designed by Mr. Campbell Swinton.

The pottery was found to be wet, and the leakage evidently on the surface. After, however, the current had been used for about ten minutes, we stopped, and were surprised to find that the leakage had practically vanished. It would be interesting to know whether this was due to polarisation or the drying of a portion of the pottery near the conductor. If the same takes place with the pottery bases, there is no practical objection to them, but the final tests for insulation must be made after the current has been on.

There is a much more serious question than this to which I would direct the attention of Fire Offices, and that is the placing of wires in new houses under boards when the pugging is not dry. A process of "sweating" then takes place, which will in a month rot off the insulation of the best wires made, and there is then a leakage through the damp casing. My own opinion is that you should give your wires all the air possible under boards, and place them well apart on the sides of the joists, but not in casing.

Another most fertile source of trouble to which no allusion has yet been made is the jointing of the cables. The usual course adopted is to smear the bared copper with Chatterton's compound and wind it up with waterproof tape; but the insulation will be found to be weaker here than elsewhere in the cable,

apart from the danger of the compound cracking and moisture penetrating the joint. This matter should be faced by the cable makers in their own interests, and they should insist, for their own credit, on a specified method of jointing, and, if possible, provide the materials themselves for use with their own cables. Mr. Drake.

Mr. GUY C. FRICKER: With regard to the insulation of slate bases and fittings, I find that the slate may be greatly improved in this respect by soaking it in melted paraffin wax, and that the films of moisture mentioned by Mr. Crompton do not then appear to be troublesome. I think that this plan might with advantage be extended to the treatment of unglazed earthenware fittings, which might then insulate better than if glazed. Mr. Crompton was also rather hard upon shellac varnish as an insulating compound. It is quite true that shellac varnish will not dry properly if it is used on each layer of wire when winding up a coil, unless it is afterwards baked for a very long time; but in well-ventilated armatures it is a perfectly good varnish to use, and I may say that I have never used anything else. In regard to the insulation leakage of cables, it seems to me that, speaking strictly, the insulation resistance should be inversely proportional to surface of the cable rather than to the number of lamps fed by it, as, I think, is proposed in the paper. Although the quantity of current carried by a conductor *should* be proportional to the cooling surface, by the rules under which electrical contractors usually work, it is governed by the cross section, and therefore it would seem to be better to specify definitely in such a way as to ensure against more than a certain current-density of leakage through the insulation. Mr. Fricker.

Mr. W. A. CHAMEN: Perhaps, Sir, I can offer a few remarks which may be useful. I can say from experience that there is a great deal of difficulty in insulating the backs of fittings, more particularly of double-pole cut-outs, which we have been almost forced to put in by some of the Fire Insurance Offices. These bring the two opposite poles of the circuit close together, thus offering facilities for leakage between them. In some Fire Office rules it is recommended that the cables or wires should be plastered in the walls; I think in some cases Portland cement is Mr. Chamen.

Mr. Chamen. mentioned. Some years ago, at the Forth Bridge, I found that Portland cement was absolutely useless. The insulation was all right for some time—I think about a twelvemonth; but after that time it was completely gone. Portland cement certainly seems to have some deteriorating effect upon insulation. Plaster of Paris has been often used, and in dry places, perhaps, that is all right; but in ordinary circumstances I think it very undesirable to plaster leads in at all. Where conductors pass through a wall, particularly from one risk to another, some of the fire inspectors insist upon their being plastered in or cemented. What I have always done in such cases, however, is to put in porcelain or glass tubes, or, better still, an iron tube lined with glass, and to plaster round that, but on no account to put any plaster next to the conductor.

Referring to the fusing of a tin wire, or other cut-out wire, causing the charring of a wooden base, I have never known such a case. I have seen wooden cut-outs charred, but I have always been able to trace this charring either to a copper wire having been inserted through somebody's carelessness, or else to an arc having been formed. The latter, of course, cannot be guarded against by any form of cut-out, and for that reason it may be desirable to make the bases incombustible.

With regard to moisture, I can quite understand Mr. Drake's remarks, although I rather think that the cable used in casing underneath the floor cannot have been sound if any amount of dampness lying between the two cables caused leakage. I think that the insulation of most of the cables which are now used is such that they may be laid under water without any serious leakage; at any rate, those used on the Grosvenor Gallery installations might. I think, however, there is a source of danger even in using such thick insulation, especially when it is vulcanised rubber, for it is well known to those who have the practical handling of these cables that where a cable has to go round a sharp corner the insulation is often broken. After a cable has been made some little time vulcanised rubber seems to get hard, and breaks more easily. I think, however, that pure rubber cables, taking them all round, stand a better chance than

the vulcanised ones. I cannot say whether after a number of years, provided they were properly and carefully laid in the first place, the pure rubber would prove to be better than the vulcanised, for I have not lived long enough or had the length of experience to find out. Mr. Chamen.

Respecting joints, I quite agree with Mr. Drake, but I have long ago discarded "Chatterton" altogether. I found that the men would plaster "Chatterton" about the joint without having it properly melted, and leave holes in it. I have for some time past used india-rubber strip, which is laid up round the cable, also using india-rubber solution for the purpose of sticking it together. If a joint is carefully made in this manner I think it is perfectly good; it may not be better than "Chatterton," provided the "Chatterton" joint is properly made. It is most difficult to keep a light, even a spirit lamp flame, burning in many places, for there is often draught enough to blow it out. When working in houses such as Mr. Evershed referred to just now, where there are no windows or doors, you cannot do anything at all with a lamp. I have actually had a man bind up a joint without soldering it, simply because he said he could not keep his iron hot enough. Of course with the india-rubber and solution no heat is necessary. After the india-rubber strip is put on I generally use a serving of waterproof tape. I do not mean that these joints are always perfect, because I believe that nine out of ten workmen, if they are not watched, will make bad joints.

Mr. H. C. DONOVAN said that experience gained in one branch of electrical construction must ever be useful in another, and most especially in the means to be adopted to maintain a good state of insulation in mains, leading and connecting wires. He was of opinion that to ensure a constant insulation in electric lighting installations the choice and quality of the insulating materials were not of so great importance as strict attention to detail, so as to prevent undue loss and a gradual fall in insulation, due, not to the loss of current through the insulating material, but to what is known to many as *surface leakage*—that is, the current escaping from the bared ends of the conductors in leading Mr. Donovan.

Mr. Donovan.

and connecting wires, also contact-pieces, plugs, switches, and commutators, which are fitted on slabs, pedestals, and brackets of the well-known materials used for this purpose. Leading wires, whether insulated with gutta-percha or the different forms of india-rubber coatings, are sure to fall off in insulation when the ends become coated with dust consisting largely of carbon particles (especially in large towns where coal is used). This coating of dust becoming wet, as it is sure to be when the atmosphere is damp, it can readily be understood that a source of leakage is set up, first through the moist dust and dirt, and thence to the damp tape and felt on the outer surface of the leading wires to "earth." The same remarks, to a great extent, apply to the insulating base of contact-pieces, plugs, switches, and commutators. When new they have perfect insulation, but in time, if they be not kept clean, damp dust will do its evil work, conduction will take place over the surface, and render the best of insulating material useless as regards the purpose for which it was selected.

For the same reason too much confidence should not be placed in paraffin wax. It is a great dust collector, and consequently its surface may become a moisture absorber. The moral of these remarks is that the ends of all leading wires, also the surface of all insulating slabs, &c., should be kept clean. It should be borne in mind by all electricians that the climate of the British Isles, except from February to June, is exceptionally damp.

It is natural to see that all these precautions are of more importance to the consumers than to the contractors, as the latter are sure to get rid of their responsibility before the trouble commences.

When the late Professor Varley first visited the United States, he was at once struck with the utter disregard electricians there had to all insulatory precautions—for instance, brass keys and fittings screwed direct on to wooden tables and benches, single cotton-covered wire used for connections everywhere. He proclaimed, much to the surprise of the Americans, that there was no insulation; but there was, for Nature had provided the American electricians with something more useful than india-rubber or gutta-percha, more insulating than paraffin wax—an exceptionally

dry atmosphere. It is the same in Australia, where the climate is very dry for the greater part of the year. Mr. Donovan.

With regard to the making of joints, it should be borne in mind that it is essential that the jointer, before he is called upon to make a joint, should wash his hands. Very often he has other work to do, and when called upon to make a joint his hands are begrimed with every form of mislocated matter, often largely consisting of oil and brass filings. He fully admitted that this was a matter of detail which at first sight might seem an item of no importance.

Mr. W. E. GRAY: I should like to say a few words on this paper. Mr. Jamieson, in referring to submarine cables, says that the engineers insist that these cables should give an insulation resistance of 300 megohms, fourteen days after manufacture, and after two minutes' electrification. I am afraid this is hardly complete, as he does not say what number of cells are to be used, and the time of immersion. For electric light installations the conditions of work are distinctly different. Mr. Gray.

I presume in such installations one does not care so much merely about the initial resistance in ohms or megohms, as about the permanent resistance—that is to say, an insulation resistance that shall remain always about the same. This may be a low resistance, as the amount of leakage would probably be inconsiderable, the tests being taken merely to show the actual state of the circuit. Mr. Crompton says he finds that when using slate or porcelain casing there is a deposition of moisture on the surface, and that if wood casing were used instead, a second-class wire could also be used—*i.e.*, that he did not depend on the insulation of the wire for the insulation of the whole circuit, but rather on the insulating qualities of the case.

In such a climate as we are favoured with here in London, this, I think, is rather risky. Another gentlemen—who, unfortunately for me, I do not know—said that he believed in cables which could be used under any conditions of climate, and, therefore, preferred such cables as would test in water. This seems only reasonable, if we are to meet with any great difficulty, due to moisture, in installation work. He then referred to vulcanised rubber, and said

Mr. Gray. that in bending this round corners he found it to crack. This seems rather peculiar, as rubber is supposed to be elastic; but probably he may refer to the cracking of the tapes, which are not rubber at all. He said, I understood, that he had not lived long enough to know whether or not vulcanised rubber would last longer than pure rubber. Well, I may not have lived longer than he has, but I think he will find, as others have found, that vulcanised rubber will last longer; and this is, I believe, the general idea amongst manufacturers, and the principle on which they work, as they actually use vulcanised rubber, and not pure rubber, which they know will not last.

Mr. Drake referred to the making of joints, and spoke of using Chatterton's compound for them. It seems rather a bastard cut of thing to do, to put gutta-percha on to an india-rubber insulated cable, as certainly, if any increase of temperature takes place, the joint will leak. There is clearly no doubt of this, owing to the different rates of expansion of these materials, and Chatterton's will not stand anything like the same degree of temperature as vulcanised rubber. To make a homogeneous joint, pure rubber tape tightly wrapped on and smeared with india-rubber solution, as has been suggested, would probably be by far the best; and if tightly wrapped on, and the workman's hand be clean, the joint being properly made, there is no doubt that such a joint can test in water. After a certain time, this joint, from its own defects—*i.e.*, the rubber starting slightly to decompose—will simply become a homogeneous covering round the conductor joint. It is true that this will not last so long as the cable itself, but I suppose it to be the best that can be done under the circumstances.

You, Mr. Chairman, made a remark that I did not quite understand. You, I think, said that you had no doubt the engineers could wring from the contractors a good quality of material. It seems to me that no engineer can wring from a contractor that which he cannot make. If he can make a first-class quality, he tells the engineer that he can, and the engineer afterwards specifies for it; but really a great deal in this installation work must depend on whether or not the contractor

is a good one. It is no good giving an empirical sort of figure for insulation resistance per lamp unless you are certain that the insulation will last; and that, it appears to me, is the great point. I am afraid that a figure taken upon a favourable occasion, as pointed out by Mr. Crompton, or on a nice dry day, really can hardly be assumed by the Fire Insurance Companies to be such an insulation as they require. They must go further, and examine into the class of material that has been used; and if this material is bad, or unsuited for the purpose, although for the time being it may give a high insulation and appear to be all right, still, in my opinion, they would be quite justified in rejecting it. Mr. Gray.

The CHAIRMAN: Before adjourning the meeting I will just remark that I have so high an appreciation of the improving power of contractors that the specification I should make in all cases would be "a little higher (whatever the thing was) than "they had been hitherto able to get," and I am sure that they would then be able to get what I want. The Chairman.

I am told that there are several members who wish to speak on this subject, and the discussion will be adjourned until February 14th.

A ballot took place, at which the following were elected:—

*Members:*

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|---|---|
| Alfred Upton Alcock. | Jorgen Henry Ferdinand Toll. |

*Associates:*

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|---|---|
| Frederick Francis Bennett. | Charles Cæsar Hawkins. |
| Walter R. Cassels. | William John Thorrowgood. |

*Students:*

| | |
|---|---|
| Donald Frank Adamson. | Edmund A. Hall. |
| Frederick Robert Connell. | Arthur William Rankin. |
| Archie Davidson. | Joseph D. Rolls. |

The meeting then adjourned.

# ABSTRACTS.

## W. KOHLRAUSCH—LIGHTNING CONDUCTORS

(*Elektrotechnische Zeitschrift, Vol.* 9, *pp.* 228 *and* 237, 1888.)

The author first deals with the vexed question of using gas and water mains as "earth," and then passes in review some simple apparatus for testing the resistance of lightning conductors.

From actual experiments he has found that the resistance of an "earth" formed by gas or water pipes is less than that of the usual earth-plates. In all buildings protected by a system of lightning rods, the conductors must in some part of their course come into proximity to some portion of the metal pipes; and since the resistance of the "earth" of the conductor is greater than that of the pipes, the electric current, even though it may for some distance traverse the conductor, has a tendency to leap across to the pipes. There is, therefore, every reason for connecting the conductor from the lightning rod to the gas and water pipes. The way in which the joint between the conductor and the main is made is not of great importance; the two essential points are that the two shall be in thoroughly good metallic contact, and that the joint shall have a fairly large surface. The connection may be made at any convenient spot, provided that, if the pipes are of iron, the section of metal between the joint and the "earth" shall nowhere be less than 100 square millimetres (0·155 sq. in.); and if of lead, five times as large. Meters, whether gas or water, should be short-circuited by a length of stranded copper wire.

The nature of the joints in the pipes is of some importance; but actual experience has proved that there is no risk of danger to the pipes if the joints are soldered, screwed, flanged, or spigot joints filled in with lead. In the town of Hanover a great number of "earths" made by means of the gas and water pipes have existed for over ten years, and no damage has resulted from their use. There is not much to choose between gas pipes and water pipes, though gas engineers have raised objections to the use of the former on the ground of the resistance which must be introduced at the joints by the use of red lead.

Some actual measurements are of interest. The first series was carried out in the High School at Hanover, on the gas pipes in the building, some of which had been put up seven years previous to the time of testing, and others three years. The total length of pipe tested was 75 metres, in which there were 117 joints, all made with red lead. The total resistance was found by experiment to be 0·12 ohm; the resistance of the pipes themselves, as calculated from their section and length, was 0·086 ohm, thus leaving 0·034 ohm as the resistance of the 117 joints, or a mean of 0·0003 ohm per joint. It appears, therefore, that screwed joints in iron pipes made with red lead give a sufficiently good metallic contact.

The second series of experiments was carried out on large gas mains with the usual spigot joints, made gas-tight by molten lead being run in. In order to obtain the resistance of the pipes alone, advantage was taken of a main existing in the gas works at Hanover, which was not buried, but rested on a wooden flooring. This served also as a road for the trucks laden with coal and coke, and consequently the pipe line was exposed to much vibration, which seems to have loosened the joints, as very varying resistances were obtained—*e.g.*, 0·015, 0·04, 10·4, 420, 0·07, 4·4, 55, 26, 40, and 61 ohms respectively. Six lengths of pipe of 10 cm. outer diameter were put together specially, and the resistance of the five new joints was found to be only 0·009 ohm. The resistance of the joints is, however, of secondary importance in the case of gas mains buried in the ground, as, owing to their large surface, the "earth" resistance of even the first length or two will be much less than that of a joint, so that the current will be earthed before it has to traverse more than one or two joints.

The second portion of the paper deals with several new portable testing apparatus, which have been devised and manufactured by different firms, and which comprise batteries, magneto machines, bridges, galvanometers, and telephones for use with both continuous and alternate currents.

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## J. CHAPPUIS and G. MANEUVRIER—THE MECHANISM OF ELECTROLYSIS BY MEANS OF ALTERNATING CURRENTS.

(*Comptes Rendus, Vol.* 107, *p.* 31, 1888.)

The absence of any electrolytic action by an alternating current in a solution of sulphate of copper, for example, is generally explained by saying that the copper deposited on each electrode by one current is immediately dissolved by the opposite current. In the course of their experiments the authors have been able to render this action visible.

. It would seem that the phenomenon depends upon the existence of a state of equilibrium between the rapidity of decomposition of the electrolyte and the rapidity of recombination of its elements. The most important factor in determining the rapidity of electrolysis is the current-density. Now this can easily be increased by diminishing the surface of the electrodes, the current being kept constant. A voltameter, when filled with acidulated water, produced an abundance of hydrogen and oxygen with a current of 2·5 amperes; but on replacing the acidulated water by a solution of sulphate of copper, no electrolysis was set up, until the electrode-surface had been reduced to about 6 square millimetres, when gas was evolved and copper at the same time deposited. The material of the electrode also influences the result. Of greater effect is the number of alternations of the current. An alternate-current machine was driven at increasing speeds so as vary the number of alternations, while, by means of an adjustable resistance in the exciting circuit, the current was kept constant.

With 133 alternations per second, the current was adjusted until recom-

bination of the elements just balanced electrolytic action; on diminishing the number of alternations to 100 per second, electrolysis took place. Again, the current, having 133 alternations per second, was brought to such a strength as to produce an abundant development of gas; when the alternations were increased to 173 per second, all evolution of gas ceased.

The effect of a change in the current-density is just contrary to that of a change in the number of alternations per second; therefore even currents of small surface density may produce electrolysis if the number of alternations are sufficiently reduced. This explains how it was that De la Rive was able even with a large electrode-surface to produce electrolysis by means of the current of a magneto machine, since it gave only about 50 alternations per second.

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## G. MANEUVRIER and J. CHAPPUIS—SPONTANEOUS EXPLOSIONS OCCURRING DURING THE ELECTROLYSIS OF ACIDULATED WATER BY MEANS OF AN ALTERNATING CURRENT.

(*Comptes Rendus*, *Vol.* 107, *p.* 92, 1888.)

It has always been observed that when water is electrolysed by means of an alternate current, the mixed gases in the voltameter explode sooner or later. De la Rive, who noticed the phenomenon, attributed it to the catalytic action of the platinum black which he saw formed on his platinum electrodes. Bertin attributed the explosions to the polarisation of the electrodes.

During the experiments referred to in the preceding abstract these explosions were a constant source of trouble to the authors. They therefore carefully investigated the phenomenon, with a view to its prevention. Their results show conclusively that the explosion of the mixed gases is brought about by the platinum electrodes becoming heated to such a point as to determine the recombination of the dissociated elements. This heating is due to three causes, all of which depend ultimately on the gradual descent of the level of the electrolyte in the voltameter, thus leaving more and more of the electrodes in contact with the gases. Firstly, owing to the smaller surface of the electrodes, the current-density is increased; secondly, the surface resistance is increased; thirdly, the cooling action of the electrolyte is decreased. It becomes, therefore, easy to prevent the explosion by taking due precautions to guard against the heating of the electrodes.

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## G. CHAPERON and E. MERCADIER—ELECTRO-CHEMICAL RADIOPHONY.

(*Bulletin de la Société Internationale des Electriciens*, *Vol.* 5, *p.* 395, 1888.)

A rod cast from sulphide of silver, and a rod of pure silver, are placed in an inactive electrolyte, *e.g.*, acidulated water; if, then, light be allowed to fall on the rod of sulphide of silver, a current will be set up. Even the light from a candle at a distance of 5 feet will produce an appreciable deflection on a

delicate galvanometer. On open circuit the sulphide of silver is little affected by diffused daylight. Better effects may be obtained by using a plate of silver on which a thin layer of sulphur has been deposited electrolytically. Screens cutting off various portions of the spectrum do not interfere with the phenomenon, so that probably a very large portion of the spectrum is capable of producing the effect.

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## W. E. AYRTON and J. PERRY—EFFICIENCY OF GLOW LAMPS WITH DIRECT AND ALTERNATING CURRENTS.

(*Philosophical Magazine, Vol.* 25, *p.* 476, 1888.)

The problem to be solved is whether a Board of Trade unit is more valuable for lighting by glow lamps when the current supplied to the consumer is direct or when it is alternating. The complete solution of the problem involves the factor of the life of lamps under the two conditions, since the cost of renewals ought to be taken into consideration; but, as data on this point are wanting, the experiments dealt only with the other factor of efficiency. By an ingenious arrangement of circuits with intercalated resistances, and of switches, the glow lamp under observation could be supplied either with the alternate current furnished by a Ferranti machine, or with the direct current of the Gramme exciter.

The currents were measured by a specially constructed electro-dynamometer, wound with unusually fine wire, in order to obtain as far as possible uniform current-density. This led to waste of power in the instrument, which, however, was of no consequence in the actual investigation. The potential difference at the lamp terminals was measured by a non-inductive voltmeter, and the intensity of the light compared on a photometer with that of a standard candle, observations being made with both ruby-red and signal-green glasses.

It is known that with alternating currents the measured watts do not equal the true watts. To obtain the correct value we have

$$\text{True watts} = \frac{r\,T\sqrt{A^2\,V^2}}{\sqrt{l^2\,\pi^2 + r^2\,T^2}};$$

and in this particular case $\sqrt{A^2}$ is the square root of the mean square of the amperes measured on the electro-dynamometer, $\sqrt{V^2}$ is the square root of the mean square of the volts measured on the non-inductive voltmeter, T is the time between one alternation and the next—*i.e.*, half a complete period—$r$ is the resistance of the lamp filament in ohms, and $l$ is its coefficient of self-induction in secohms.

The observations—75 in number—were made alternately with the direct and with the alternating current. With green light, the alternating current gave 2·857 watts per candle, and the direct current 2·811 watts per candle; with red light the figures were respectively 3·247 and 3·286. The mean of all the 75 observations gave for the alternating current 3·0497 watts per candle, and for the direct current 3·0490 watts per candle. This very small difference the authors consider may be set down to experimental errors, and they there-

fore come to the conclusion that the efficiency of a glow lamp is the same for both direct and alternating currents.

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## G. FERRARIS—ELECTRO-DYNAMIC ROTATION PRODUCED BY MEANS OF ALTERNATING CURRENTS.

(*Beiblätter, Vol.* 12, *p.* 542, 1888.)

Two currents produce two magnetic fields in planes $x$ and $y$ at right angles to each other, which act on a point; then, if the currents are alternating and follow the sine law,

$$x = \mathrm{A} \sin. \frac{2\pi t}{\mathrm{T}}, \text{ and } y = \mathrm{B} \sin. \frac{2\pi(t+\beta)}{\mathrm{T}};$$

and if there is no phase-difference, the point will move in a straight line, or, in other cases, the motion will be elliptical or circular.

Two flat coils, the one with a few turns of thick wire, the other with many turns of fine wire, are placed at an angle of 90° to each other, with the windings in a vertical plane; the one is connected in the primary circuit, the other in the secondary of a Gaulard & Gibbs transformer, with an equal number of turns in each circuit. In the secondary circuit is a variable resistance. Between the flat coils a small closed hollow cylinder of copper hangs by a fibre. If the current passes only through the one circuit, the cylinder remains at rest; but if both circuits are closed, the cylinder rotates in one or the other direction, according to the disposition of the connections. The coils may also be arranged in two parallel circuits, one of which contains an inductionless resistance, and the other an electro-magnet with a large coefficient of self-induction. The copper cylinder may be replaced by one of iron, in which case the rotation is produced, not by the induced currents in it, but by the change of magnetisation; or, instead of hanging it from a fibre, the cylinder may be provided with pivots, and if made of sufficient size, may be used as a motor, and the work given out may be measured.

If the two sine currents are of equal strength, and have a phase-difference of a quarter of an oscillation, the magnetic field rotates with a uniform velocity V. If the cylinder revolves with an angular velocity $v$, and M is the moment of the couple exerted by the currents induced in the cylinder by the magnetic field, then the mechanical work of the motor per unit of time is $\mathrm{W}=\mathrm{M}v$, and the work done in the cylinder in producing heat is $\mathrm{P} = \mathrm{M}(\mathrm{V}-v)$; therefore $\frac{\mathrm{W}}{\mathrm{W}+\mathrm{P}} = \frac{v}{\mathrm{V}}$. If the specific resistance of the metal of the cylinder is $s$, then the work spent in heat is $\mathrm{P} = k\frac{(\mathrm{V}-v)^2}{s}$, where $k$ is a constant. Hence $\mathrm{M} = k\frac{\mathrm{V}-v}{s}$. The work W is a maximum if $v = \frac{1}{2}\mathrm{V}$, and a minimum if $v = 0$. If W is a maximum, then $\mathrm{W} = \mathrm{P}$.

In another form of apparatus four coils are used, arranged in two co-axial

pairs, which form the vertical sides of a cube, in the centre of which the metal cylinder, or a quantity of mercury contained in a glass vessel, can be made to rotate.

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**Professor J. A. EWING and W. LOW**—INFLUENCE OF A PLANE OF TRANSVERSE SECTION ON THE MAGNETIC PERMEABILITY OF AN IRON BAR.

(*Philosophical Magazine, Vol.* 26, *p.* 274, 1888.)

The bar to be experimented upon passed through a massive yoke of wrought iron, the cross section of which was more than a hundred times that of the bar. The lower end abutted against a set screw, whilst a lever, which could be loaded to any desired degree, rested on the upper end. The entire bar was wound with the magnetising coil, the current being furnished by means of accumulators. A small induction coil was wound at the middle of the bar, and was connected to a ballistic galvanometer, which was calibrated in the usual way by means of an earth induction coil.

The bars were first tested in one piece, then cut into two, four, and eight pieces successively. The magnetic permeability was considerably diminished by the cuts, but could be made to re-approach its former value by pressing the parts together, especially when the cut surfaces had been brought to true planes; indeed, with true planes a considerable force will destroy the resistance of the joint almost completely. Even if a film of gold-leaf is interposed between the iron faces, the resistance of the joint is only very little more than without, if the surfaces be true planes and the pressure very considerable.

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**H. A. ROWLAND and L. BELL**—ACTION OF A MAGNET ON CHEMICAL ACTION.

(*Philosophical Magazine, Vol.* 26, *p.* 105, 1888.)

Two small pieces of iron wire were embedded, side by side, in an insulating medium, and were connected to a galvanometer; the whole was placed between the poles of a powerful electro-magnet in a small beaker, which could be filled with any liquid. The end of one piece of wire was bared of insulation and filed to a point; the other piece was laid bare on a portion of its side. Various liquids capable of acting chemically on iron were tried, nitric acid giving the best results. The experiments tend to show that the magnetic field has a protective action on the iron, this being more especially the case when the attacking liquid does not evolve hydrogen. The most complete protection occurs where the iron has points from which the lines of force can radiate. Thus, if there is a particle of iron rather harder than the surrounding ones, this will be less attacked, and will result in a little elevation from which the lines of force diverge, still further shielding it, so as to produce ultimately a small ridge. Nickel and cobalt show the same phenomenon, but in a less egree than iron.

## A. VOLLER—EARTHING LIGHTNING CONDUCTORS BY MEANS OF GAS AND WATER PIPES.

*(Elektrotechnische Zeitschrift, Vol. 20, p. 473, 1888.)*

It is generally assumed that the path of the discharge follows only the line of least resistance, and no attention has been paid to the fact, on which the author insists, that the direction of the discharge is chiefly influenced by the state of electric potential of the buildings in closest proximity to the charged cloud. The better the connection of the metallic masses in buildings is with the earth, the higher will be the potential of the induced electricity, and the greater likelihood is there of a discharge taking place between the cloud and the points in question. Since the general introduction of gas and water pipes into our houses, it is these which offer the least resistance between the roofs and earth. Hence, if a charged cloud should pass over such a house, the gas and water pipes must be at a higher potential, and there is much greater probability of the lightning entering the house through them than at any other point. In other words, it is more likely that the discharge will take place through the pipes than through the lightning conductor; and if the lightning rod is not connected to the pipes, the discharge will find its way somehow to the latter, causing destruction in its path.

At the request of the Hamburg Fire Insurance Companies, the author undertook to inspect cases of lightning-strokes, and to ascertain the point struck, as well as the path followed. A great many interesting cases investigated are duly recorded, but some general results only can be reproduced. It generally happened that, when the building struck was unprovided with a lightning conductor, the lightning struck some part of the roof or walls, found its way to the gas and water pipes, and then passed harmlessly to earth. In the few cases where lightning struck a building fitted with a lightning conductor, the discharge jumped over from the conductor to the pipes. In fifteen cases which were specially investigated in the years 1884 and 1888, after the lightning had done more or less damage at the point where it struck, and in the immediate neighbourhood, it was found that in nine cases the discharge made its way to earth through the water pipes, in two cases through the gas pipes, in two cases through rain pipes, in one case probably through the lightning conductor of a telephone line on the next house, and in one case through an iron crane. In all cases where the pipes were the conductors, the path of the discharge could be clearly traced up to them, and then ceased. One of the cases of discharge through the gas pipes occurred in an ordinary dwelling-house provided with a lightning conductor, from which the discharge had passed over a distance of about 2 metres to the pipes; subsequent tests showed that the conductor-earth had a resistance of 138 ohms. In no case was any damage done to the pipes by the discharge occurring through them.

# LIST OF ARTICLES

RELATING TO

# ELECTRICITY AND MAGNETISM,

**Appearing in some of the principal Technical Journals during the month of JANUARY, 1889.**

## I.—BATTERIES AND ACCUMULATORS.

A. Voller—E.M.F. of Fleming's Standard Daniell Cell.—*Beiblätter*, vol. 13, p. 47, 1889.

Anon.—Sharf's Gas Battery.—*Lum. El.*, vol. 31, p. 79, 1889.

Anon.—Wright's Dry Battery.—*Lum. El.*, vol. 31, p. 137, 1889.

A. de Waltenhofen—The Accumulators of Farbaky and Schenek.—*Lum. El.*, vol. 31, p. 142, 1889.

C. Heim—Use of Accumulators in Telegraphy.—*El. Zeit.*, vol. 10, p. 41, 1889.

## II.—DYNAMOS AND MOTORS.

F. Larroque—Ancient Machines for Producing Electricity.—*Lum. El.*, vol. 31, p. 27, 1889.

W. C. Rechniewski—Changes in the Magnetic Flux in Alternate-Current Machines.—*Lum. El.*, vol. 31, p. 101, 1889.

G. Richard—Details of Dynamo Construction.—*Lum. El.*, vol. 31, p. 120, 1889.

O. H. Schmöller—Construction of Dynamos to obtain Best Results.—*El. Zeit.*, vol. 10, p. 34, 1889.

## III.—ELECTRO-CHEMISTRY AND ELECTRO-METALLURGY.

F. Quincke—Electrolysis of Copper Chloride.—*Annalen*, vol. 36, p. 270, 1889.

E. Drechsel—Electrolysis of Phenol by Alternate Currents.—*Beiblätter*, vol. 13, p. 27, 1889.

E. Drechsel—Electrolysis by means of Alternate Currents.—*Beiblätter*, vol. 13, p. 28, 1889.

P. H. Ledeboer—Bleaching by Electricity.—*Lum. El.*, vol. 31, p. 151, 1889.

W. de Fonvielle—Jacobi's Invention of Electro-Deposition.—*Lum. El.*, vol. 31, p. 189, 1889.

## IV.—ELECTRIC LIGHT.

A. Palaz—Unit of Light and Photometric Standards.—*Lum. El.*, vol. 31, p. 109, 1889.

Anon.—Portable Electric Light Plant.—*Lum. El.*, vol. 31, p. 131, 1889.

Anon.—Installation at the Theatre at Prague.—*Lum. El.*, vol. 31, p. 134, 1889.

Anon.—Installations in Germany by Schuckert & Co.—*El. Zeit.*, vol. 10, p. 21, 1889.

Cance—Installation at the Bon Marché.—*Bull. Soc. Int. des Elect.*, vol. 6, p. 11, 1889.

## V.—ELECTRIC POWER.

Anon.—Electric Tramways at Hamburg.—*Lum. El.*, vol. 31, p. 178, 1889.

A. du Bois-Reymond—Difficulties attending the Transmission of Power by means of Alternate Currents.—*El. Zeit.*, vol. 10, p. 1, 1889.

## VI.—MAGNETISM AND ELECTRO-MAGNETISM.

E. H. Hall—Action of Magnetic Force on the Equipotential Lines of an Electric Current.—*Beiblätter*, vol. 13, p. 32, 1889.

L. Külp—Experiments on Magnetic Coercive Force.—*Beiblätter*, vol. 13, p. 34, 1889.

C. Reignier—Magnetic Induction of Iron.—*Lum. El.*, vol. 31, p. 170, 1889.

## VII.—MEASUREMENTS AND MEASURING INSTRUMENTS.

Dr. J. A. Fleming—New Form of Standard of Electrical Resistance.—*Phil. Mag.*, vol. 27, p. 24, 1889.

F. J. Smith—Continuous Heat and Electric-Current Measuring Instrument.—*Phil. Mag.*, vol. 27, p. 28, 1889.

J. W. W. Waghorn.—Determination of Electro-magnetic Capacity.—*Phil. Mag.*, vol. 27, p. 69, 1889.

E. Dorn—Determination of the Ohm.—*Annalen*, vol. 36, p. 22, 1889.

Bellati and Lussana—Passage of Electric Currents through Bad Contacts.—*Beiblätter*, vol. 13, p. 21, 1889.

A. D'Arsonval—Universal Dead-Beat Galvanometer.—*Lum. El.*, vol. 31, p. 13, 1889; *Bull. Soc. Int. des Elect.*, vol. 6, p. 6, 1889.

Becker, Lahmeyer, and Picard—Photographic Records of Electric Currents.—*Lum. El.*, vol. 31, p. 16, 1889.

A. Paalzow—Measurement of the Resistance of a Conductor.—*Lum. El.*, vol. 31, p. 30, 1889.

Kipp and Zonen—Electro-Dynamometer for Measuring Telephonic Currents.—*Lum. El.*, vol. 31, p. 31, 1889.

C. Decharme—Some New Forms of Galvanometers.—*Lum. El.*, vol. 31, pp. 70 and 127, 1889.

Anon.—Edison's Electrolytic Meter.—*Lum. El.*, vol. 31, p. 180, 1889.

P. Nipkow—Electro-Dynamometer for Alternate Currents.—*El. Zeit.*, vol. 10, p. 28, 1889.

## VIII.—RAILWAY APPLIANCES.

E. Zetsche—Couplers for Conductors on Trains.—*Lum. El.*, vol. 31, p. 159, 1889.

M. Cossmann—Metzger's Automatic Signalling Apparatus.—*Lum. El.*, vol. 31, p. 164, 1889.

Anon.—Siemens & Halske's Alarm Signal for Level Crossings.—*El. Zeit.*, vol. 10, p. 39, 1889.

## IX.—STATIC AND ATMOSPHERIC ELECTRICITY.

Edlund—Some Theories of Atmospheric Electricity.—*Beiblätter*, vol. 13, p. 48, 1889.

A. PALAZ—Construction of Lightning Conductors.—*Lum. El.*, vol. 31, p. 58, 1889.

C WEYHER—Cyclones and Waterspouts.—*Lum. El.*, vol. 31, p. 75, 1889.

## X.—TELEGRAPHY AND TELEPHONY.

E. ZETSCHE—Domestic Telephony.—*Lum. El.*, vol. 31, p. 28, 1889.

ANON.—Use of Telephonic Conductors for Synchronising Clocks.—*Lum. El.*, vol. 31, p. 32, 1889.

SCHAEFFER—New Iron Telegraph Post.—*Lum. El.*, vol. 31, p. 38, 1889.

F. LARROQUE—Permanent Effects produced in Copper Conductors by a Long-continued Passage of a Current.—*Lum. El.*, vol. 31, p. 161, 1889.

B. PESCH—Automatic Call and Conclusion Signal for Telephone Exchanges. —*El. Zeit.*, vol. 10, p. 12, 1889.

T. D. LOCKWOOD—Apparatus for Diminishing Induction.—*El. Zeit.*, vol. 10, p. 14, 1889.

ANON.—The Swiss Telephone System.—*El. Zeit.*, vol. 10, p. 46, 1889.

Dr. S. S. WHEELER—Overhead and Underground Conductors in New York.—*Bull. Soc. Int. des Elect.*, vol. 6, p. 22, 1889.

## XI.—THEORY.

O. HEAVISIDE—Maxwell's Electro-magnetic Equations in a Homogeneous Isotropic Medium.—*Phil. Mag.*, vol. 27, p. 29, 1889.

Prof. J. V. JONES—Calculation of the Coefficient of Mutual Induction of a Circle and a Coaxial Helix.—*Phil. Mag.*, vol. 27, p. 56, 1889.

J. PARKER—Thermo-electric Phenomenon.—*Phil. Mag.*, vol. 27, p. 72, 1889.

H. HERTZ—The Forces of Electric Oscillations.—*Annalen*, vol. 36, p. 1, 1889.

W. G. HANKEL—The Electro-dynamic Law—*Annalen*, vol. 36, p. 73, 1889.

G. P. GRIMALDI—Effect of Annealing on the Thermo-electric Properties of Bismuth.—*Beiblätter*, vol. 13, p. 25, 1889.

J. L. LORET—Action of Electricity on Steam.—*Beiblätter*, vol. 13, p. 37, 1889.

C. E. GUILLAUME—Notation and Symbols.—*Lum. El.*, vol. 31, p. 10, 1889.

J. and P. CURIE—Electric Expansion.—*Lum. El.*, vol. 31, p. 60, 1889.

## XII.—VARIOUS APPLIANCES.

W. DE FONVIELLE—Applications of Electrolysis in Operative Surgery.—*Lum. El.*, vol. 31, p. 48, 1889.

G. RICHARD—Some Mechanical Applications of Electricity.—*Lum. El.*, vol. 31 p. 54, 1889.

ANON.—Electric Mine-Exploders.—*Lum. El.*, vol. 31, p. 76, 1889.

M. MONTPELLIER—Fire-Alarms.—*Lum. El.*, vol. 31, p. 78, 1889.

W. DE FONVIELLE—Death by Electricity.—*Lum. El.*, vol. 31, p. 90, 1889.

P. SAMUEL—Maximum Thermometer.—*Lum. El.*, vol. 31, p. 169, 1889.

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*An Index, compiled by the late Librarian, to the first ten volumes of the Journal can be had on application to the Secretary, or to Messrs. E. and F. N. Spon, 125, Strand, W.C. Price Two Shillings and Sixpence.*

# JOURNAL

OF THE

## Institution of Electrical Engineers.

*Founded* 1871. *Incorporated* 1883.

Vol. XVIII. 1889. No. 78.

The One Hundred and Eighty-fifth Ordinary General Meeting of the Institution was held at the Institution of Civil Engineers, 25, Great George Street, Westminster, on Thursday evening, February 14th, 1889—Dr. JOHN HOPKINSON, M.A., F.R.S., Vice-President, in the Chair.

The minutes of the Ordinary General Meeting held on January 24th, 1889, were read and approved.

The names of new candidates for election into the Institution were announced and ordered to be suspended.

The following transfers were announced as having been approved by the Council:—

From the class of Students to the class of Associates—

Charles Alfred Baker. | Frederick Bathurst.
Gustav H. C. Risch, jun.

Donations to the Library were announced as having been received since the last meeting from Mr. Charles Todd, C.M.G. (Local Hon. Sec. for South Australia), and Mr. A. C. Swinton, Associate, to whom the thanks of the meeting were duly accorded.

The CHAIRMAN: We will now resume the discussion on Professor Jamieson's paper on "The Insulation of Electric Light

"Installations." A letter has been received from Professor Jamieson, which the Secretary will read.

The SECRETARY: I have received a letter from Professor Jamieson, which he has requested me to read to the meeting before the discussion is resumed, as it has some bearing on the paper. It is as follows:—

*February* 12*th*, 1889.

DEAR SIR,—Since the Council have given a further opportunity for discussing my paper on "The Insulation Resistance of Electric Lighting Circuits," I would like if you would kindly point out to the meeting, on my behalf, that in my paper I intentionally confined my remarks to *insulation resistance*, and to the rules as well as methods of testing for the same, as far as they were known to me. I purposely avoided saying anything about *insulation*, or the different kinds of *insulating materials*, or the different ways in which they are made and erected, &c. To have done so would have defeated the object of the paper, viz., to find out what is the *minimum* insulation resistance which we may with safety allow an installation to have without considering it below par, and what are the simplest and best methods of ascertaining the insulation resistance of the various circuits *before* and after the installation has been set to work.

If I had treated on insulation and insulating materials, &c., I might have written a treatise on the subject and not have exhausted it, and the discussion could easily have been continued for many nights without covering the whole ground or coming to a decision.

In regard to testing, there seems to be a popular belief that it is necessary to employ as high an E.M.F. as the working current when ACTUALLY *ascertaining* the insulation resistance. This is not the case, as old submarine cable electricians know very well. If you have a sensitive mirror galvanometer and a few ordinary chloride of silver or Leclanché cells, you will be able to tell quite as well as if you had 100 or more cells and a less sensitive galvanometer, what the real resistance is. Of course, if you wish to submit the core or cable to an electrical stress equal to or greater than that of the working current, you may do so, but you would not keep your testing galvanometer and resistance coils in circuit at the time of applying this stress?

What I maintain is this—that we can ascertain the actual insulation resistance in ohms, or the position of a fault, with great accuracy and ease by using only a few cells and a sensitive mirror galvanometer. I have never used anything else than a mirror galvanometer with which to take the insulation resistance of electric light circuits, and I have employed it in the most awkward positions, such as cable-huts, engine-rooms in ships and on shore, bunks, cellars, stores, &c. A couple of ordinary chairs serves, the one to support the galvanometer and the other the scale. Galvanometer, lamp, scale, and key all go into one small case, and the whole can be carried by the little finger, and swung round the head at arm's length without affecting the fine cocoon silk suspension! In five minutes after arrival at the spot where the tests have to be taken I have got a spot, and in two minutes after I have got

the resistance the whole is packed and ready to be taken away. If you wish a sketch, I will send one for the Journal.

I am, dear Sir,

Yours faithfully,

ANDREW JAMIESON.

F. H. WEBB, Esq., Secretary.

The SECRETARY: I have received a short communication from Mr. Smellie, who is the electrician for Messrs. Denny & Co., and who was referred to in Professor Jamieson's paper. He regrets he could not come to town to take part in the discussion, and writes as follows:—

"*Re* PROFESSOR JAMIESON'S PAPER—INSULATION RESISTANCE.

"The instrument used by me, and referred to by Professor "Jamieson, is not quite correctly described.

"It consisted of one of the small hand-magnet generators used "by the telephone companies, in conjunction with a small vibrating "needle 'tell-tale' used in Wheatstone A B C exchanges before "the advent of telephones, serving to show the operator when "two lines were speaking; and was much more sensitive than "any polarised bell I have yet seen. Since then it has been in "constant use, and although the magnets are weaker than they "were at first, it is still capable of ringing through 16,000 $\omega$.

"The value of an instrument of this description can only be "appreciated by those who have been accustomed to use a "detector galvanometer and battery, which gets out of con- "dition or is upset, often when most needed.

"Regarding the total insulation resistance of E.L. systems, "I would suggest that the I.R. should be given *per lamp*, as a "means of comparison between installations of different sizes.

"At the time the Professor refers to I was obtaining from "30 to 35 $\Omega$ per light, but since then have got as high as 95 $\Omega$ "on board ship. With the same men and quality of material, "the I.R. of installations on shore far exceeds this.

"The value of a test taken with a low battery E.M.F. and "reflecting galvanometer varies with the dew point, and raises "considerable difficulty in making a fair comparison of any two "installations.

"However satisfactory such a test would be with a submerged "cable, whose conditions are constant, it is by no means suitable "for a system of electric light leads that varies from day to day "according to atmospheric conditions, and thereby renders all "tests made in this manner useless for comparison.

"Being dissatisfied with this method, I have made it a "practice to use a 'breakdown' E.M.F. of 700 volts on installa- "tions for 100-volt lamps: this gives a factor of safety of seven, "and the high potential quickly decomposes the moisture which "may exist, without sensibly heating the conductors.

"The total leakage may then be measured, and the total I.R. "calculated therefrom.

"Contractors might demur at this being insisted on, on "account of a sufficiently high potential current being unobtain- "able; but this is easily got over by using a small *constant* "*current transformer* having a ratio of 7 to 1, and *excited by the* "*dynamo for the installation.*

"A test of this kind gives a satisfaction which no other "method can, and I feel sure would assist in regaining the "public confidence, which has been sadly shaken from time to "time through failures caused by indifferent workmanship,—not "due to the indifference of contractors, but to workmen without "a conscience."

The CHAIRMAN: Mr. W. H. Preece will perhaps resume the discussion?

Mr. Preece. Mr. W. H. PREECE: I should like to make one or two remarks on this paper from the user's point of view. Something was said in the letter which has just been read from Professor Jamieson from the contractor's point of view, but it is a very different thing indeed when this question is looked at from the result of the experience of a few months' or a few years' working. One of the most serious evils that followed the introduction of the electric light into England was the indifferent work that accompanied it in the early days, and the very imperfect material used for the purpose. I have been told myself, by one of our old manufacturers, that he has absolutely refused to execute orders that have been put into his hands for very indifferent wire

intended for electric light installations; and I am quite certain that the first rule that is to be followed in the installation of any electric light equipment, and in instituting any regulation to determine the insulation resistance of that equipment, is to specify that none but the very best materials shall be used. Mr. Preece.

Now the conditions that determine the quality of materials, and the tests that have to be made upon those materials in electric light and transmission of power installations, is a very different thing indeed to that which determines the installation of telegraphs and telephones. In an electric light installation the conductors, during the time the light is going, are under a constant electro-motive force; and where the smallest leak is present under the existence of moisture and of a constant electro-motive force, the breakdown of the wire is simply a question of time. I have been astounded to find the quickness with which electric light currents ferret out a fault and destroy the wire. There is no place where faults occur so much, for instance, as in stables, where ammoniacal vapours are constantly about, which assist in the rapid destruction of conductors. When there is the smallest leak, and a current constantly passing through that leak, electrolytic action is set up, nascent oxygen—or, rather, ozone—is produced at the point of leakage, and ozone, as it is produced, has a destructive influence very similar to burning on organic substances. When you examine a conductor that has been broken down in this way, it is, for all the world, exactly like the result of a flash of lightning. I have often heard it said that underground wires and wires of different kinds have been broken down by lightning, because they bore the distinct marks of burning, of something very likely to happen with a flash of lightning; but I have been able to find, in many instances, that it has been really due, not to flashes of lightning, but to the development of ozone at the leakage, and to the eating away and destruction of the hydro-carbon that, as a rule, forms the dielectric. Therefore it is that faults of this class are developed so very rapidly under the influence of electric light installations, and so it is that it is most essential that we should exercise great care in the selection of our materials, and in their

Mr. Preece. being tested before they are erected. Then, again, we must remember this—that in all electric light installations there are a number of positions, a number of localities, that are themselves identical with faults, such as switch-boards, the dynamo itself, cut-outs, and fixings; also the frames of arc lamps, and, worst of all, gas fixtures. It follows from this that with an installation practically full of faults at all points there must be a distinct rule for deciding what shall be the *minimum insulation* resistance to be given to the installation under the worst circumstances; and I take it that the principal point of Professor Jamieson's paper that he has brought strongly before us is his own personal view of what should be the minimum resistance that under any circumstances should be allowed.

We tried to battle with this question when the Committee of the Society of Telegraph Engineers discussed the question of fire risks, and a rule was established, which was defended on the last occasion by Mr. Crompton, and who said—with which I fully agree—that now with our present experience we might rather enlarge that rule, and instead of defining the leakage that should be admitted as being 1-5,000th of the maximum output of the installation, we might very well raise it to 1-10,000th part. If we did that, we should bring the leakage allowable more near to the law and formula of Professor Jamieson.

This subject, although it has not been received with quite the feeling that one would have expected from so many engineers who are associated not only with the maintenance but with the construction of these installations, has met with a considerable amount of attention in America. There they are far ahead of us in the practical development of electric lighting; there the absolute necessity of having some rule to determine the minimum insulation resistance has forced itself forward, and I think it was there that Mr. Leonard was the first to initiate a rule that has been the basis of several others. His law was that the insulation resistance ($R_1$) should be equivalent to K $\frac{E}{C}$, K being a constant multiplied into a fraction having the electro-motive force as its numerator and the

current as its denominator. That formula has been taken up by M. Picou in France, and it is the basis of the formula that Professor Jamieson has given us; but Professor Jamieson has very properly replaced the denominator C for current by $n$ for the number of lamps. The current may be constant, but the lamps may be in series, and therefore vary. Take, for instance, a Brush arc circuit with 60 lamps: the current is constant; the number of volts does not depend upon the current in any way, but upon the number of lamps. Hence the denominator should be $n$, the number of lamps, and not the current flowing. There is another element that enters into this question that, I think, has not been touched upon by Professor Jamieson, and a very important one it is. Professor Jamieson's paper has been written from the point of view of examining the insulation resistance of an indoor installation; and if simply an indoor installation be considered, then probably that formula will answer very well. But it is not indoor installations that are going to give us trouble; the trouble is going to arise from exterior installations. We are going to see great advances in electric lighting; we shall see in the next two years, say, nearly every town of any consequence in the United Kingdom taking up the subject of electric lighting. The whole atmosphere is now simply redolent with electric lighting, and it only wants the purse-strings of our London capitalists to be undone to find every town in this country swarming with electric light engineers; and when that is the case—which will be the case in a very few months' time—it will not be alone the insulation of installations of the interior of buildings that we shall have to think about, but it will be those circuits that will be carried round about our towns, and our sparsely inhabited villages and places, where insulation will be a matter of very serious consequence—insulation not alone for the safety of the electric circuit, but also for the safety of the public—and it is here where a rule of some kind is wanted. It is in outdoor circuits where really some rule is necessary. Those who have studied Provisional Orders, and those who have studied the Acts of Parliament, well know that it will rest upon the local authorities to decide what shall be a defective main or what shall be a defective Mr. Preece.

Mr. Preece. conductor; and how is it possible for these authorities, scattered all over the country, to decide what is a defective main or what is a defective lead, if we, the Institution of Electrical Engineers, are not able to define some law that shall decide the difference between a bad and a good conductor? I think that this formula is the basis of a very good law. I think that it is perfectly certain that the insulation resistance must vary directly with the electro-motive force. We have installations of 50 volts, of 100 volts, of 2,500 volts, and in one instance it is proposed to go as high even as 10,000 volts, and it is perfectly clear that the insulation resistance of an installation must increase directly with the electro-motive force. Then, again, it must depend upon the number of lamps, for every lamp is a fault. Again, especially with aerial wires, it must vary with the length of the circuit, and particularly so in the case of those circuits that are used for high pressure—arc circuits. I have a case in my mind's eye at the present moment where we shall have to light up a series of rows and streets where the circuit must be twenty miles long. It is very evident that with a circuit twenty miles long you must have a constant that will give you the same insulation for the whole circuit as for one mile long; so that I say for such circuits we must introduce in this denominator the function of length. The lamps should be represented by N + 1, the length of the circuit by L. I say N + 1, because if we gave N nothing the formula would be absurd, but by making it N + 1 we deal with all cases. The formula would therefore be

$$R_1 = K \frac{E}{L\,(N + 1)}.$$

Now, then, we have only to determine what the coefficient K shall be. Professor Jamieson says it should be 100,000. In America, for a somewhat similar formula, they have made it as low as 1,000; but on this point it is really a question of experience, and I would ask every electrical engineer present who has an electric light installation under his charge to measure the insulation resistance, and to transmit the results to the Secretary for publication in the Journal of Proceedings. If he does so, he will give us the oppor-

tunity of being able to say to local authorities, "Now there is "a formula which you have to follow: the coefficient K shall be "one thing for high-pressure arc circuits, it shall be another "thing for alternate-current circuits, it shall be a third thing "for low-pressure circuits;" and we can make it agree with every condition that may arise. Speaking from experience in the Post Office, and in telegraphs generally, we have absolutely neglected the testing of interior installations, for we never meet with faults there. We do not require to go wandering about the country with a mirror galvanometer that you can "swing "round your head by the aid of your little finger;" we have instruments—galvanometers—of real practical character, that will enable us to see whether the wires are good or whether they are bad. In electric light installations, or any other installations, if wires are properly tested before they are erected, and if they are occasionally tested to see that no faults exist, I am inclined to think that our duty as electrical engineers is fulfilled; but what we really do want is this—that we shall be able to submit to local authorities, and to such powers as the Board of Trade, some formula that shall enable them to decide what the insulation shall be for circuits when used for arc lighting under high pressure, for glow lamps under low pressure, or for alternate systems under pressures varying from 1,000 to 10,000 volts; and we can only arrive at the proper value of the coefficient K if we are assisted by all those electrical engineers who have installations under their charge and will send to us the results of their measurements. I ought to say this—that on searching through my papers to find any illustration of a test, I have only come across one, and that is the Post Office at Glasgow, which has been lighted by electricity for the last four or five years. Recently we had a fire on the roof there—a pure accident, nothing to do with the electric lighting, although it was in the electric light leads; but we took that opportunity to test the installation, and the report of an extremely able electrician—Mr. Boldchild, of Glasgow—says: "I tested the whole of the glow system, including "all switches and cut-outs, using 50 bichromate cells [that would "be 100 volts], and found the total resistance to earth to be Mr. Preece.

Mr. Preece. "about 56,000 ohms; the resistance between the leads was "100,000 ohms, all the lamps being in and the switches open. "The arc leads showed nothing with a full coil of the tangent "galvanometer. When I tested in October, 1886, one lead gave "60,000 ohms resistance, and the other only 1,500; this proved "to be a defect, which was immediately removed. My opinion is "that we should have no difficulty in maintaining an installation "of from 50,000 to 60,000 ohms, even with the present crude "workmanship." Of course he is speaking of the workmanship in Glasgow. I mention that as being just what we really want: we want to have measurements of the insulation resistance, not only of circuits in buildings, but especially circuits working under high pressure, and there are now a good many of them about the country.

Mr. Kempe. Mr. H. R. Kempe: Sir, I think that Professor Jamieson has made a most important mistake in not distinguishing between the insulation of the cables and the insulation of the fittings in an electric light installation. The leakage in a complete installation is due to loss in the fittings, or to loss in the cable, or to both. The former will be chiefly, if not wholly, due to surface leakage, the latter to leakage through the dielectric. Now the result of leakage in the case of fittings would probably be to some extent to improve the insulation, owing to the decomposition of the moisture which would consequently pass off; but this is certainly not the case if the leakage is due to a defect in the cables. In the case of the cable the circumstances are totally different: a current would undoubtedly open a fault if it existed, and in time would certainly break it down, even if the fault be exceedingly minute at first. It is not sufficient for the insulation of a cable, if the dielectric is of any thickness, to be only a few megohms; it ought to be several hundred, if not several thousand, megohms; for if that is not the case, the low insulation, although it may be as high as several megohms, is almost sure to be due to a defect, which defect would go from bad to worse, sooner or later, under the influence of a strong current. This, I think, is an established fact, and is well known to all who have had to deal with cable cores, or

with insulated wires. The standard of insulation of a wire—of an ordinary cable core—perhaps may be as high as 500 megohms per mile. Now, if it runs down to, we will say, 100 megohms, that insulation, it is true, would be perfectly sufficient to prevent any leakage of the current, provided it remained constant; but a fall of from 500 to 100 megohms would certainly be due to a slight defect, and a current, even of as low a potential as 100 volts, would be almost sure in a short time to completely break the fault down. Of course a defect like that would be likely to cause rather serious results, because it is quite possible that the current might arc across and set the insulation on fire. It is a well-known fact in telegraph work that an insulated underground wire, if its resistance does not considerably exceed a few megohms, is sure to go bad sooner or later, although the insulation of a few megohms would be quite sufficient to enable perfect signals to be carried through it. There is another point. No one engaged in telegraph work would ever think of testing an underground wire with the instruments in circuit. The instruments would be taken off, and the wire would be tested by itself; because we should know that if we got a low result—a result equal to, say, 10 or 12 megohms—it might be entirely due to the leakage over the instruments, and not due to leakage on the cable. It is a most important point that in testing cables or insulated wires they should be perfectly free. I recollect, some time ago, having to test thirty or forty underground wires, having each a length of about 1½ miles. The insulation came out, with all the wires on the test box, about 12 megohms per mile all round. That insulation, of course, was quite good enough for working purposes; but I knew perfectly well that the resistances of good underground wires could not possibly be as low as that, so I had the wires taken off the test box, and tested the insulation resistances of the terminals alone, and the results came out exactly the same as they were before the wires were taken off; but the wires tested off the test box gave between 200 and 300 megohms per mile. That showed that the test with the wires on the box was perfectly useless. Mr. Kempe.

Mr. Kempe. In the letter from Professor Jamieson which the Secretary has just read, it is stated that it was not of importance that the insulation resistance of an insulated wire should be taken with high battery power. As regards that point I am totally at issue with Professor Jamieson. For the last fifteen years I have had a great deal of experience in testing several thousand miles of wire, and my experience tells me exactly the contrary; and this is especially the case with india-rubber insulation. You may test an india-rubber cable, and the result may apparently be perfectly satisfactory if the test is taken with, say, 20 or 30 cells: the insulation will be extremely high, and the electrification, which is considered to be an indication of the soundness of a cable, may be perfectly steady. Suppose 300 cells are used, you may find that after two or three minutes the deflection on the galvanometer scale will become unsteady, and it will finally go off the scale; in fact, a fault will develop, and the insulation run down to something exceedingly small. I therefore consider that it is very necessary in an electric light installation that the testing of the cables should be taken quite independently of the fittings, and that a certain standard should be taken for the fittings themselves, and another standard—which would be very, very much higher—for each of the cables tested independently of the fittings and of the test box with which they may be connected.

Mr. Walker. Mr. Sidney F. Walker: I should just like to make a few remarks from the point of view of the dynamo manufacturer and the contractor; and as a preliminary I should like to say that I agree with Mr. Preece that the work and the conditions of an electric lighting circuit are quite different to those of a telegraph circuit, and that the rules which would apply to the testing of telegraph circuits will only apply to electric lighting circuits in a minor degree: we have larger currents as a rule, and higher electro-motive force always at work. I think that the thanks of electric lighting contractors are due to Professor Jamieson for bringing this matter forward, and for attempting to fix a standard of insulation. What we want more than anything is a standard of insulation. What most contractors are troubled with when we tender for electric lighting work is that those of us who have all

the conditions of work before us tender for a high standard, while somebody else, with a light heart, tenders for a low standard, and gets the contract. Of course that leads, as Mr. Preece has said, to disgrace. Electrical engineers are troubled at the present time, as they have been in the whole matter of electrical work, more with men going into the thing without being properly trained, and without proper knowledge, than from anything else. One striking point about the paper, though, I notice, is the variation of the standard. Professor Jamieson gives his own standard as 1-10,000th of the current; that of the Society of Telegraph Engineers is 1-5,000th; Mr. Preece has just told us that the Americans take 1-1,000th; and I understand that M. Picou takes 1-500th, if I understand it rightly. If I may make a suggestion, I think that we ought to have an authoritative standard. I certainly agree with the author, and with the remarks of others, that within certain limits you can hardly have the standard too high. It is a simple matter, comparatively, to get a high standard of insulation at the start, but it is by no means a simple matter to keep it up. My work happens to be principally what Mr. Preece has mentioned as outdoor work. The greater portion of my work has been amongst collieries, where we have considerable difficulty in keeping up insulation; and I have no doubt that there are places, even indoors, where there is considerable difficulty in keeping up insulation. I have places at collieries, inside buildings, where cables are liable to a considerable amount of wet; in pit-shafts, again, a cable must stand a stream of water, often impregnated with the salts of different metals. Now I have found no insulating substance yet which will stand a stream of water, if it is allowed to get to the insulating substance itself, except gutta-percha, and that only if there is plenty of it—if it is a good thickness. I hope that there are some cable manufacturers here, because I understand that cable manufacturers object to make electric lighting cables with gutta-percha covering, I presume on account of the heating effect of the current. But I feel sure that there are many places where only gutta-percha will stand. India-rubber and Callender's core will stand if you stop the wet getting to them at all; otherwise they will not stand. India- Mr. Walker.

Mr. Walker. rubber—vulcanised rubber—may start at a thousand megohms, or as many megohms as ever you like, but it will not keep it up; it is only a question of time. As has been pointed out in the paper, and by Mr. Preece, one of the main difficulties in maintaining insulation of electric light circuits is the action of the current itself. The current itself passing across the cable—even the very small current that is going—deteriorates it; and not only that, it also deteriorates the fittings. I should differ from Mr. Kempe in that respect. The effect of a current passing across fittings, especially if they are wooden fittings, is not only to dry up the moisture in the fitting, but also to carbonise the insulation of the fitting itself, and eventually sparks pass across and something very like a lightning flash occurs.

With regard to the testing, I certainly agree with those speakers who have said that for electric light work you must test with a high voltage—as high as ever you can get it. You cannot take a mirror galvanometer and a set of delicate testing instruments into the majority of places where an electric light installation exists. Professor Jamieson is apparently very lucky with his apparatus, but the best apparatus for rough-and-ready testing is a well-constructed detector galvanometer with a high voltage behind it. I am bound to say that I join with those who view the high tensions which are about to be used in London with considerable apprehension. I certainly admire the skill which has been shown in arranging the conductors for the Deptford circuit, which are to stand 10,000 volts; but I shall be agreeably surprised if that insulating substance is not gradually deteriorated till in some part, where possibly there is a small bend or some slight difference in thickness, a spark passes across, and then the whole thing will break down. I will end by saying that I hope a standard will be arrived at that we can all work to.

Professor Ayrton. Professor W. E. AYRTON: It will be observed that all these rules quoted by Professor Jamieson as to what the insulation resistance should be, really lead to the result that the insulation resistance should be some definite multiple of the conductor resistance of the cable, viz., $\frac{E}{C}$; and it really comes to the

question, What multiple of the conductor resistance of the cable should be specified as being the minimum insulation resistance allowable? Mr. Preece's formula, however, differs from all of these rules in character, and not merely in the value of the coefficient, for he would make, according to his formula, the insulation resistance diminish more rapidly than the length, if there were many lamps. In fact, if you assume that the number of lamps in circuit is about proportional to the length of the cable, he would make the resistance vary as the inverse square of the length; *i.e.*, the resistance would fall off very rapidly for long conductors. I can hardly think that that can be the right way of specifying the resistance, because it would make the resistance much too low a value for a long conductor. Last time there was a sort of feeling that this question of specifying what the insulation resistance should be for an electric light circuit should be left to personal judgment; but that, of course, is not possible under the particular circumstances of the case. In the matter of submarine cables it is a mere question of a compact between the contractor and the buyer of the cable: the buyer is willing to pay so much, and the contractor is willing to give so much insulation, among other things, for that sum of money; and as long as those two agree how much insulation is to be given on the one hand, and how much money is to be paid on the other hand, I defy anybody to enter between the two and say, "You "shall not make that compact." In the case of an electric light circuit there is, fortunately or unfortunately, a third party to be considered, and that is the Board of Trade or the Insurance Office; and there comes the difficulty. I may say I wish my house fitted up by a certain firm; I have only a certain amount of money to spend, and I want to get as much insulation as I can for it; but the Insurance Company steps in and says, "No, we shall not be satisfied with that; you must pay "more and get better insulation." Of course there is a very valid reason for their doing this—on account of safety—and therefore it becomes necessary to settle some sort of standard; it becomes all-important, as Mr. Kempe has pointed out, to see in what way that standard can be settled. I quite agree with him

Professor Ayrton.

Professor Ayrton.

that it is a mistake, if you are going to settle a standard which is to bind the English world generally, to mix up the insulation of the fittings with the insulation of the cable, especially when dealing with long lengths of cable. The same sort of mistake that Mr. Kempe has pointed out I happened to point out nearly twenty years ago. At that time a specification in connection with telegraph insulators was drawn up by a very eminent electrician in England for the Indian Government Telegraph Department, and I, as a not eminent electrician, on leave in England, was desired to work under that specification. The specification was to the effect that no telegraph insulator, tested in the usual way, should have a less resistance than so many thousands of megohms; but, for convenience, the insulators should be joined up in hundreds and should be tested in parallel, and if the resistance of the 100 exceeded one-hundredth of the specified resistance per insulator, then I was to pass them. Well, I pointed out that that was wrong, and in fact I said that until the specification was changed I could not work under it, for the simple reason that it might be that the 100 insulators would have a higher resistance than one-hundredth part of the specified insulation, and yet the 100 might contain one or more very bad insulators. For example, 99 of the insulators might have far more than the specified resistance, but one insulator might have a good deal less, and the 100 might still have a higher resistance than the one-hundredth of the specified resistance per insulator. I am happy to say (and that enables me to correct a wrong impression which I see one remark I made last time about contractors seems to have produced) that the contractors, who were Messrs. Siemens Bros., thoroughly fell in with the objection I raised, and they were quite willing to allow me to test to a much higher standard than they had arranged to supply insulators to for the Indian Government; indeed, they allowed me to make an important change in the mode of testing (a change which might have been serious to themselves), because they were most anxious to supply good material, and because they realised the point I raised that, had a few insulators a less resistance than they ought to have, their resistance would probably diminish in

time, and we should have defective telegraph lines in spite of the testing of the insulators at the works. This corroborates what Mr. Kempe has said: let the resistance of the cable plus the resistance of the slate switch-board be pretty good; then, if the leakage be in the switch-board and the cable be good, all right; but if the switch-board be much better, on a very dry day, than you would suspect, and the cable be bad, it ought to be rejected, although the resistance of the two in parallel may be very much higher than the specified resistance. If the defect be in the cable, that should be rejected. The resistance of the cable should be thousands of megohms, even on a wet day, whereas the switch-board on a wet day might have comparatively low resistance and yet not be bad.

Professor Ayrton.

There is no question as to the importance of testing with high electro-motive force, and here again I find myself quite in accord with Mr. Kempe; the importance of using high E.M.F. for testing has been realised for a number of years. Even long ago we never thought of testing telegraph insulators with a less electro-motive force than 200 volts; one never thought of putting on a few volts, but used as high electro-motive force as could be conveniently obtained. In fact, as the late Professor Fleeming Jenkin pointed out in his "Electricity," in 1873, leakage resistance is not a constant, but *diminishes* in amount as the E.M.F. employed increases. There will no doubt be exceptions to this rule where the leakage is a surface one and where the E.M.F. is sufficiently large to produce a drying-up action; but in any case, if the circuit is to be worked with a high E.M.F., it is all-important that that high E.M.F. should be used for testing the insulation. The idea of using a magneto machine was suggested (I do not know whether it was for the first time) by the late Mr. Schwendler in 1871, and out in India we had magneto machines for testing insulators. The form of galvanometer that we used was a very simple and natural one: it was our tongue. We used to whirl round the handle of the magneto, and if we could taste any of the current that got through an insulator, that insulator was temporarily rejected, to be tested more carefully. Of course we were using a large electro-motive force and

Professor Ayrton.

a very sensitive detector; the machine was made by Messrs. Siemens for the purpose, and it gave an alternate current.

With reference to the modes of testing which Professor Jamieson suggested, I would like to refer to one of them, viz., Fig. 4. He says in connection with Fig. 4: "Second, join up "the voltmeter as in Fig. 4, with one side to earth, having "previously ascertained its resistance, $r$, ohms. Also note, C, on "the ampere-meter [why not 'ammeter?' it is much shorter], "and see that it is the same as before. If any leakage to earth "exists in the dynamo or in any part of the circuit, a deflection "on the voltmeter, V M, will be produced corresponding to the "leakage current, $c$, passing through it, and is represented "by, $e$, volts difference of potential between its terminals. "Then, $\frac{e}{r} = c$, the total leakage current." Well, now, let us examine that statement. I will take the case of the lower red line in Fig. 4, having a dead earth on it—a frightfully bad state—and I will suppose that dead earth is near the dynamo, and we will join up the voltmeter as shown in Fig. 4. What will the voltmeter indicate? The voltmeter tells you, of course, the difference of potential between the point of the circuit to which it is attached and the earth. There is a dead earth on the circuit somewhere near the dynamo; the potential of the lower circuit near the dynamo is therefore 0. The voltmeter will tell you the interesting fact that there is no difference of potential between the lead and the earth: there will be no current through it; therefore the total leakage current, according to Mr. Jamieson's formula, is 0, or the line is all right. But the line is not all right; it is in a very bad state. In fact, the indication of the voltmeter depends entirely upon where the leakage happens to be, and not simply on the amount of that leakage. If the leakage is in some other part of the circuit, I grant you that the voltmeter may indicate that there is a current through it; but if the leakage happens to be near what is represented as the positive terminal of the dynamo in Fig. 4, the greater the leakage or the worse the insulation, the less will the voltmeter indicate; in fact, the voltmeter will indicate 0 when there is a dead earth on, and

therefore, obviously, the total leakage current cannot be measured as given in Fig. 4 by the deflection of the voltmeter. Professor Ayrton.

I feel interested in this particular test, because my colleague and myself, after testing a certain installation, and ascertaining that it could pass the standard required by the authorities, amused ourselves by trying this particular test. We did not at the moment see how foolish it was, but immediately afterwards saw the absurdity of it. I need hardly say that our report was not based on that particular test, as, obviously, it does not tell you anything whatever. Instead of the deflection of the voltmeter, as Professor Jamieson thinks, telling you the amount of leakage, the deflection depends on where the leakage is, and we have the maximum leakage giving a minimum deflection if it happens to be near where the voltmeter is attached.

I do not agree with Professor Jamieson that a mirror galvanometer and a scale put on two chairs in the upstairs of a building would be a very convenient method of testing. I do agree with him, however, when he says, at the end of his letter just read by the Secretary, that within five minutes of getting to the spot he would find the spot; indeed, I venture to think that if he did not find the spot before he got there, he would never get there at all.

Professor SILVANUS P. THOMPSON: I quite agree with most of the previous speakers in entirely condemning the use of a very small electro-motive force in testing the insulation resistance of installations. It clearly can be of very little value as indicating the likelihood of that installation to break down. In Mr. Smellie's letter he suggests a method of testing with a magneto machine and a sensitive bell, and I think says that his magneto machine rings a bell through 16,000 ohms resistance. Surely that is no test whatever of the insulating resistance of the lighting system, because, if there is only a resistance of 16,000 ohms, you have practically a most abominable condition of things. No reliance must be placed on the fact that the bell does not ring, because, if it does not ring with over 16,000 ohms, it would not detect a leak of 20,000 ohms, which would be a very important fault. Then, again, I think when we are looking at Professor Thompson.

Professor Thompson. formulæ, of whichever shape they are—whether those from Professor Jamieson's paper, or Mr. Preece's formula on the board —we ought to be very careful as to what case we are applying the formula. We ought to make a broad distinction between three general methods of setting up electric light circuits—those that work at constant potential with continuous currents; those which work (principally arc light circuits) with a constant current, and with a potential which varies with the number of lamps in circuit; and, thirdly, the alternate-current systems, chiefest among them at present being those worked by means of transformers. What I want to point out is that a rule that may be perfectly appropriate for one of these, may be perfectly inappropriate for another. I do not want to find fault with Professor Jamieson's rule if it is to be confined to continuous currents worked at constant potential; but it is quite wrong, and must be wrong, for constant current worked with varying potentials. See what it lands us in. The insulation resistance is to be proportional to the electro-motive force—I have no objection to that—and to be inversely proportional to the number of lamps. Take a case, and see what it lands us in. Suppose an arc system of lamps in series, requiring, say, 50 volts each. If you have three arcs, you want 150 volts. Suppose, instead of taking three arc lamps, you have sixty, then you want 3,000 volts. Dividing your volts by the number of arc lamps brings you back to the same insulation resistance for sixty lamps as for three. If E is to agree in proportion to N, the insulation resistance is to be the same when you have 150 volts and when you have 3,000 volts on the circuit, which is an absurdity. Again, take Mr. Preece's rule, where he introduces the length of the line. I do not quarrel with that rule for a constant potential system, but for a varying current it is worse than Professor Jamieson's, because not only is the electro-motive force to be divided by the number of lamps, which would bring it down to Professor Jamieson's, but it is to be divided by the length of the circuit. Well, now, if you have ten arc lamps, and they are comparatively close together, you have very few insulators on the line between one lamp and the next lamp; but suppose these ten lamps are some distance apart, you have many insulators on the

circuit. Do you want a less insulation resistance because you have many insulators and more sources of leakage? I think that in that case you ought to insist on a higher insulation resistance. I think you want L above the line in the formula, and not below it; you want more perfect insulation as the line is longer, not less. Further, applied to alternate-current work, one has to remember that it is the maximum, and not the average, electromotive force that breaks the circuit down; and if the alternating E.M.F. is rising above and falling below the average value, then clearly a different rule is wanted for alternating-current systems. Take the case of the proposed Deptford system, with its 10,000 volts. If we are going to divide the 10,000 volts by the number of lamps on the circuit—there are no lamps on the main cables, but suppose there are 100,000 lamps on the system—dividing your electro-motive force by the 100,000 lamps, you are going to have only one-tenth of the insulation resistance that you would have if you had 1 volt with one lamp on it. According to that formula you would make the insulation resistance one-thousandth of what it should be with 1 volt on an ordinary 100-volt system. Professor Thompson.

Mr. ALEXANDER SIEMENS: Might I say, as Mr. Preece is not here [*Mr. Preece had left the meeting*], that we spoke about that point yesterday? In such a case N would be 0, and that is why he introduced N + 1, because that primary circuit from Deptford would have no lamps on it at all.

Professor S. P. THOMPSON: I quite understood why Mr. Preece introduced the 1, but my objection is to a rule which gives less insulation for 100,000 lamps than for one lamp.

Mr. ALEXANDER SIEMENS: Yes; but you have not got 100,000 lamps on that circuit; you have only the transformers on the circuit.

Professor S. P. THOMPSON: This formula wants writing differently for different cases; if it is right for one, it is wrong for another.

Mr. ALEXANDER SIEMENS: I only interrupted because your illustration was against Mr. Preece's intention.



Professor S. P. THOMPSON: Then I think I am right in asking

Professor Thompson.

Another point that I think we ought to be careful about is as to the way in which this formula should be used according to the method in which the cables or conducting wires are carried. Clearly the same formula is inadmissible for underground conductors as would be admissible for overhead conductors, and for overhead conductors the rule ought surely to be different according to the way in which the overhead conductors are strung. I have spoken in this room before now of the absurdity of using Bright shackles for overhead wires at every pole. They are all right where you have to shackle off at the end of a line; but it is absurd to use shackles, with two chances of leakage and bad insulation, at every pole, instead of using some other insulator which has less chance of leakage. That absurdity becomes multiplied when you are dealing with high electro-motive force with any distributing system.

Lastly, are we quite clear that these formulæ for insulation resistance are not going to mislead us altogether? Is not the test we want to apply to the various systems a test of quite a different order? The test is not what number of ohms the insulation is to-day or to-morrow, but what will break that insulation down. Is the measurement by a mirror galvanometer, and with a battery of a power below the usual electro-motive force of the circuit, really any test as to the likelihood of the insulation of the system to break down? I will give you a parallel case. Suppose you are building a dynamo. You have the coils, in many cases, made with cotton-covered wire which is very carefully soaked, say, with shellac varnish, and these coils are put into a steam-heated chamber and heated for some time, to drive off the spirit from the shellac. When these coils are brought out, after a week's heating, and are tested for the resistance between the copper and the framework on which they are wound, you may get something considerably under 100,000 ohms. Are you going to condemn that dynamo because it tests so low? I very much doubt whether half the dynamos in the market will show anything like half a megohm between the iron and the copper as they leave the factory. It seems to me that the question is not what the tested insulation is, but really what will break down that

insulation. And surely, if the practical question that presents itself in that way in dynamo machines, in parts of the circuits, in switch-boards, and so on, is what electro-motive force will break the insulation down, then these elaborate systems of testing for insulation resistance are really very much beside the mark. Professor Thompson.

Mr. J. N. SHOOLBRED: The remarks of Professor Silvanus Thompson only confirm Professor Jamieson's paper and Mr. Preece's remarks upon it, in that it is impossible to state any one formula for insulation that would apply to all cases, even of one kind of installation. Take Professor Jamieson's formula, which is stated in his paper to be based upon the number of incandescent lamps of 16 candle-power. Professor Thompson could scarcely have borne that in mind when he spoke of and applied that formula to arc lights, because Professor Jamieson speaks always of incandescent lights. His tables show that, and I think, therefore, it is hardly fair that it should be made to apply to a number of arc lights in series—that is, with a variable potential. Mr. Shoolbred.

Professor S. P. THOMPSON: Will you excuse me? He does not say that it applies to incandescent lamps—with constant potentials certainly—only; they are used constantly in arc circuits.

Mr. J. N. SHOOLBRED: Professor Jamieson's formula has been framed more as an assistance for arriving at a fair insulation for the ordinary run of installations of incandescent lamps. My object is merely to point out, that from the various uses of the current it is impossible to give one formula applicable to all, as Mr. Preece suggested that we should try to arrive at a formula that would be applicable for all cases. Professor Silvanus Thompson has just distinguished between incandescent lights in parallel circuits, arc lights in series, transformers, and alternate-current systems. In addition, particularly in towns, where there may be a long length of untapped main, or where the majority of the work consists of incandescent lamps. Again, there may be motors introduced, or arc lamps, and other circumstances which would cause the requisite amount of insulation to vary much in degree. Hence the difficulty of forming one single formula that shall apply in all cases; the circumstances of each

Mr. Shoolbred.

must be considered, and duly taken into account in prescribing the required amount of insulation. Of the three formulæ named in the paper, Professor Jamieson's tries to represent, by means of the number of lamps, that of the corresponding connections and joints which would occur in incandescent light installations; while M. Picou lays stress upon the length of wire used; and the Society of Telegraph Engineers' rule dwells upon the density of the current. Each formula is best suited to certain circumstances, but hardly can it be made equally well adapted for all installations.

Mr. Spagnoletti.

Mr. C. E. Spagnoletti: I should like to add my experience to the remarks that have been made by Mr. Kempe, by Mr. Sidney Walker, and by other speakers, as to the necessity of testing electric light wires with a very high potential. The difference of currents carried by telegraph wires and electric light wires is of course very great. A case came under my notice in the early days of electric lighting, when the Société de l'Electricité came over here, M. Baudet being their representative. A cable was put down on the District Railway. That cable came into my hands to test. It was tested in the ordinary way with battery power, and we could get no leakage whatever; but as soon as the current was put on the cable from the dynamo, we could positively see the sparks going into the wall; showing that, although we had a good battery power for testing, the current was quite insufficient for its purpose, and useless when compared with what was necessary for the requirements. Mr. Kempe also pointed out the advisability of disconnecting the instruments and test-box connections from the main circuit. That, again, I think, is most necessary to do. I recollect, some years ago, when the Mont Cenis Tunnel was being built, the cable for it was manufactured here at the Gutta-Percha Company's works, and I had the testing of it. I went to the testing room and saw the apparatus. Of course I knew nothing of how the connections were made, all wires being out of sight. The test of the first coil showed no movement of the spot whatever on the scale. I said to Mr. Willoughby Smith, "Are you sure that "all the connections are right?" We were then standing on a

sheet of gutta-percha. The temperature of the room was, I think, 65° Fah.; the sun had been shining on a desk by the window, which was warm. He touched the instrument with one hand, and the mahogany desk in the window with the other, and the spot went off the scale altogether. That, I think, will show the necessity of having the test-box connections off while testing, the more so as they would in all probability not be in so dry a state as the mahogany desk was. Mr. Spagnoletti.

Some very good information has been given us by Professor Silvanus Thompson; but I am afraid that if you adopt the system that he has spoken of—*i.e.*, of finding out what currents would break the cable down—this would not always apply, and would depend upon the age and the deterioration of the cable, for what might not break it down the first six or eight months would in a year or two's time give a very different result. I think it has been clearly shown by the remarks of most speakers to-night that in testing electric light cables they should be tested with as high a voltage as is going to be used, if not with a higher pressure, and free of all connections.

Mr. C. T. Fleetwood: I can speak from past experience, confirming what Mr. Spagnoletti has just said, also Mr. Kempe, about the necessity of disconnecting wires from test boxes when testing. Some years ago, I remember drawing in sixty wires over Blackfriars Bridge, made up of 3 × 20, from different manufacturers, and after the cables were in place instructions were given for them to be tested. It was found that they only gave 30 megohms instead of several hundred megohms. Every lead that was put down to the testing room was condemned. At last it struck me that possibly it was the test box. I went and cut the wire off the test box—the same wire—and joined it to the same lead, and went down to see it tested, when every one was pronounced perfect. From that time to the present wires are always disconnected when being tested at the central station. In reference to testing with a magneto machine, previous to 1870 it was the practice of men belonging to the National Provincial Telegraph Company to test their wires by means of the A B C instrument, and as a rule they got good results; but their cables at that time were overhead, and Mr Fleetwood.

Mr. Fleetwood. of very low insulation. I remember one of the circuits being transferred to the underground system between Winchester House and Clerkenwell fire stations, and the circuit worked very badly indeed. The men who had been in the habit of testing with these magneto machines again and again complained that the underground wire was faulty, while the men who tested the underground wires declared that they were perfect. I went to the end of the circuit with the men using the magneto machines, and found that they got a move on their indicator as soon as they began to grind, but when tested with the Wheatstone bridge the wire was perfect. We then went to the middle of the circuit, at the corner of the Old Bailey, divided the circuit, put the machine in again, and found that the wire was perfect; there was not sufficient capacity in the divided circuit for the charge and discharge of the wire to move the needle. When we tested the whole length there was sufficient of the charge and discharge alone to move the indicator round. It may be that the same difficulty may arise if the instrument is used for testing electric light wires.

Mr. Holroyd Smith. Mr. M. Holroyd Smith: I wish to say, Mr. Chairman, that I rather fancy the question of insulation resistance for electric light installations is not going to be determined by this Society, or any other Society, fixing the value of K in any of the formulæ presented. In my own experience the manufacturers of electric light leads have always been surprised at the low insulation that I have specified, but they have been equally surprised when they have come across the condition that that insulation should be maintained and guaranteed for a considerable length of time. It is very much more a question of the endurance than of any very high and fanciful insulation at the outset. We are depending in this matter almost entirely upon the manufacturers, and it is for them to bring us some material for which they can guarantee long duration of a moderate degree of insulation. It would then be a question of how much we can afford to lose in any installation through leakage, when we know that the loss is going to be constant. I therefore wish to draw especial attention to the fact that it is not the fixing of a high initial value for K, but it is the ascertaining of some material that can be relied

upon under varying conditions, say of heat and cold, dryness and moisture, the condition of which will not be deteriorated by the passage of that very small current that now, like the little rift within the lute, will make the music incomplete. If such a material can be supplied, it may then be possible to fix a value for K that would really be a constant. Mr. Holroyd Smith.

I cannot agree with those speakers who are content with delicate tests. I much prefer that all wires should, when possible, be tested under the maximum current likely to pass through them. An illustration of the importance of this practice occurred to me only yesterday, when I declined to send a machine abroad until it had been so proved, though the instrument tests were perfectly satisfactory. There were four circuits in the machine; three stood remarkably well, but the fourth gave way at once.

Mr. F. WYLES: I think, Sir, with reference to the figures given by Professor Jamieson regarding indoor lighting, there can be no difficulty in maintaining his standard of insulation; but directly you come to outdoor lighting, when you have to deal with smoke, fog, and atmospheric influences, there is difficulty in keeping up high insulation. Mr. Wyles.

With regard to testing, I would rather see some system of regular testing. My own plan is to have tests taken weekly. A galvanometer is kept connected up, the foreman takes the deflections, and I work out the insulation resistance from his reports in my own office.

Nothing seems to be shown on the diagrams with regard to testing the machine for contact. My own experience is that if the machine is rigorously tested a breakdown may be saved. It does not matter, probably, if only one contact occurs on the machine, two being generally required to cause a short-circuit; by regular testing, therefore, the first contact may be detected, and a breakdown prevented.

As to Mr. Walker's remarks about gutta-percha being used for electric light cables. My experience is that with the heavy wire used there is a tendency to produce decentralisation; it may be a question of time, but some day or other the core

Mr. Wyles. will drop through the percha, and the copper will be almost bare at the bottom.

Mr. Lant Carpenter. Mr. W. Lant Carpenter: I desire for a moment to emphatically confirm the remarks just made by Mr. Holroyd Smith, because I happen to know of several contracts where that point was insisted upon by those who were buying the cable. For a year at least—and in many cases for a much longer period—the insulation has to be maintained up to a given standard at the expense of the maker of the cable; and it is not a question of the value of K, as pointed out.

Mr. Walker. Mr. Sidney F. Walker: I should like to say that I have gutta-percha cables that have been standing five years without that fault being met with; and if I were to give a specification for duration, I should say not less than five years; one year would be no good at all.

Sir Henry Mance. Sir Henry C. Mance: With reference to the durability of the insulators, I may mention that in India we have had a good deal of experience both with india-rubber and gutta-percha. If the latter material is not to be subjected to a greater voltage than 100 or 200 volts, I see no objection to the use of gutta-percha for electric light leads. I assume, of course, that the carrying capacity of the wire is sufficient, and that the cable is buried in damp ground. An excellent example of the durability of gutta-percha is afforded by the Persian Gulf cable, which at the head of the Persian Gulf is laid for two or three miles through the date gardens, from two to three feet below the surface. It has been there for more than twenty-five years, and has never given the slightest trouble. Under such circumstances gutta-percha is practically imperishable, but exposed to the dry air it is undoubtedly inferior in lasting qualities to india-rubber. The latter material we found most subject to deterioration when exposed to alternate wet and dry.

In an electric lighting system the question of joints becomes a very important one. You will find in the Proceedings of the Society, vol. iv., 1875, an account of a series of experimental joints in old gutta-percha and india-rubber cores which were under observation for two or three years. There was some

difficulty in making what cable electricians would consider a perfect joint in old gutta-percha; but although the insulation would gradually fall, the joint appeared to be mechanically perfect, and I do not think any of the series would have been prejudicially affected by the currents it would be considered prudent to use on a gutta-percha insulated electric light main. The india-rubber joints made in Hooper's core showed no sign of deterioration. Sir Henry Mance.

As regards the formula before us, I think in practice we shall, as hitherto, have to rely on the common-sense and experience of the electrician in charge, who would probably adopt different standards and apply different tests under the varying conditions. We may assume that periodical tests will be taken in every well-managed installation, not to ascertain the potential the leads will bear (that has been done in the first place), but to keep a record of the insulation of the whole system. At the central station the electrician would probably prefer to use a sensitive galvanometer and a moderate battery power, but under conditions when a delicate galvanometer cannot be used he would naturally avail himself of a higher voltage; it is impossible to lay down a hard-and-fast rule. It may sometimes be necessary, for the purpose of localising faults, to test portions of the system away from the central station where shelter is not available. When compelled to test under these conditions, I have always used a small Siemens galvanometer with a suspended astatic needle, in conjunction with a portable bridge and battery.

Professor W. E. AYRTON: Without delaying the meeting a moment, might I suggest to Sir Henry Mance that from an electric light engineer's point of view the cable is, as a rule, not in soil, damp or dry, but is in the air, either in a room or in a trench under the streets; and under those circumstances (Sir Henry) would your experience lead you to prefer gutta-percha to india-rubber? For example, you will remember that in the telegraph offices in India the gutta-percha wires have had to be replaced by india-rubber-covered wires, for the simple reason that the gutta-percha was found to separate into small pieces round the wire about an inch long, exactly like bits of tobacco pipe Professor Ayrton.

Professor Ayrton.

strung along the wire, and which merely served to keep the copper away from the wall.

Sir HENRY MANCE: That is quite correct.

Professor W. E. AYRTON: For the insulation of wires for electric light purposes, where the wires are to be in air, and not in water or damp earth, would you not prefer india-rubber to gutta-percha?

Sir HENRY MANCE: Yes.

Sir Albert Cappel.

Sir ALBERT CAPPEL: I might just add that in the interior of telegraph buildings in India we use india-rubber-covered wires solely, as Professor Ayrton has just said.

Mr. Spagnoletti.

Mr. C. E. SPAGNOLETTI: May I say just a word on this gutta-percha question? In 1852 some wires were put under ground for about a mile in the Paddington yard, in earthenware pipes, with the old gutta-percha covering pure and simple, without compounds; it was of a very light colour. Being a telegraph circuit, we worked with only a low voltage. About six years ago we had occasion to renew some underground wires, and on opening the ground through the yard we came across the pipes containing these old gutta-percha wires. They were of No. 16 gauge copper, with gutta-percha covering to No. 3—a thicker coating than is now generally used for telegraphic purposes. We examined and tested the gutta-percha, and found it in perfect condition, although it had been down for about thirty years. We joined the old wires to wires we were then laying, as they ran in the right direction for our work, and they have been working well up to the present time, and I have no doubt they will last many more years.

Mr. Wyles.

Mr. F. WYLES: Might I be allowed to say, Sir, that I did not criticise gutta-percha as used for ordinary telegraph work, because we know it is the proper thing in England? But I do criticise it when it is used for electric light wires with heavy cores.

Mr. Donovan.

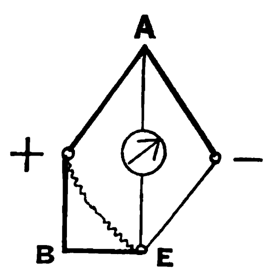
Mr. H. C. DONOVAN: Might I ask Professor Ayrton and others whether they could not give us a formula for interior surface leakage, where exposed contacts would come into the formula?

Professor Ayrton.

Professor W. E. AYRTON: I do not think there would be any difficulty in giving a formula, but I do not think one formula can

cover even one case—that is, one kind of installation—for the simple reason that one standard ought to be required for the dynamo, another standard for the insulation of the gutta-percha, or india-rubber-covered, conductor, and a third standard for the lamp-holders, fuses, and so on, where surface leakage is mainly important. If a single formula be taken to cover the whole thing, then, whatever number be taken for the coefficient K, you are very likely, if you are bound by one formula, to condemn a good installation and to pass a bad one, for the simple reason that there may be a serious leakage in the cable for a cable, and which ought to condemn the cable, but which may not diminish the value of the parallel resistance of the whole installation sufficiently to make the result worked out by the formula low enough to ensure condemnation. Professor Ayrton.

The CHAIRMAN: I quite agree with what fell from Mr. Holroyd Smith—that the great thing is to be sure of maintaining a moderately good insulation, rather than to merely start with a very high one. There also seems to be a consensus of opinion that it is desirable to test the resistance of the conductors with as high a potential as that which is to be used. In my judgment, it is desirable that we should be able to test the insulation resistance of the two conductors from the earth at all times, whether running or standing—particularly when running—and it has long been known that it is quite easy to do that. It is done in this way:— Dr. J. Hopkinson.




It is simply a modification of the ordinary Wheatstone bridge. Suppose + and − to be the two terminals of a dynamo and the conductors connected thereto, and that E represents the earth, with a certain leakage passing from + to E, and a certain leakage going from − to E. Connect + to − through a resistance which can

Dr. J. Hopkinson

be divided into two parts at A, adjustable in relation to each other; connect A to E through a galvanometer; adjust the resistances + A and A − till the galvanometer is not deflected: then the ratio + A/A − will be equal to the ratio of the two earth insulations. Connect either conductor, say +, to earth through a known suitable resistance, + B E, and again adjust the resistances + A, A −. These two experiments give us two equations to determine the two unknown quantities, the insulations from earth.

Gentlemen, it is now my duty to propose that the best thanks of this meeting be accorded to Professor Jamieson for his interesting paper. It certainly has given rise to an interesting, and I have no doubt a valuable, discussion.

The motion was carried unanimously.

The CHAIRMAN: Another paper has been announced for this evening, but it is of such a length and of such interest that it would be very unwise for us to begin at this late hour, and in fact I understand that there is a rule that no fresh matter is to be taken after half-past nine; we must therefore postpone it to the next meeting of the Institution, which will take place on the 21st instant.

A ballot took place, at which the following were elected:—

*Foreign Member:*

Virgilio Machado.

*Members:*

Charles Laurence Baker.
George Cameron Sillar.
Norcliffe George Thompson.
Edward Willmore.

*Associates:*

John Richard Bainton.
Emile Guitton.
Robert Cattley Jackson.
Edwin Isidore Lloyd.
John Macfee, jun.
George Henry Rew.
Brabazon Rutherford.

*Students:*

Joseph Pratt Sleigh.
George Herbert Thornton.
Henry Walker.

The meeting then adjourned.

# COMMUNICATIONS.

## INSULATION RESISTANCE OF ELECTRIC LIGHT INSTALLATIONS.

By WILLIAM McWHIRTER, Member.

Installation at Domira House, Partick—50 lamps, 16 candle-power, and 60 volts. Tested 9th January, 1886, when insulation resistance = 14 megohms.

Installation at Domira House, Partick—90 lamps, 16 candle-power, and 60 volts. Tested 16th January, 1889, when insulation resistance = 35,000 ohms.

*Note.*—This installation in three years has been more than doubled, and the drop in insulation is largely due to leads and fittings in conservatory, which are always dripping wet, as it is washed down with a hose-pipe.

Installation at Heriot-Watt College, Edinburgh—400 lamps, 16 candle-power, 100 volts. Tested April, 1888, when insulation resistance = 30,000.

Installation at Heriot-Watt College, Edinburgh—400 lamps, 16 candle-power, 100 volts. Tested 15th September, 1888 (after buildings had been standing without heating for over three months), and found insulation resistance = 6,000 ohms.

Tested again 22nd September (heating having been going on since 15th), and found insulation resistance = 11,000 ohms.

The fall in insulation was entirely due to moisture, which was deposited heavily on the slate base switches.

Installation at *Free Press* office, Aberdeen—200 lamps, 16 candle-power, 100 volts. Tested 30th August, 1888, and found insulation resistance = 70,000 ohms.

I may add that the above examples are average practice, and very much what we get in installations on board ship.

In dynamos we find it much more difficult to work up to Jamieson's rule; in fact, it requires *great care* in the quality of materials and in the application of same to maintain this standard.

FARADAY ELECTRICAL WORKS,
CAMPBELL STREET, GOVAN, GLASGOW,
13*th February*, 1889.

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By E. W. BECKINGSALE, Associate.

Of the rules on the insulation resistance of electric light installations presented to the meeting of the Institution of Electrical Engineers for discussion by Professor A. Jamieson, that which appears to be the most generally suitable is $R = 10^5 \frac{E}{C}$; this constant, $10^5$, being not too high for contractors to work up to, and not too low for electricians to pass.

But such a rule has an arbitrary character, and does not meet the requirements of some of the electric lighting cables and wires in use which have a very high resistance to leakage.

Reference is made to the lead-cased cables in which the conductor is insulated with a fibrous material steeped in a hydrocarbon. Though high insulation *per se* is not required to produce a good sound cable or wire, yet it is desirable that the normal electric resistance of the insulation should be maintained within some 30 per cent. of the factory tests after the wire is erected; otherwise the cables contain defects, probably caused by damp or by rough usage, which are detrimental to their life and contain the elements of mischief.

It is evident that we require different values of K for different types of cable. If we consider the factory tests of a No. 5 B.W.G. lead-cased fibre-insulated wire and of a No. 18 B.W.G. wire of similar make, which are 8,000 Ω and 15,000 Ω per mile respectively, and compare these resistances with the 300 or 600 Ω per mile of a vulcanised india-rubber-covered cable, it does not seem right that the same constant ($10^5$) should be applied to the tests of these so widely different classes of wire. Dampness of the insulating

material, especially of the lead-cased cables, will generally let down its megohms in a surprising manner, and within reach of the most humble instrument to measure; but this is not always one's fortune. Cases have occurred within the cognisance of the writer in which the admission of water caused only a partial loss of resistance. One of these leakages was measured—or, rather, that of the cable was—which showed a decrease of insulation from 33,000 Ω to 1,700 Ω. The cause of the fault was apparent, and it was cut out, but under the rules it need not have been.

The abundant use of compound in the insulation of joints is to be condemned. The writer has employed a sleeve of the same, or of similar, insulating material to that which the conductor is covered with: this is placed over the end of one of the wires before it is soldered, and then drawn into position over the joint. The sleeves must be long enough to lap over and embrace the insulating material of the conductor at either side. India-rubber cement may be used sparingly, and the ends bound with fine twine.

A slight modification of this insulation joint is required for T joints.

*February 15th*, 1889.

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By CHARLES BRIGHT, Member.

I was rather surprised to hear it suggested during the discussion that gutta-percha was, in a *general* way, preferable to vulcanised india-rubber for electric light purposes; though it appears to me that it may perhaps be superior in the particular case of mines or collieries, under certain circumstances—where the ground is subject to water, or where it is, at any rate, moist or damp.

It is well to remember the following facts, however:—Gutta-percha is, so to speak, essentially a "wet bob": in water it may be said to be imperishable, and not only retains its electrical qualities, but absolutely improves in insulation by age to a considerable extent; whereas the resistance of india-rubber, even when vulcanised, goes off, as a rule, by age when in water. In a

dry soil, however, vulcanised india-rubber generally proves more durable as an insulating medium than gutta-percha; and when exposed to the oxidising influence of air (as in the case of an aerial line), gutta-percha rapidly deteriorates by cracking, especially in a smoky, or otherwise foul, atmosphere, and its resistance is soon fatally lowered in consequence. Vulcanised india-rubber is found to be much more durable under the same circumstances. In very low temperatures, moreover, gutta-percha becomes hard and cracks still more readily.

The highest temperature before which any species of gutta-percha gum begins to soften is 120° Fah., whereas no mixture of vulcanised india-rubber softens until about the boiling point of water is reached. No. 4 of the Society of Telegraph Engineers' "Rules for the Prevention of Fire Risks" suggests, very properly I venture to think, that "whatever insulating material is "employed, it should not soften until a temperature of 170° Fah. "has been reached."

India-rubber when vulcanised, being so much less affected physically or electrically by temperature than gutta-percha, it is better suited as an insulator to a conductor carrying high currents. If large currents are used, or if the line be exposed to the sun, the conductor is much more liable to fall excentric in a gutta-percha covering than in one composed of vulcanised india-rubber, owing to the greater sensitiveness of gutta-percha to temperature; with a given limit of thickness for insulating purposes, this is liable to be the cause of serious faults.

In order to avoid the above ill effects of overheating, the conductor would, in a given circuit, require to be heavier in a gutta-percha core than with a vulcanised india-rubber covering, and, consequently (which is a more important item from an expense point of view), a greater weight of gutta-percha will be necessary than in the case of the india-rubber core, in order to give the same thickness, so as to offer the same insulation resistance beyond the extra amount which might be required owing to the difference of their specific resistances.

*February 20th*, 1889.

## PROFESSOR JAMIESON'S REPLY TO DISCUSSION ON HIS PAPER ON "THE INSULATION RESISTANCE "OF ELECTRIC LIGHT INSTALLATIONS."

I am very glad that my communication has given rise to such an extensive, complete, and instructive discussion. In reading over the printed reports, I find that every point raised in the paper, and almost every fresh idea expressed by members, has been met and answered by one or more of the speakers and correspondents. The duty of replying is thereby rendered much easier than it would otherwise have been.

Mr. Preece hit the nail on the head when he said, "There "must be a distinct rule for deciding what shall be the *minimum* "insulation resistance to be given to installations under the worst "circumstances;" and, "How is it possible for local authorities, "scattered all over the country, to decide what is a defective "main, or what is a defective lead, unless we, the Institution of "Electrical Engineers, are able to define some law that shall "decide the difference between a bad and a good conductor?"

I gather from the remarks of several of the speakers that it would be necessary to have different rules for each of the following cases:—

1. Installations worked by continuous currents at constant E.M.F.
2. Installations with constant currents, such as are common to arc light circuits or with low-resistance incandescent lamps in series.
3. Alternating-current systems.

These would require to be supplemented by constants for—

(*a.*) Subterranean main supply cables.
(*b.*) Overhead main supply wires.
(*c.*) The complete system of leading wires within buildings.
(*d.*) The switch-boards, and the fittings, separately and combined.
(*e.*) The generators of electrical energy.
(*f.*) The transformers of electrical energy.

I would therefore suggest that the Institution of Electrical

Engineers should appoint a Committee to draw up and send out forms, including each of the above cases, to electricians and firms, requesting them to fill these up and return them to the Secretary. The desired information would then be obtained upon a uniform and comparable system, which would enable the Committee to decide upon the most suitable rules and constants.

Mr. Kempe draws attention to the omission in the paper of not distinguishing between the insulation resistance of the cables or leads and that of the fittings. When I first began to test electric light installations (*i.e.*, when I was fresh from submarine cable work), I always tested the leads by themselves, and laid down a rule of "one megohm per lamp for every volt in the case "of the leads by themselves." Now, however, when called in to report upon *completed* installations, I find that contractors object to the disconnecting of all their leads from the fittings and lamps, for they say, "To disconnect all the wires every time a special "test has to be applied would do more harm than good;" and usually add, "Apply your tests to the whole installation as it "stands, seeing that is the working condition of affairs." When I have the opportunity of drawing up specifications and of inspecting the whole of the work, I now insist upon insulation resistance tests being applied to all the coils of insulated wire *before* they leave the contractor's works; the coils being submerged in water for not less than six hours immediately previous to, and during, the tests. The coils are, of course, afterwards carefully dried. In the case of coils that do not considerably exceed the minimum insulation resistance allowed, I have them subjected whilst in the water to an E.M.F. 50 per cent. greater than the proposed working E.M.F. for some considerable time, and then tested again. Should any appreciable reduction in their insulation resistance have taken place, due to the stress or fault-ferreting currents, the coils are rejected. The leads are again tested when fitted up, before the switches, &c., are attached, and finally the whole combination, immediately previous to and after the preliminary or trial run of the complete plant. I still think that the application of high E.M.F.'s is *only* necessary for the purpose of putting a severe stress upon the insulating material o

the cables and leads. If a weakness or small fault exists, it will be opened up by the proper application of this stress, and then the correct measurement of its value (and even the position in some cases) can be more accurately obtained by the employment of a lower E.M.F. combined with more delicate apparatus than could be conveniently or safely used with the higher E.M.F.'s.

In the case of important district mains and sub-mains, it will be found absolutely necessary to carefully test them periodically and record the results of their insulation resistance *quite independent* of that of the distributing apparatus and the leads, &c., in the various places or buildings supplied with current. The very simple and easily applied test described by Dr. Hopkinson (and referred to by a foot-note in the paper, see p. 58) will, however, be found very useful and convenient, since it is specially adapted for the working conditions of installations. The person in charge of an installation does not necessarily require to know anything about the principle of the test, or to be even able to work out the equations, if he is supplied with a table giving him the insulation resistance corresponding to the employment of the various resistances in the arms of the bridge. All he has to do is to vary these resistances until he finds no deflection on his galvanometer, and then refer to the table and record the result. In fact, it would be possible to arrange for an automatic recorder of the working insulation resistance of an installation upon this principle.

The moral effect of a definite set of rules and system of testing for insulation resistance upon contractors, and still more so upon the workmen employed in fitting up installations, was not touched upon by any of the speakers. There cannot be the slightest doubt but that a contractor and his foremen have a whip hand over their men if they can say, "Well, now, Jack, you "must take special care this time with the leads, joints, and "fittings, for Professor Spot is coming down with that magic "mirror of his, which will show up the weak points in your work, "and if the deflection should be too great you will get time for "reflection at your own expense!"

## COMMUNICATION FROM SIR WILLIAM THOMSON, PRESIDENT.

In speaking of the practical superiority of copper over iron for high-speed telephony or telegraphy in my recent Presidential Address, with reference to which I quoted Mr. Bennett's practical experience, I should also have quoted the following from Mr. Preece's paper "On the relative Merits of Iron and Copper Wire "for Telegraph Lines," read before the British Association in September, 1885, which I had overlooked when I was preparing my address, but which I have since seen with much interest. It is contained on pages 908 and 909 of the British Association Report of the 1885 Aberdeen Meeting:—

"The second series of experiments were conducted between London and Newcastle, and were designed, as stated above, to test the working efficiency of the copper wire as compared with iron wires. They were conducted by Messrs. Chapman and Eden.

Copper shows a very decided superiority over iron, the speeds being as follows:—

| | Copper | Iron |
|---|---|---|
| Simplex working . | 414 . . | 345 words per minute. |
| Duplex ,, . | 270 . . | 237 ,, ,, |

It is anticipated that the superiority of copper over iron indicated by these experiments will have a beneficial and economical influence on our telegraph system, and that its extended use will enable us not only to work better, but to dispense with intermediate repeaters in many cases where, on long lines, they are now necessary.

The most interesting point, however, in connection with these experiments is that they apparently prove that the superiority of copper is not simply due to its smaller electrostatic capacity and resistance, but that it is more susceptible to rapid changes of electric currents than iron; for when the resistance and capacity of the copper and iron wires were equalised by the insertion of resistance coils and condensers, the speed on the former was not thereby diminished. Possibly the magnetic susceptibility of the iron is the cause of this. The magnetisation of the iron acts as a kind of drag on the currents. It is well known that telephones always work better on copper than on iron wires, doubtless for the same reason.

These experiments also show the high speed of working that is now attained by the Post Office authorities with the Wheatstone automatic apparatus. The following table gives an interesting *resumé* of the different stages of the progress made, and its rate of growth":—

| | | | |
|---|---|---|---|
| 1877 . | . 80 words per minute. | 1881 . | . 190 words per minute. |
| 1878 . | . 100 ,, ,, | 1882 . | . 200 ,, ,, |
| 1879 . | . 130 ,, ,, | 1883 . | . 250 ,, ,, |
| 1880 . | . 170 ,, ,, | 1884 . | . 350 ,, ,, |

W. T.

*March 6th*, 1889.

The One Hundred and Eighty-sixth Ordinary General Meeting of the Institution was held at the Institution of Civil Engineers, 25, Great George Street, Westminster, on Thursday evening, February 21st, 1889—Professor W. E. AYRTON, F.R.S., Vice-President, in the Chair.

The minutes of the previous meeting were read and approved.

The names of new candidates for admission into the Institution were announced and ordered to be suspended.

The following transfer was announced as having been approved by the Council:—

From the class of Associates to that of Members—

Frederick Jonathan Down.

The paper for the evening, which, as the author was in the United States, was read by the Secretary, was—

## ON CERTAIN PHENOMENA CONNECTED WITH IMPERFECT EARTH IN TELEGRAPH CIRCUITS.

By A. E. KENNELLY, Associate.

Since the earliest days of commercial telegraphy—the days that first reaped the labours of Sœmmering and Steinheil, Gauss and Weber, Cooke and Wheatstone—the substance of our planet has been impressed into such universal conducting duty that now scarcely a circuit closes, be it across an ocean, or be it across a meadow, but embraces its share of the earth in its completion; and the task thus imposed is generally borne so readily and executed so perfectly that, until the existence of the eaves-dropping telephone, it was really doubted whether in truth the earth could or did conduct the current entrusted to its care.

Considering, then, the great obligations telegraphy owes to the earth—that the existence of some hundred thousand miles of

metallic conductor, at present in daily telegraphic operation, implies in a certain sense the corresponding twin existence of an equal length of terrestrial conductor ready made by Nature; that it has even been possible to telegraph over a space of which the metallic conductor formed but a very proper fraction—the cases that are met with where the earth fails to fulfil its part excite astonishment more by their rarity than by the force of those laws whose mandates they obey.

Almost all such cases of imperfect earth are, we know, traceable to some fault either in the earth connection itself or in its immediate neighbourhood; and a remedy is found by adequately increasing the surface area of the buried electrode, or the conductivity of the ground in its vicinity; so that they are not really failures of the earth's conduction, but failures in securing the necessary degree of connection with the earth's mass.

There are, however, other cases, brought about by conditions more deeply seated and more rarely met with, which will occupy our present attention.

A paper by Mr. James Graves, "On Vibrations due to Earth-"Plates," was read before this Institution in January, 1875, and it will be seen that the phenomena therein described are probably related to those that will be presently detailed. Although the matter has not occupied the Institution's direct attention since that date, no doubt some of its members will have independently observed similar phenomena, and can bring forward such additional evidence as cannot fail to elucidate a subject at once replete with technical interest and important practical bearings.

These phenomena are presented by the Atlantic cables of the Direct United States Cable Company, with whose permission the facts are laid before the Institution.

The two cables of this company terminating in Nova Scotia run to Ireland and to Rye Beach respectively. Their landing place was originally Torbay, N.S., but they were removed thence to Halifax some eighteen months since.

The diagram map supplies the outlines and main features of Halifax Harbour. The Irish cable was first brought into the cable-house, situated 40 feet above high-water mark, upon the

beach opposite George Island, on the 23rd October, 1887, and it was put in communication with the town office by means of a line cable and a return earth cable, side by side, in an iron pipe. Each cable was 0·7 knot long, had a resistance of 8·3 $\omega$, a capacity of 0·25 $\phi$, and a high insulation. The line cable united with the Irish shore end through lightning guards, while the earth cable made good soldered connection with the shore end sheathing.

Some vibration of the mirror spot, due to electrical disturbance, was noticed on first joining up the circuit at the office through instrument and condensers, but this was at the time attributed to faults which then existed in the cable. Beyond this continual vibration, no noteworthy feature occurred until six days later (29th October), when the Rye Beach cable was landed at the same cable-house, and joined up with the office by a precisely similar pair of cables running through iron piping in the same trench with the other above mentioned. This cable is worked on the double-current Morse system without condensers, and it was immediately noticed that sending to Rye Beach with 12 volts of battery power visibly disturbed the mirror spot on the Irish circuit, although each cable made earth separately through its own sheathing *via* the insulated return cables to the beach. Receiving from Rye Beach did not disturb the mirror, nor did the operation of the Irish cable in its turn affect the less delicate apparatus of the Morse circuit.

The only remedy that could be, or has since been, practically available, without adding to the retardation of the circuits, was the reduction of the Morse battery power on one hand, and the sensitiveness of the Irish mirror on the other, as far as the operation of the circuits would conveniently permit.

After the removal of the faults in the Irish cable the vibrations of the mirror still continued, and it was noticed that not only these, but periods of disturbance coincident with local meteorological changes, affected the signals more than had been on record with the same circuit when worked previously from Torbay. On this was superposed the interference caused by transmission on the Rye Beach circuit, and the combined disturbance at times made the mirror signals difficult to read.

The Rye Beach shore end was at first laid very near to the Irish cable, and actually over it at one or two points. The cables were, however, subsequently separated, although they still necessarily lie at different points in close proximity. This separation of the cables was found to have sensibly lessened their interference.

The operation of the cables from the cable-house instead of from the office was found to produce no change in the nature or degree of disturbance, while experiment failed to produce any visible interference between the trenched cable circuits from office to Beach, even on the most delicate instruments.

A third cable was then laid from the cable-house across the harbour and out through the eastern passage in the direction shown on the diagram. It had a total length of 5·8 knots, sheathed up to within half a mile of the sea end. This unsheathed half-mile of core terminated in a copper earth-plate one yard square, nearly on a line between Thrumcap Rock and Hartland Point. This well-insulated line offered every prospect of securing a good and independent "earth" by its large terminal plate. When, however, it was joined up at the cable-house with the Irish circuit, in the place of the Irish shore end sheathing, the interference from the Rye Beach transmission remained as marked as ever.

To determine the nature of this obstinate interference, a series of experiments was made at the cable-house, as follows.

All existing connections were for the time dismantled, and only the two cables, the two earth-wires soldered to their respective sheathings, and the core of the third cable, or six-mile earth, were brought up to the testing table.

The resistances between these three earth-wire terminals was measured as follows by bridge :—

| | | |
|---|---|---|
| Between Irish sheathing and Rye Beach sheathing | 0·16 $\omega$ | all steady. |
| " " " " six-mile earth ... | 65·7 $\omega$ | |
| " Rye Beach " " " " ... | 65·7 $\omega$ | |

From which the resistance of each sheathing earth connection would, of course, be 0·08 $\omega$, and of the six-mile earth 65·6 $\omega$.

The only apparatus then retained for investigation was a

Daniell battery of fourteen gravity cells—used with, and sometimes also without, a pole-changer—for exciting disturbance, and a sensitive speaking mirror—used with, and also without, condensers of 40 $\phi$ capacity—to render any interference visible.

*Experiment* 1. See Fig. 1.

Sending reversals from seven cells into the Rye Beach cable produced vibrations of the spot on the mirror instrument in circuit with the Irish cable, its sheathing, and 40 $\phi$.

*Experiment* 2. See Fig. 2.

Similarly, sending reversals into the Irish cable, using its sheathing, produced vibrations on the mirror in circuit with the Rye Beach cable condenser and sheathing.

*Experiment* 3. See Fig. 3.

Sending reversals into the Rye Beach cable produced vibrations on the mirror in circuit with the Irish cable condenser and six-mile earth.

*Experiment* 4. See Fig. 4.

Sending reversals into the Rye Beach cable, using the six-mile earth, produced vibrations on the mirror in circuit with the Irish cable condenser and its own sheathing.

*Experiment* 5. See Fig. 5.

Sending reversals into the Irish cable, using the six-mile earth, produced vibrations on the mirror in circuit with the Rye Beach cable condenser and its own sheathing.

*Experiment* 6. See Fig. 6.

Sending reversals into the Irish cable, using its own sheathing, produced vibrations on the mirror in circuit with the Rye Beach cable condenser and six-mile earth.

*Experiment* 7. See Fig. 7.

Sending reversals on the circuit formed by the six-mile earth, battery, and Rye Beach sheathing, the disturbance on the mirror in circuit with the Irish cable condenser and sheathing was more decided than in any of the preceding experiments.

*Experiment* 8. See Fig. 8.



Similarly, sending reversals on the circuit formed by the

six-mile earth, battery, and Irish cable sheathing, the disturbance on the mirror in circuit with the Rye Beach cable condenser and sheathing was equally great.

*Experiment* 9.

The two last experiments were repeated with the condenser removed and the mirror in direct circuit between cable and sheathing. The continual vibration of the spot made observation difficult; but during periods of comparative quiescence it was soon determined that the disturbance produced by depressing one of the keys was not of a momentary nature only, but consisted of a small deflection, permanent during the whole period of key application, and of direction depending upon the key selected. It was also ascertained that this deflection was not due to any local electro-magnetic action by

*Experiment* 10. See Fig. 9.

On sending reversals between the two cable sheathings in circuit with the battery, no disturbance could be detected on the mirror in circuit with one of the cables and the six-mile earth.

This small steady deflection produced by holding down the key in either of the two cases tried in Experiment 9 was more plainly visible when the mirror was in circuit with the Rye Beach cable, which, besides its shorter length, had no condenser in circuit at the distant station. To obtain some quantitative information regarding the strength of current represented, the fourteen cells were connected in series and more careful observations made.

*Experiment* 11. See Fig. 10.

Closing the circuit formed by the fourteen cells, the six-mile earth, and Irish sheathing, measurements were made of the steady deflection so produced on the mirror joined up with the Rye Beach cable and sheathing.

The deflection obtained was subsequently found to approximately represent the two-hundred-thousandth part of an ampere —*i.e.*, 5 micro-amperes—and its direction was observed to be that indicating a current entering the Rye Beach cable from that pole of the battery which was connected to the Irish sheathing,

reversing when the battery was reversed. Thus, in the figure the copper pole of the battery being shown connected to the sheathing, the deflection represented a current from the copper pole entering the Rye Beach cable through the mirror.

*Experiment* 12. See Fig. 11.

The Irish sheathing was replaced in the battery circuit by the water-pipe earth connection at the office through the trenched cable. On depressing the key the mirror showed the same permanent degree of disturbance as before, in each cable circuit.

*Experiment* 13.

This experiment was a repetition of No. 3, with the condenser removed. It was subsequently observed in this case that the disturbance was not permanent during the depression of the battery key, but consisted of a small vibration of the spot at the opening and the closing of the battery circuit.

*Experiment* 14. See Fig. 12.

This was carried out at the office, situated in the principal street of the town, and nearly opposite to the Western Union Telegraph Office, fifty yards distant. All the Western Union wires are overhead, and the earth connections made for them are the gas and water mains, which, with the telephone lines, form the only metallic connection between the two buildings. With the courteous assistance of the Western Union officials it was arranged that at a given word by telephone several of their circuits should suddenly be operated, and again, all stopped.

When this was done the effect was distinctly visible upon a mirror in circuit with each cable, the disturbance being greater on the Rye Beach line, the earth connections being the cable sheathings through the insulated cables in the trench. Only four known causes could under any conditions produce interference between the two cable circuits, these being, of course, electrostatic induction, electro-magnetic induction, leakage, or bad earth connection in common; and, in the absence of evidence, any one or more might produce it, although no reason could be demanded for the operation of any, since cables lie in other parts

of the world for greater distances at equal proximity without the least visible interference.

Experiment No. 10, together with the preliminary trials with the trenched cables, showed that the causes were not to be traced locally to these or to the apparatus; and the high insulation of the cables and circuits, together with the fact that the tests were carried out at the cable-house, precluded all possibility of leakage being concerned, so that the operating causes were thus limited to the two forms of induction and bad earth.

The first six experiments did not directly point in favour of any particular cause, but Nos. 7 and 8 rendered the possibility of electro-magnetic disturbance untenable, since the exciting circuit, except in the cable sheathing itself, took a course, as seen on the map, almost at right angles to the two main cables. They further showed that the interference was such as a deficient earth connection would produce. Experiment No. 9 confirmed this view, since no form of induction could set up the steady deflections observed; and leakage being out of the question, bad earth was the only cause that could produce them. It was further necessary to suppose that not only one but both cable sheathings failed to secure good earth, and that the resistance in their respective earth connections was common to both.

Experiment 11 corroborated this position; and No. 12 did more, for it extended the limits of this bad earth region over the whole water-pipe area of Halifax, as it would have been impossible to obtain interference under those conditions unless the cable sheathings and the town water-pipes formed a common conducting system throughout whose limits imperfect earth connection existed. Finally, the evidence of Experiment 14 followed the same direction, the exciting circuit being in this case the Western Union land lines.

The imputation of bad earth to the gas- and water-pipe area of a town, and that town a seaport, is sufficiently remarkable; but the presumption of a general earth imperfection in a system of extended water and gas mains, together with the sheathings of three cables running directly into an arm of the sea, may at first sight appear an outrage upon credulity. Due consideration

of the experimental evidence renders the conclusion, however, inevitable; and this is tantamount to the assertion that the whole of this system—that is to say, all the land covered by the town, as well as that forming the harbour basin—is composed of substance more or less insulated or insulating.

One of the most interesting features in the geology of Nova Scotia is that the Atlantic seaboard presents an uninterrupted line of Lower Cambrian formation. The relation of this fact to our immediate subject is patent when we remember that these rocks—early in order of geological structure, and said to be almost destitute of organic remains—are here almost entirely composed of argillites, quartzites, and micaceous schists—slates and grits—substances experimentally known to be non-conductors; and according to the published papers of Professor Honeyman, who has studied the geology of Nova Scotia for many years, no other formations mingle with the Lower Cambrian in the structure of the Halifax basin except the still lower archæan granites, all of which are presumably non-conductors. In fact, the renowned natural advantages of Halifax Harbour are due to the unrelieved presence of these abrupt slaty cliffs that immediately underlie the soil for miles inland.

The question then arises, Granting that on the above geological considerations the rocky basin of the harbour and foundation of the city form an insulating crust, or one, at least, interposing a decided amount of resistance between the superficial conducting system and the ocean or earth's mass,—is the resistance that can be jointly offered by the three cable sheathings and the whole waterway between the cable-house and open sea, of sufficient magnitude to produce the degree of interference observed?

The answer, which seems to be affirmative, strictly demands a computation of the resistance offered by the entire mass of sea-water in the harbour under those conditions. This would be a gigantic problem to solve with any pretence to accordance with nature; but for the purposes of the case such limitations may be admitted as shall greatly simplify the calculation.

The map shows that three channels open upon the inner harbour, one running inland through the narrows, and two passing seawards—the eastern passage, and the main or southern entrance. The inland channel terminates in a bay some miles beyond, and may be considered to be practically insulated with the rest of the basin. The waterway in the shallow and narrow eastern passage will evidently offer a comparatively high resistance compared with that of the main entrance, which, therefore, demands principal attention.

The general mathematical theory of the earth's resistance is given in Schwendler's "Testing Instructions," following the reasoning of Smaasen's theory, published in 1847. It is shown that if a perfectly conducting small spherical electrode of radius $r$ were situated concentrically within a larger hollow perfectly conducting sphere of radius R, and the intervening space filled with a homogeneous substance of specific resistance $\rho$, the resistance of any elementary spherical shell of radius $x$, measured from the common centre, would be $\frac{\rho\, dx}{4\, \pi\, x^2}$, and hence the total resistance ($\omega$) between the two spherical electrodes would be the integral of this quantity between the limits $x = \text{R}$ and $x = r$; whence

$$\omega = \frac{\rho}{4\, \pi}\left(\frac{1}{r} - \frac{1}{\text{R}}\right).$$

If the radius R of the outer electrode be indefinitely increased, the resulting equation becomes

$$\omega = \frac{\rho}{4\, \pi\, r},$$

which represents the resistance of an unlimited medium of resistance $\rho$ to a current passing from a sphere of radius $r$ to its ultimate confines. Similarly, a second small spherical electrode of radius $r_1$ would offer under the same circumstances a resistance

$$\omega_1 = \frac{\rho}{4\, \pi\, r_1};$$

and if these two small spherical electrodes were placed so far apart in the medium that their direct influence on each other's lines of current-flow would be very small, the resistance between them would be

$$\omega + \omega_1 = \frac{\rho}{4\, \pi}\left(\frac{1}{r} + \frac{1}{r_1}\right)$$

—a result equivalent to the statement that almost all the lines of current-flow issuing from the surface of each electrode would unite with least opposition at indefinitely great distances, and after indefinite diffusion throughout the medium.

If the system were then symmetrically divided by an unlimited insulating plane, the resistance ($\Omega$) between the hemispheres on one side of the plane would be double the above, or

$$\Omega = \rho\left(\frac{1}{c} + \frac{1}{c_1}\right),$$

where $c$ and $c_1$ are the circumferences of the two hemispheres.

This would then be the resistance between two hemispherical electrodes imbedded in the bounding plane surface of an indefinitely extending homogeneous medium; and if the electrodes were not hemispheres, but vertical plates imbedding active surfaces ($s$ and $s_1$), the above formula would approximately apply, if $c$ and $c_1$ be the circumferences of equivalent hemispheres exposing those areas respectively, and the transformed equation would be

$$\Omega = \frac{\rho}{\sqrt{2\pi}}\left(\frac{1}{\sqrt{s}} + \frac{1}{\sqrt{s_1}}\right)$$

—an equation independent of the distance between the plates.

Similar results are obtained and published by MM. Mascart and Joubert by separate reasoning. The formula applies to the earth's resistance in a telegraphic circuit, on the assumption that our planet has a practical uniform conductivity, and forms practically an indefinitely extended medium.

In the absence of experimental evidence it would be difficult to say how far the variable conductivity of the earth's mass would affect the application of this formula. The conductivity of the material composing the earth's crust probably varies considerably in different localities, and is generally not the same as that of the ocean, for example. It might even be assumed that the resistance of the primary rocks is so great as to limit the conducting area almost entirely to the superincumbent stratified formations.

In that case the earth's telegraphic resistance would be more nearly comparable to the resistance between two small distant

electrodes imbedded in the surface of an indefinitely extended sheet of uniform thickness and conductivity. On this supposition, let $d$ be the depth of the earth's conducting crust, assumed to be uniform: then the resistance, $\Omega$ (*vide* Appendix), between two earth-plates of active surfaces ($s$ and $s_1$) would be, approximately,

$$\Omega = \frac{\rho}{\sqrt{2\pi}}\left(\frac{1}{\sqrt{s}} + \frac{1}{\sqrt{s_1}}\right) + \frac{\rho}{\pi d}\log._\epsilon\left(\frac{4\,D}{d}\right).$$

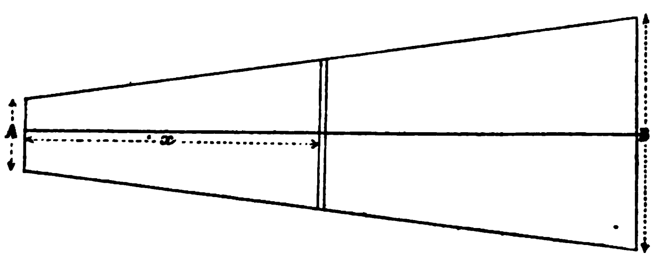
This equation is the same as that of Schwendler last stated, but with the addition of a second term involving D, the distance between the plates; so that on this theory the earth's resistance would increase with the length of the circuit. If submarine cables were sufficiently well insulated, it would be possible to ascertain which of these two theories is nearer to the truth, by measuring the conductor resistances of cables which are laid in duplicate or triplicate between two stations; the difference between the resistance of any pair looped, and the sum of their separate resistances employing the earth circuit, giving, of course, double the earth's resistance between the terminals. It is, however, impossible to decide from present experimental evidence in this way, since not only does the difficulty of accurately measuring these quantities probably increase with the square of the length of cable, owing to leakage and other causes, but also because this second term of the last equation whose existence is in question must in all cases be very small.

Generally speaking, therefore, the experimental evidence may be said to establish the results given by Schwendler's formula, namely, that when the earth-plates are separated by any distance great compared with their dimensions, the resistance varies with their active area, and is independent of their distance.

Schwendler gives an estimated superior limit of the specific resistance of the earth in India as 3,300 $\omega$, or about 100 times that of saturated zinc-sulphate solution at 10° C.

Returning to the resistance offered by the waterway in the main entrance of Halifax Harbour, we may suppose a current to be flowing seawards through it and the cable sheathings from the cable-house. The lines of flow will permeate its whole area, and the equipotential surfaces may be supposed to form across it in

successive curves. These surfaces will be practically vertical, and a position has to be assumed for the ultimate one representing the zero-potential, or the surface that may be considered to mark the limit of perfect earth connection. Theoretically, of course, this limit would be far out in the ocean; but it may be arbitrarily assumed that a vertical plane shown on the map by a dotted line connecting the Thrumcap with the Mars Rock buoy represents this boundary, and that the resistance to earth of the ocean mass outside this plane is altogether negligible. The two other dotted lines converging from this base up to the cable-house—one skirting Sandwich Point, and the other running over Maugher Beach—are intended to represent what may be considered the simple boundaries of an equivalent waterway equal in conductivity to the actual one. The average depth may be taken as 14 fathoms. We have, on this supposition, a mass of sea-water resembling a wedge with a blunt point, the breadth of the base being 1·2 knots; the breadth at the apex, say, 5 metres; the length, measured perpendicularly to the base, 4 knots; and the depth, 14 fathoms. Strictly speaking, the equipotential surfaces of flow will be curves convex to the ocean; but the error introduced will not be great if we consider them as planes parallel to the base, or zero surface. Since the fall of potential will take place with equal rapidity through the water and through the sheathings, the resistance of the latter may be taken into account later and independently. The resistance of any




elementary lamina of thickness $dx$, distant $x$ from the cable-house, will be $\frac{\rho\, dx}{h\,(ax+b)}$, where $\rho$ is the specific resistance of the

water, $h$ the depth, while $a$ and $b$ are constants; so that the total resistance between the two ends is

$$R = \frac{\rho}{ah} \log_{\epsilon} \frac{B}{A},$$

where B and A are the breadths at the base and apex.

In this case

$$a = \frac{1 \cdot 2}{4 \cdot 0} = 0 \cdot 3;$$

$$h = 14 \times 6 \times 30 \cdot 5 = 2{,}560;$$

$$\frac{B}{A} = \frac{185{,}000 \times 4 \cdot 2}{500} = 1{,}550;$$

$$\rho = 31.$$

The specific resistance of a saline solution of density 1·027 has been observed experimentally to be about 31 $\omega$ at 5° C.

With these data,

$$R = \frac{31 \times 2 \cdot 72 \times 3 \cdot 19}{0 \cdot 3 \times 2{,}560}$$

$$= 0 \cdot 35\ \omega,$$

| | |
|---|---|
| about one-third of an ohm, equivalent to a conductivity of ... ... ... ... ... ... | 2·9 mhos. |
| Compared with this, the conductivity of the eastern passage channel may be estimated at about one-fourth, or ... ... ... ... ... | 0·7 mho. |
| The conductivity of the two cable sheathings, each consisting of ten No. 6 wires, and length 4·2 knots, would be 20 × 0·020 ... ... ... | = 0·4 mho. |
| And, finally, the conductivity of 7 knots of cable sheathing in the eastern passage, 10 × 0·012 | = 0·1 mho. |
| Total conductivity ... ... | 4·1 mhos, |

equivalent to a joint resistance of 0·24 $\omega$. So that if the above reckoning is sufficiently fair, the resistance between the cable-house and the ocean is about a quarter of an ohm.

The resistance actually offered can be estimated from the data of Experiment 11, as follows:—

The E.M.F. in the exciting circuit was about 15 volts; the resistance, 66 $\omega$ in the cable and earths, and 24 in the battery—

total, 90 $\omega$. In the Rye Beach circuit the resistances at the time were, approximately—

| | | | | |
|---|---|---|---|---|
| Observing mirror | ... | ... | ... | 2,000 $\omega$ |
| Cable conductor | ... | ... | ... | 5,800 |
| Relay at Rye Beach | | ... | ... | 1,000 |
| | | Total | ... | 8,800 $\omega$ |

Since the interference current observed in this circuit was, as already mentioned, $5 \times 10^{-6}$ amperes, the E.M.F. impressed upon the cable due to interference was

$$5 \times 10^{-6} \times 8{\cdot}8 \times 10^{3} = 0{\cdot}044 \text{ volts};$$

and consequently, if $x$ be the required resistance of the general earth connection,

$$x : 90 :: 0{\cdot}044 : 15,$$

or

$$x = 0{\cdot}26\ \omega,$$

or an actual resistance of a quarter of an ohm. The agreement between the actual and measured values is really much closer than the accuracy of either determination would warrant.

The only experimental results still left unaccounted for are those of Nos. 3, 4, 5, and 6, in all of which the six-mile earth was used either in the exciting or observing circuit; and since Experiment 13 proved that this earth-plate was outside the limits of the semi-insulated basin, as might be expected from its position on the map, it would be impossible to account for the interference in those cases on the score of a common imperfect earth. Reflection shows, however, that the interference observed would be an indirect necessary consequence of the imperfect earth, since the six-mile earth, when connected to either main circuit, forms a cable-loop of, say, 10 knots within the limits of the semi-insulated basin, and a slight variation in the potential of the basin is calculated to produce a considerable electrostatic effect on the cable. It has, in fact, been found experimentally that when 1,000 feet of insulated cable core (of similar capacity to that of the cables at Halifax) are coiled in an insulated tank and left in communication with the earth through a mirror instrument, the electrostatic disturbance of the mirror is about equal to that observed in these experiments (3–6) when the potential of the tank is altered

by 0·2 volts. The total charge passing through the mirror in Experiments 3 and 6 would be the resultant effect of the whole system of harbour potential variation on the four knots of cable under observation, except in so far as the capacity of the cable seawards would modify the influence. In Experiments 4 and 5 the interference would, on the other hand, be due to electrostatic charge entering the observing circuit owing to the variation in the harbour potential, electrostatically produced by the loop of exciting cable traversing it. An external average variation of 0·01 volt in the potential of the basin would suffice to account for the degree of interference observed; and this amount of variation, it is evident, the conditions amply supply. Experiment 13, contrasted with Nos. 9 and 11, showed that the interference observed in cases 3, 4, 5, and 6 was of an electrostatic nature.

The sensitiveness of a cable to external electrostatic influence can be shown experimentally by joining up a mirror galvanometer in circuit with 100 miles of cable and earth. If the distant end be sealed, and laid for two or three feet in an insulated joint trough filled with water, the potential of which is capable of being varied suddenly through 100 volts (from + 50 to − 50), the effect is immediately visible on the galvanometer; and in fact, simple signals have been transmitted in this way from ship to shore through the coatings of a joint.

The hypothesis of an insulated or semi-insulated basin, therefore, satisfactorily explains all the phenomena observed. It accounts, first, for the meteorological disturbances which are observed on the cables, since any alteration of the charge or position of electrified clouds in the vicinity of the harbour may affect the distribution of charge in the cable to an extent that would not be possible if the harbour were in more perfect electrical communication with the ocean or earth's mass; and all the phenomena experimentally observed can be traced either to the direct or indirect effects of this imperfect earth connection—directly by conduction, indirectly by induction.

Assuming, then, that the Lower Cambrian rocks effectively insulate the Halifax basin, it is necessary to suppose that the same effect would be produced at any landing place on the

Atlantic shore of Nova Scotia, where these rocks so generally prevail. This is also corroborated by the fact that previously at Torbay some difficulty of the same kind appeared when attempts were originally made to utilise an ordinary earth connection for an overhead Morse circuit. The interference with the cables thus set up rendered a special earth necessary for the land line in the bay. The fact that no interference was visible between the cables themselves is accounted for by the open landing place and absence of any such basin as Halifax Harbour presents.

In Mr. James Graves's paper on earth vibrations, already alluded to, mention is made of the slaty character of the rock in the neighbourhood of the station, and so favours the supposition that the interference there observed was due to the same causes.

It is said that in the Lake Superior region of the United States great difficulty has occasionally been found in obtaining efficient earth, even when no local cause for the deficiency has been apparent. I am indebted to Professor Moses G. Farmer for the facts of a case which came under his own notice, where an earth connection, apparently good and well situated, was found useless, and measurement showed that its resistance to earth was 12,000 $\omega$. I also owe to him the information that interference due to imperfect earth was found in 1869 at St. Pierre Miquelon, the Morse signals on the St. Pierre Placentia circuit affecting the Atlantic cable to Brest, and that special measures had to be taken to effect a better earth connection.

Generalising from these facts, it would seem that there may be difficulty in obtaining good earth at cable stations opened on a shore of primary or metamorphic rock, and that permanent interference beyond remedy by earth connection is possible between cables landed in a basin of such formation; so that if this statement receive confirmation at the hands of the members of this Institution, it will be evident that a due regard will be given to geological conditions by electrical engineers among the many incidental considerations that determine their selection of a cable's landing place.

## APPENDIX.

On the resistance between two electrodes of active surface ($s$ and $s_1$) imbedded in the surface of an indefinitely extended homogeneous medium of specific resistance $\rho$ comprised between two unlimited parallel planes separated by a perpendicular distance $d$, the distance between the electrodes being represented by D.

Let $r$ and $r_1$ be the radii of the equivalent hemispheres exposing surfaces of $s$ and $s_1$ respectively. Then, if an infinitely thin perfectly conducting hemispherical shell of radius $d$ concentrically surrounds each of these electrodes, these shells will touch the opposite bounding plane of the medium, and the resistance between them and their included electrodes will be

$$\frac{\rho}{2\pi}\left(\frac{1}{r}-\frac{1}{d}\right) \text{ and } \frac{\rho}{2\pi}\left(\frac{1}{r_1}-\frac{1}{d}\right) \text{ respectively.}$$

If D be large compared with $d$, these two shells may be approximately considered as two parallel cylinders of radius $\frac{d}{2}$ traversing the medium at right angles to the bounding planes, and their surface potentials may be written*—

$$V_1 = \text{constant} - \frac{c\,\rho}{2\,\pi\,d}\log._\epsilon\left(\frac{\frac{d}{2}}{D}\right);$$

$$V_2 = \text{constant} - \frac{c\,\rho}{2\,\pi\,d}\log._\epsilon\left(\frac{D}{\frac{d}{2}}\right),$$

where $V_1$ and $V_2$ are the potentials and $c$ the strength of current.

$$\therefore V_1 - V_2 = \frac{c}{2\,\pi\,d}\left(\log.\frac{2\,D}{d} - \frac{\log.\,d}{2\,D}\right),$$

$$\therefore \text{Resistance} = \frac{\rho}{\pi\,d}\log._\epsilon\left(\frac{4\,D}{d}\right);$$

so that the total resistance,

$$\Omega = \frac{\rho}{2\,\pi}\left(\frac{1}{r}+\frac{1}{r_1}-\frac{2}{d}\right)+\frac{\rho}{\pi\,d}\log._\epsilon\left(\frac{4\,D}{d}\right);$$

and since $d$ is large,

$$\Omega = \frac{\rho}{\sqrt{2\,\pi}}\left(\frac{1}{\sqrt{s}}+\frac{1}{\sqrt{s_1}}\right)+\frac{\rho}{\pi\,d}\log._\epsilon\left(\frac{4\,D}{d}\right),$$

approximately.

* See Mascart and Joubert's "Electricity and Magnetism," vol. i., p. 205.

The CHAIRMAN: No doubt there are some submarine cable engineers present, and I think that perhaps the best thing to do would be for them to favour us with an account of any similar experience they may have had to that described in the paper, of signals on one cable being interfered with by currents on another cable, due possibly to imperfect earth. Professor Ayrton.

Mr. W. P. GRANVILLE: There are one or two paragraphs in this admirable paper which, I think, should be carefully considered. Mr. Kennelly states that "the Rye Beach shore end was at first "laid very near to the Irish cable, and actually over it at one or "two points." In that case you would certainly expect induction between the two parallel wires. He then goes on to say that "the cables were subsequently separated, *although they still* "*necessarily lie at different points in close proximity;*" and that "this separation of the cables was found to have sensibly lessened "their interference." Now, as the partial removal caused a considerable cessation of the disturbance, is it not probable that a further removal would practically eliminate the interference? Mr. Granville

Certain experiments are also described as having been made with the condensers removed, and it was then found that a feeble continuous current was set up in one cable circuit whenever the other was charged by the strong battery employed; this feeble current being indicated by a small permanent deflection on a sensitive galvanometer. This also, according to my experience, is not an exceptional effect; in fact, I do not believe, if two "earths" are in proximity, that you can charge the one without affecting the other, however good the "earths" may be, especially if no condenser is in the receiving circuit. It therefore appears that the phenomena here noticed may be fully accounted for without introducing the theory of an insulated basin.

It occurs to me that the practical difficulty experienced might be overcome by an additional coil to the speaking galvanometer of the Irish cable, preferably made so that its position relative to the ordinary coils could be readily altered, and thereby the strength of its effect upon the mirror magnet varied at will. If this extra coil were coupled up in the sending circuit of the Rye Beach cable, or as a "shunt" thereto, it is clear that the

Mr. Granville.

mirror would be affected in one direction by the disturbance referred to by Mr. Kennelly, and in the other by the current circulating in the extra coil; and these two effects could, I should imagine, by careful adjustment, be made to cancel each other, and a balance be thus obtained.

Mr. Donovan.

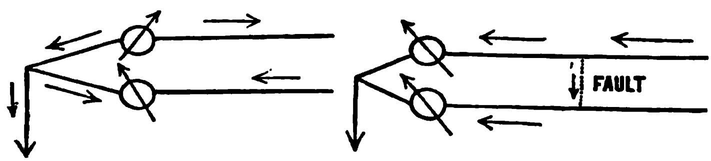
Mr. H. C. Donovan: I desire to compliment Mr. Kennelly on his able investigations into a phenomenon which has been a source of trouble to more than one station, to my knowledge. The late Professor Varley foresaw that geologic conditions should be taken into account in the working of long submarine cables. Consequently he took the precaution to have a special earth-wire laid from the cable-house to some distance out to sea. Conditions somewhat similar to Halifax existed at Valentia. The island is a slate rock. I am under the impression that this precautionary "earth" was not used, as the iron sheathing of the cables themselves afforded all that was required when working from the cable-house. When, however, the cables had subsequently to be worked from the office at Knightown, some five miles distance from the cable-house, the iron wires of the subterranean cables, which connected the shore ends with the office, were used as the "earth;" then—owing, I suppose, to the non-conductivity of the island—there were the same conditions, in a lesser degree, which, judging from Mr. Kennelly's paper, exist in Halifax Harbour, due, I imagine, to the impediment to the complete diffusion of the return currents, which were conducted both by the iron wires and the earth itself. A complete "earth" should be mass with the lowest possible resistance. The iron wires of a cable under conditions which exist in Halifax Harbour do not fulfil the conditions of a perfect "earth," hence the disturbances noticed, and so ably obviated by Mr. Kennelly.

Mr. Pitman.

Mr. C. E. Pitman, C.I.E.: The only case of an insulated earth I know of, occurred during one of our frontier expeditions. We were about to establish a temporary office at the close of the day, and selected a very damp, swampy place for the earth-plate, but to our horror we got no signals whatever; our line would not work in the least. On examining the nature of the ground in the

vicinity of the camp, we found that a few hundred yards away up the river the bank was formed of a shelving rock, which, passing under the camp, cropped out again some distance off in the opposite direction; but we soon got over the difficulty by hitching a wire on to the trees and running it back about half a mile. That is the only case I have met of a thoroughly insulated basin. Mr. Pitman.

Mr. W. H. PREECE: In the early days of telegraphy "bad "earths" were constantly cropping up, and one infallible mode of determining whether the disturbances were due to bad earth or to other causes was very easy when the double-needle telegraph instrument was used. The double-needle instrument required two wires, as per sketch, with virtually a galvanometer on each, Mr. Preece.




making earth. It was rare in those days to have an instrument always free from disturbance, contact, weather contact, or whatever it might be called: the slightest shower of rain in any part of the circuit would invariably cause disturbance from one needle to the other; the early letters of the alphabet—a, b, c, d, and so on—which were formed with one needle, were repeated on the other. Earth currents were also a constant source of trouble, as well as contacts from various causes, and the way in which we were able to at once say what was the nature of the fault was this: Suppose a current is sent upon one wire of the double needle, in one direction, it would divide itself, and the result would be that the two needles would be deflected in the same direction. With such a result one knows for a dead certainty that the fault is due to contact between the two wires. Suppose the disturbance caused the two needles to deflect in opposite directions, with an equal dead certainty one would know that the fault was a bad "earth;" so that by the direction of the two deflections the nature of the fault can be easily distinguished. The same practice is followed at the present day: if any one doubts the character of his "earth," he simply has to put two galvanometers in circuit in

Mr. Preece. the same way as they were used with double-needle instruments, and he will quickly see whether the "earth" is good or bad.

In Mr. Kennelly's very exhaustive and admirably worked out paper there are a lot of facts and plenty of food for thought, but there is one point which I have not detected in the paper: he does not appear to have made a particular test to ascertain for certain that the fault was a bad "earth;" he speaks merely throughout the paper of vibrations. His tests nearly throughout the whole paper are made with reversals; and if he had used reversals only, then he would have left us in very great doubts; but in the experiments about Nos. 7, 8, 9, he has used a constant current, and though he has given facts that lead us to think the cause to be something of the kind that he points to, he has not given us sufficient data to make one feel quite sure that he is right in his premises.

This question of bad "earth" was very thoroughly investigated and written about by Professor Fleeming Jenkin, and I am a little surprised to find that Mr. Kennelly was not acquainted with Professor Fleeming Jenkin's work in this matter. He mentions the fact that he received information from Professor Moses Farmer that the island of St. Pierre had a bad "earth;" but Professor Fleeming Jenkin read a capital paper, entitled "An "Insulated Island," a paper which is reprinted in the "Life" written by Stephenson (the second volume of which contains a synopsis of his works, collected and edited by Professor Ewing), and in which the investigations of Mr. Gott, the superintendent of the French Atlantic Company at St. Pierre, are given. Professor Fleeming Jenkin, in his remarks, leaves no doubt whatever that the "earth" was bad, and that St. Pierre is an insulated island.

Mr. Kennelly has also referred to Mr. James Graves's paper—Mr. Graves is still the superintendent of the Anglo-American Telegraph Company at Valentia—in which observations were described of a very curious phenomenon which was altogether aside from the subject of Mr. Kennelly's paper. Mr. J. Graves's observations were not primarily on a bad "earth," but on curious vibrations that were observed at the earth-plates, and he showed

that they were due to some defect in the earth-plate; and eventually—in the discussion, I think—it was pointed out that probably the results that he had observed were due to electro-polarisation of the earth-plates: that the earth-plates were too small; that the current decomposed the water, gases were formed on the surface, the gases as they moved away caused a variation in the resistance or potential of the plate, and so produced vibration. There is no doubt that the explanation was quite true, because the vibrations entirely ceased when the earth-plate was enlarged by using the outside sheath of the cable. I remember, too, a great many years ago now, a similar difficulty being met with at Torquay; it was described by Mr. Culley in this room. When the telegraph was first carried to Torquay, there was very great difficulty indeed in getting an "earth," and "earth" was only at last obtained by carrying a bare wire to sea. Mr. Preece:

In my own experience I can only call to mind a great difficulty being met with in the chalk districts of Hampshire, when the railway from Basingstoke to Salisbury was constructed. All through that district there was very great difficulty indeed in making earth—so great that between two of the stations a return wire had to be used. Whether the difficulty continues I do not know.

I should like to call attention to a point not alluded to by Mr. Kennelly—that is, the use of the telephone in detecting the nature and character of these vibrations. The telephone is an extremely delicate instrument of research; with constant practice its indications can be mastered with very much greater confidence than the indications of a galvanometer: you can tell with almost absolute certainty whether the indications are due to electro-magnetic induction or to electrostatic induction. It is wonderful how the telephone seems to speak to you in a complete language, which is only acquired by constant practice. You may remember that I brought before the British Association the subject, and went into the whole question of the disturbances that are occasioned between parallel wires—between wires that run in contiguity to each other—overland, underground, and at sea. I made a great number of experiments in

Mr. Preece. this direction. Some very accurate and very reliable experiments were made for me by Mr. Gavey in the neighbourhood of Cardiff, on the sands of Porthcawl, on the South Wales coast. There, as you know, the tide rises and falls enormously: the rise and fall in spring tides is something like 40 feet. I was very anxious to find out whether the water acted as a screen—whether it prevented the influence of one wire from affecting another wire in its neighbourhood—and the result of those experiments was to show that the water offered no obstruction whatever to the disturbance between one wire and another. If two wires were carried within, say, 20 feet of each other, in air or water, in the earth or under the earth, the effect was precisely the same. Experiments were also carried out by me in the General Post Office. Wires were arranged around the corridors of the different floors; the circuit was something like 400 feet long. There are four floors, and the currents sent on the top floor were clearly indicated in the circuits on every other floor; in fact, between the top floor, where the experiment was made, and the lower floor—a distance of 60 or 70 feet—we found that, in an electrical sense, "stone walls do not a prison make."

There are no known present means of preventing the production of these electro-magnetic and electrostatic disturbances through space. They are evidently, as we now know from the investigations of Hertz, conveyed through the ether; and whether the space be occupied by stone walls, whether it be the air, whether it be water, or whether it be the crust of the earth, the interposition of these elements does not affect the question at all. At Porthcurnow, on the Cornish coast, where the Eastern Telegraph Company have their landing place for cables to Lisbon,—where the cable to the Scilly Islands, the cable to Brest, and several other cables land or start from,—they are all within a short distance of each other. There Mr. Ash, the superintendent of the Eastern Company, found that currents sent on one wire were clearly and distinctly traceable on all the contiguous wires. He sent telephone currents on the Lisbon wire, and indications of them were distinctly heard on the Brest wire; so that there was absolute evidence in the character of the signals that the electro-magnetic disturbance passed through the body of the water.

I do not feel that Mr. Kennelly's paper is perfectly exhaustive, or that he has sufficiently investigated the electro-magnetic disturbance between one wire and another; for I think that if he had used telephones he might have added considerably to the value of his paper, and the result might have been negatived, though I will not say that it would have been so. He has, however, done all that one man can do to prove his point; but it is a very curious thing that when you have a point to prove, when you find an experiment to confirm your view of that point, you often, by sheer neglect, omit some very trifling thing that will entirely upset your notions: that happens over and over again. I ought to mention that there is a remarkable case of the propulsion, as it were, through water of an electric disturbance —I will not call it electro-magnetic, I believe it is electrostatic disturbance—that is, the existence of thunderstorms in mid-Atlantic. It is a common and frequent thing at the end of Atlantic cables to see sudden, sharp, decisive currents knock the mirror about or move the style of the recorder, and these things are known as "kicks." It is well known that they always occur when lightning is present. It very frequently happens that there may be a lightning storm a thousand miles away from land, and yet the disturbances produced by the lightning discharges pass through the 2,000 fathoms of water and there induce in the cable at the bottom of the sea the disturbances that produce the kicks at the landing place at each end. They are very interesting facts, and I think that, bearing the fact in mind that electro-magnetic and electrostatic disturbances can proceed through water as easily as they can through air, the effects that Mr. Kennelly has observed would be clearer if he had looked at them from this point of view more than from the resistance point of view. Mr. Preece.

Mr. A. J. S. Adams: The mass of formulæ and figures given in Mr. Kennelly's paper appear, to my mind, to be based upon practically nothing, and the consideration of them to have been a waste of time. I defy anyone to say, from the experiments mentioned in the paper, what the disturbances really were, or what it is intended that we are to consider them. There are Mr. Adams.

Mr. Adams. several possible causes of interruption — induction, leakage, derived currents from "bad earth," and vibrations. If we have a disturbance due to leakage, then there will be a more or less permanent effect so long as the key is depressed; whilst, if the disturbance be due to induction, the effects will be momentary. Surely it could have been possible to throw a little more light upon the point.

It happens that the vibration disturbances referred to, and which formed the subject of an interesting paper by Mr. James Graves in 1875, are no strangers to me, although, unfortunately, my own opinion as to probable causation differs from that of Mr. Preece, inasmuch as I am of opinion that these peculiar *vibrations* are not due to electrolytic nor to any electro-chemic action at the earth-plate.*

So far as time may permit, I should like to relate some of the experiments made by myself in connection with the subject, and which tend to show that the vibrations are not necessarily due to so-called bad earth at the plate, nor to mere electrolytic action.

At each extremity of my garden—north and south—an "earth" was put in, as also right and left of my house to distances of 250 and 500 yards respectively. Each "earth" was led into my house by means of No. 16 copper wire, without joint, thoroughly insulated, and brought directly on to the ebonite base of the reflecting galvanometer. I had thus a means for securing six different pairs of "earths."

Each "earth" was formed by bunching together and soldering one end of twenty 2-foot lengths of clean No. 16 copper wire, the line wire being included.

The holes were about 5 feet deep, and in each case the earth-wires were spread out from the common soldered centre. As the earth was filled in it was thoroughly punned.

Now in the case of each pair of "earths" these unmistakable disturbances were always visible, the jerky movement of the "spot" being altogether unlike the steady variation of an ordinary earth current.

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* The disturbance of a current, not the current itself, is here referred to.

Two other "earths" were also put in, the one 5 feet immediately below the other. Here also, in a minor degree, *vibrations* presented themselves, although the flooding of the locality with water had little influence upon the result. Mr. Adams.

In continuation of these experiments, a box 18 inches square was parted off into two compartments, space being provided at the bottom of the partition for proper connection between the two. Two copper plates were inserted, one in each compartment, and the whole filled in with sandy earth. By means of an india-rubber tube attached to the house tap, water was caused to dribble down the front of one plate and drain off under the other. Of course the usual electrolytic current was the result; and surely, if *vibrations* are the result of such action at the earth-plates, they ought to have appeared in this instance, but there was no sign of them. It was evident from this and from similar experiments that *vibrations* are not due to electro-chemic action at the earth-plate.*

It was a noteworthy feature of Mr. Graves's results that directly he made earth connection by means of the main cable-sheathing the vibrations ceased: the "earth" had been cut out, and practically a "return" substituted. And so, I take it, directly a foot even of the earth's surface is introduced into any circuit, so surely also will these vibrations present themselves.

In the paper read this evening it would seem that this vibration effect has been so mixed up with some, or all, of the other disturbances mentioned, as to render a serious consideration of the figures and formulæ given a waste of time, and it is to be regretted that the particular kind of disturbance witnessed in each experiment at Halifax was not stated clearly by the author of this interesting and important paper.

Mr. D. C. BATE: It may be interesting to state that on one occasion, when I was connected with the short-lived Central News Mr. Bate

* *Vibrations* are not observed in the case of ordinary electrolysis—the resultant current of which is steady in its effects—unless mechanically disturbed, as by a shake or jar. There is the possibility of a mechanical movement in the earth's crust as a cause for vibration; and although seemingly not calculated—if existing—to produce the necessary disturbance of earth-plate relationship, the point is worthy of being followed up in that direction.

r. Bate. Telegraph Company, we worked an overhead and underground wire, parallel with the Post Office wires, from Fleet Street to the *Manchester Guardian* office. The latter office, if I remember rightly, had an A B C instrument, and at night a news wire to Manchester. Our own wire was worked with about 200 volts through 3,000 ohms, and therefore had a pretty considerable current on. It used to work perfectly well at the opening, but as the evening wore on we used to upset the news wire relays both at Newcastle Street and at Manchester. Does not this, therefore, point a little to Mr. J. Graves being correct in saying that such faults may be due to the polarisation of the earth-plate, considering that we were using such a large current? Another explanation, possibly, is that we were connected to the house side of the water service (it is true there were several moderately good "earths" on, because a large number of water taps were supplied), and when the cistern was full of water we worked through almost without trouble; but if the cistern became empty, then the increased resistance of the earth, as might be expected, increased the difficulties, although still good enough for the small news current alone. The circumstance will probably be remembered by Mr. Preece, as I think we had considerable trouble to find out where the disturbance came from, and it was finally traced to the Central News as being the culprits.

. Swan. Mr. Howard Swan: It might be interesting to mention, in connection with the remarks that have just been made, a case that was reported a few months ago from Bridgewater, Nova Scotia, in which a telephone line was run in the gold-mining district. It was found that a constant current was flowing in the wire—not very strong, but sufficient sometimes to overpower the exchange battery—and it was difficult to find out wherein the cause consisted. The electrician who investigated the case seemed to believe that there was what he considered a vast natural battery, consisting of the gold mine as one pole, and the large deposits of iron or gold, or other metals, as the other pole. Of course, if this were so, it would be a battery on closed circuit, and most of the current would go through the earth, and part only of the current through the wire. From some reason or other

a current did go through the telephone wire, and there is, it is said, to this day a small constant current flowing through the wire without any battery except a natural one. That point is, I think, of interest in this discussion. Mr. Swan.

Mr. D. C. BATE: Might I ask M. Despointes, who I see is here, whether I am correct in my memory that when the Rev. Mr. Highton's gold-leaf receiver was placed on a faulty dead-earthed Dover-Calais cable, they got signals which were being sent over one of the North Sea cables? Mr. Bate.

M. DESPOINTES: I cannot say. I do not remember the matter; and, if tried, the experiment would be at Dover perhaps, while my duties have been in London only. M. Despointes.

Mr. C. T. FLEETWOOD: I remember making the "earth" for the London Central Station telegraph circuits, and I have always felt that it was an excellent one, because it has never been complained of since the time that it was made in 1874. That "earth" is made by a connection with the pneumatic tube system, which runs for some thirty-five miles under the London streets. I hoped that we should have heard more from Mr. Preece this evening, for I have heard of induction between overhead and underground wires, and in the paper referred to by Mr. Preece mention is made of induction being heard in wires half a mile apart. Surely, if this is so, it would be well for Mr. Kennelly to use a telephone and further investigate the case that he has in hand. Mr. Fleetwood.

Mr. W. H. PREECE: I should like just to mention some experiments that I made in order to detect the presence, if any, of disturbance between two cables separated from each other by a distance of twenty-five miles. One cable was the old cable connecting Dublin and Holyhead; the other was a new cable laid only two or three years ago between Nevin, in North Wales, and Newcastle, near Wicklow. Those two cables lie parallel to each other at a distance of twenty-five miles apart; they are each about sixty miles long. I had very powerful currents sent on the Dublin-Holy head cable, and listened for the signals on the other cable. In telephonic experiments I never trust to my own judgment, and I do not think anybody should ever rely on his own Mr. Preece

Mr. Preece. judgment with telephones—it should always be confirmed by somebody else. It is perfectly absurd how your imagination leads you adrift with telephones, and therefore, unless observations are confirmed by those who are with me, I do not make use of them. In this particular instance I am bound to say that I knew what I was going to hear, and I am equally bound to say that I heard what I expected to hear, but nobody else did, although there were three accomplished engineers and electricians assisting in the experiment; but they did not quite know what they were to listen for. My imagination may have put me wrong in this case; but I have not the slightest doubt that the effect could be heard, and that signals sent on one cable could be heard on another cable separated from it by water at a distance of twenty-five miles. It depends on the magnitude of the primary currents.

Mr. Crawley. Mr. C. W. S. Crawley: I had some experience of earth-current disturbances when investigating with my friend Mr. Hawes the currents due to passing trains. In conjunction with these we had frequent occasion to observe the currents between two zinc earth-plates some 200 yards apart, as well as those between these plates and the rails. These currents were in a constant state of variation. Not only, as shown by the galvanometer, did they vary in waves of considerable amplitude—say one to two minutes between the crests—but there was also a constant bubbling when a telephone was substituted, showing continuous quicker vibrations to be present. These we could hardly put down to conduction or induction from neighbouring telegraph wires, as the bubbling was equally noticeable at 2 and 3 a.m., and all Sunday, when few or no messages would be about. The telephone would always bubble if set fine enough, even between "earths" a few feet apart.

Mr. Granville. Mr. W. P. Granville: With regard to Mr. Preece's remarks upon the sensitiveness and suitability of the telephone for researches of this kind, I have had the opportunity of listening to a telephone joined in circuit with two earth-plates submerged in the Thames about 100 fathoms apart. On putting the telephone to my ear, it became apparent that the river was perfectly

teeming with Morse alphabets, and at all times of the day the rapid clicks of Morse instruments could be heard. I am not a rapid reader by sound, but I could now and then manage to interpret a word, or even part of a sentence. Mr. Granville

The CHAIRMAN: The first duty, in closing this interesting discussion, is to propose a hearty vote of thanks to Mr. Kennelly for the paper that he has been so good as to send us from America. As some of the speakers have pointed out, there seems to be some little doubt—possibly some would say much doubt—as to the cause of the effects which Mr. Kennelly has observed. The importance of taking an earth-wire out to sea has been urged by some of the speakers, but it must be remembered that when we use the sheathing of a cable as our earth we do really take an earth-wire out to sea; so that I hardly see how such an earth-wire could improve matters, especially as the cable which was taken out to sea did so little to improve matters that Mr. Kennelly came to the conclusion that the whole basin is insulated. Some of his experiments, however, do not seem to be consistent with, and are not explained by, that hypothesis. Take, for instance, Experiment No. 9. He says: "The last two "experiments were repeated with the condenser removed and "the mirror in direct circuit between cable and sheathing. The "continual vibration of the spot made observation difficult; but "during periods of comparative quiescence it was soon deter-"mined that the disturbance produced by depressing one of the "keys was not of a momentary nature only, but consisted of a "small deflection, permanent during the whole period of key "application." I cannot see how that experiment can be explained by any want of goodness of the earth; how it is possible in Figs. 7 and 8 to get a steady current on the other cable which is not connected with the battery. He says you do, but I do not see how the absence of good earth would explain that. Take the case, again, of Experiment No. 7, where the mirror galvanometer is connected with the copper conductor which goes to Ireland. At the other end of that circuit, he mentions somewhere else in the paper, there is a condenser; so that in fact the copper conductor going to Ireland is well insulated, or assumed to be Professor Ayrton.

Professor Ayrton.

well insulated. How, then, a steady current can be prod through that conductor, assuming that the cable is well insul by a defect of the earth at Nova Scotia, I do not at all Some of the experiments do seem more or less consistent bad earth, but these others are not; so, if I am supposed in way, as Chairman, to give a verdict on the discussion, I can say that my judgment will have to be reserved until fu evidence is brought forward. Really my own mind is undec I would not go as far as some speakers appear to have go concluding that Mr. Kennelly is certainly wrong; at the time I would not go as far as Mr. Kennelly has gone in assu that he is perfectly right. I will therefore leave the matte in the case of a Scotch verdict, in a state of "not proven."

A ballot took place, at which the following were elected:—

*Foreign Members:*

Colonel Huber. | A. Paoletti.

*Members:*

Emile Garcke. | Willoughby Statham Smi

*Associates:*

Samuel Wells Cuttriss.
Lawrence H. S. Ellson.
Charles E. Hodgkin.
Arthur Hough.
Robert Edward Pemberton Pigott.
Frederick Arthur Pock lington.
Robert Hodgshon Postle thwaite.
George Edward Pritchett.
William Stevenson.
Alfred Mills Taylor.

*Students:*

George William Bousfield. | Edwin S. Jacob.
John Leonard Thomson.

The One Hundred and Eighty-seventh Ordinary General Meeting of the Institution was held at the Institution of Civil Engineers, on Thursday evening, February 28th, 1889—Dr. J. HOPKINSON, F.R.S., Vice-President, in the Chair.

The minutes of the Ordinary General Meeting held on February 21st were read and confirmed.

The following transfers were announced as having been approved by the Council:—

From the class of Students to the class of Associates—

William Peto. | Frank Taylor.

Donations to the Library were announced as having been received since the last meeting from S. S. Wheeler, Esq., the Proprietors of *The Electrician*, and E. Hospitalier, Foreign Member, to whom the thanks of the meeting were duly accorded.

The following paper was then read:—

## SOME ELECTRIC LIGHTING CENTRAL STATIONS IN EUROPE, AND THEIR LESSONS.

By Professor GEORGE FORBES, F.R.SS. (L. & E.), Member.

### *Part I.*

Before commencing the subject of this paper, I think it may be well if I give you some idea of the reasons why I have brought the subject to your notice. You are aware that great strides are about to be made at the present time in electric lighting in this country, specially by means of the Gaulard & Gibbs system with the converters put in parallel. To complete the practical details of such a system involves a large amount of experiment, and whether low-pressure or high-pressure work is in question, I have

frequently maintained that a great deal of experience has still to be bought and time to be wasted. It has appeared to me that we can only hope to ensure the best success of electric lighting schemes now in hand if we bury our pride a little, and try to obtain the experience of foreign countries without paying for it or wasting time over it, as they have done. Some irresponsible persons have considered that I am much to blame for thus advocating the discussion, and consequent approval or disapproval, of systems which have been developed in foreign countries. I do not say that we are not even now capable of doing much better work than these countries, as we have done with steam, and I am certain that we shall do so in the course of time; but I do say that we are wanting in the experience, and I see all over the country work being done where the rules of experience have often been set at defiance; and my object in introducing this subject is chiefly for the second part of the title of the paper, namely, a discussion of the lessons which can be derived from a study of the work which has been done abroad. The object of this paper, then, is to raise a discussion on the details of central station lighting and electrical distribution, and to enter a protest against defective systems, which, without some such discussion, I feel sure will be as plentiful in the near future with us as they have been up to the present time.

Of course I am aware that many of my hearers have appreciated these points, and have, like me, tried to learn from those who have had most experience; and I know that they will pardon me for drawing attention to work that they have already examined, since they know as well as I do the need we have for such discussion.

I believe that my object can be best attained by proceeding, first, to describe the principal central stations in Europe which I have lately visited, pointing out specially the features in which they differ from the practice in this country so far as hitherto developed; and then to discuss the details, with a view of deriving the most profitable lessons from them as to what to adopt and what to avoid. I will deal only with what I believe to be the three most important central stations in Europe, namely, Berlin,

Rome, and Milan. I have already partially dealt with America elsewhere.*

Berlin is supplied on the low-tension direct-current system, as originally developed by Edison, except the street arc lights, which are on a high-pressure direct-current circuit.

Rome is supplied altogether, both for arcs and incandescent, on the Gaulard & Gibbs system, with converters arranged in parallel.

Milan is worked by Thomson-Houston apparatus for arcs, and by both low-pressure currents and high-pressure alternating currents for incandescent lamps.

At all the European stations which I have visited I received the same cordial welcome which I have always had in America, and the same desire that I should thoroughly inspect every part of the works. The stations have literally been placed at my disposal to watch the action of the machinery and to see what attention is required for its management. I have been allowed to interchange dynamos at will, and generally to learn all that was possible. I must especially record my thanks to Signor Pouchain, Professor Mengarini, Signor Trenza, Professor Colombo, Mr. Lieb, Herr Rathenau, and Herr Datterer for the willingness with which they imparted to me so much information in the details of their working, and to Mr. Zipernowski and Messrs. Ganz & Co. for much valuable information.

## Berlin.

The Berlin mains are at present, I believe, supplying a larger number of lights than any other connected system of mains in Europe. They supply 36,000 lamps of 16 candle-power, or the equivalent current, and 144 arc lamps of 15 amperes. The conductors are all laid underground. A map is shown (Plate 1, Fig. 1) giving the position of mains already laid down and those in process of construction. There are three central stations at work, and two more of improved type are in process of construction. The existing stations are—

* British Association, 1888.

1. Markgrafen Strasse.
2. Mauer Strasse.
3. Friedrich Strasse.

Those about to be added are—

4. Spandauer Strasse.
5. Schiffbauer Damm.

The first is the most important; the third is quite unimportant. Here the boilers are by Steinmiller, and anthracite coal is used to prevent nuisance from smoke. The foundations are of concrete in iron boxes built up with brickwork and cement. There are six engines of 160 horse-power each, each driving old-fashioned Edison six-legged dynamos. There are also four engines of 400 horse-power each, each driving a new type of dynamo direct at 80 revolutions per minute. These are compound tandem Corliss condensing engines by Van den Kirchove, of Ghent, who is one of the few foreign makers of engines who can compete with this country. The attached dynamos are by Siemens & Halske. The Electric Company of Berlin are also making a machine somewhat similar. A diagram (Fig. 1) is shown of this dynamo and of the engine combined, the height of which is 23 feet. These continuous-current dynamos have never been built in this country, and are worthy of some notice. They may be described as multipolar Gramme machines, the poles being radial, and the ring armature external to the poles. A photograph of the four engines and dynamos is shown (Plate 2, Fig. 1).

There are ten poles on each machine. The armature is 3 metres in diameter; the commutator is 1½ metres in diameter. There are ten brush-holders. Each pair of poles, and the corresponding part of the armature, with the corresponding pair of brush-holders, constitute a section which is virtually one machine. All these sections are joined in parallel. The field magnets are not seen in the drawing, being inside the armature. Two handles are provided, one of which adjusts all the brushes at the same time; the other puts them on all together. Each brush-holder carries four brushes. Another drawing is shown of a similar dynamo being made by the Berliner Elektricitäts-Werke, in which

the radial poles are external to the armature. I believe that this type of multipolar machine is a good design where the object

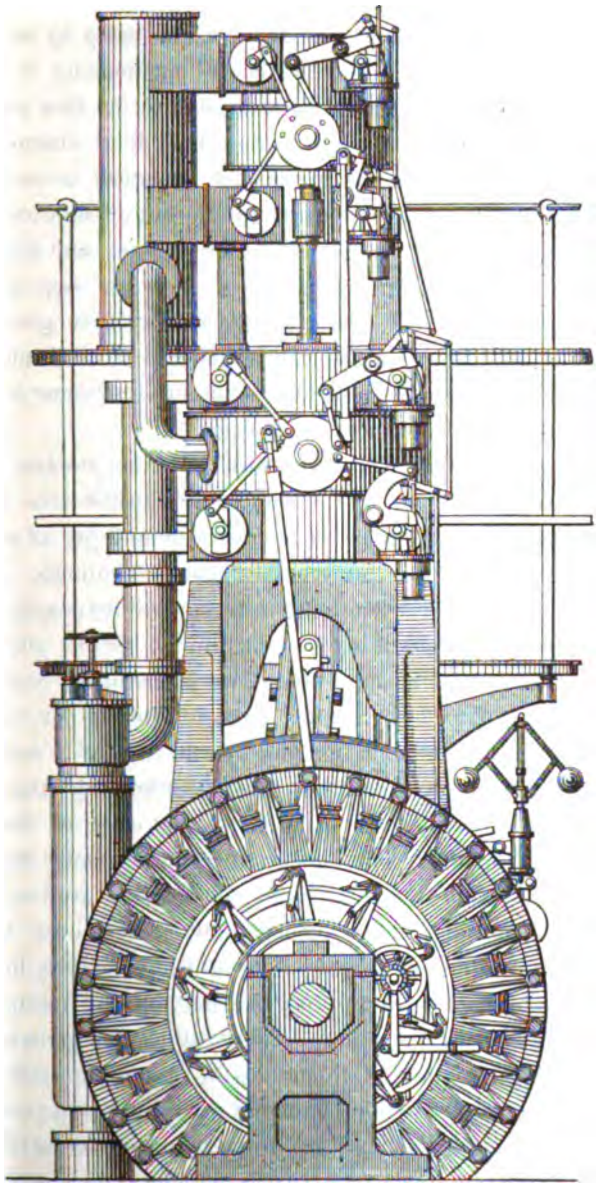

Fig. 1.—Engine and Dynamo, Berlin Central Station. (Scale, 1 to 48.)

is to get a large output with a slow speed. I hold working drawings in my hand of a similar design prepared by myself in

1881, but it was never executed. The number of bars of the commutator and the number of parts generally make it expensive to construct.

At this station the boilers are fed automatically by means of a hydraulic accumulator. The water for condensing is drawn from fifteen artesian wells which have been sunk for this purpose. Steam is used at a pressure of 115 lbs., and it is claimed that 15·5 lbs. of steam only are required per electrical horse-power. This is probably the most economical production of electricity for central station work in the world, and the engines and dynamos, though expensive, must certainly have a very low depreciation. The maximum work which this station can do is to generate a current for 26,000 lamps of 16 candle-power. It is found that the maximum number of lamps in use at any one time is about 66 per cent. of the total number of lamps.

The thing which strikes one most in this station is the enormous mass of copper. The conductors inside this station consist of eight strips of copper, 6 inches wide and ⅓rd of an inch thick. These are led into the room for cable terminals. These strips, before entering this room, are interrupted by a gap, and a junction is made by pinching seven similar strips, about 18 inches long, between the strips and the interrupted ends, the seven strips being placed between. Thus, in case of any serious accident, the whole of the mains can, by one turn of a screw, be disconnected from the dynamos, the interlapping pieces all dropping out. The only exception is in the case of theatres, where, to prevent panic, a separate connection is made with the dynamos. In the cable terminal room the positive and negative mains run horizontally, and vertical rods come down to the different cables. There are eighty-four of these cables, forming forty-two pairs of feeders supplying the network of mains with current on the two-wire system. These cables are generally of large size, being frequently 3 inches in diameter complete. They consist of stranded cables covered with jute prepared with bituminous compound, enclosed in lead, then covered with tape and preservative compound, and finally armour-plated with two crossed spirals of iron ribbon. The system of feeders and mains

is one of the features to which I wish to direct the special attention of English engineers, since the principle has been adopted of trying to produce as small a variation of pressure in the mains as possible. This is accomplished by having a large number of feeders. The cost of the underground cables has hitherto amounted to about £90,000, half of this being spent in mains and half in feeders. The greatest variation of pressure allowed in the mains is 1½ per cent. The loss of pressure in the feeders during periods of maximum supply is 15 volts.

The next point which I wish to draw special attention to in connection with the Berlin central station work is the type of cable which I have now described. This cable succeeded very well for three years, but lately they have been giving way very generally. The lead gets eaten into and water percolates to the copper, which is then destroyed. At first it was supposed that the deterioration came from the cable being pierced by the borers used by the gas people, but it was found that the same fault is produced even in iron pipes which have remained uninjured. It is supposed that the deterioration of the lead may be due in some way to its forming a galvanic element with the iron sheathing. Whatever the cause may be, the fact seems to be established that such cable will not stand underground electric light work for more than about three years. These cables generally run under the footways without any casing, and are connected by circular junction boxes to the houses, powerful clamps and no soldering being used for the contacts. All the dynamos feed direct on to the mains in parallel. One feeder alone, serving the mains close to the station, has a resistance in circuit in the station to equalise its loss of pressure to the others.

A few detached facts strike me as being worth noting.

Each 16 candle-power lamp uses 50 to 55 watts.

The charges for electric supply are 6s. per lamp per annum, and 9·6d. per Board of Trade unit.

The Aron meter is universally employed.

The price of gas in Berlin is 4s. 10d. per 1,000 cubic feet.

The distances from central stations to lamps extend to 1,000 or 1,200 yards.

100 kilometres of cable have been laid on the two-wire system.

Each of the large machines can supply 3,000 to 3,500 lamps of 16 candle-power.

The largest cable is 1½ square inches of copper in section.

I spent a long time in this station, and had machines taken off and put on—an operation which occupied two or three minutes. I took careful note of the amount of skilled attention which was required, and saw that it was considerable. A man of high training, accustomed to the use of scientific instruments, is always in attendance and looking after the amperes and volts from the different machines and pilot wires, and recording the measurements. It struck me that the working of this station was thoroughly German—certainly not American—but it was most methodical and very effective. One of the most interesting parts of this central station is the switch-board, with the special means for introducing a fresh dynamo in parallel, or cutting it out. This also is a point to which sufficient attention has not generally been given in England. In all foreign stations that I have ever seen, when a current of over 100 amperes is switched in or out, it is found necessary to make use of an auxiliary load in the form of resistances, a bank of lamps being generally employed as being the cheapest resistance available. When a new dynamo is going to be introduced in parallel, it is first worked on an artificial load, and the exciting current regulated until the volts and amperes agree with those of the other dynamos with which it is to be put in parallel. As soon as this equilibrium is attained the mains are switched into connection with the new dynamo and its load. After that the load is taken off. This prevents the possibility of the dynamo thus introduced being caused to run as a motor by the current from the other dynamos, and prevents too violent a sparking at its brushes. The switch-board at this central station for the four large dynamos, with the accompanying artificial load or resistance, and with its ammeters and voltmeters, is very perfect. Facing the operator, the switch-board is in four sections for the four dynamos. Each dynamo has three massive double-pole switches—one for the dynamo, one

for the artificial resistance, and one for the mains. Plugs can be inserted for varying the resistance on the artificial load. The ammeter is placed above these switches for each machine, and a voltmeter can be attached to any one of them, as required. A photograph of this switch-board is reproduced (Plate 2, Fig. 2).

The second station is in the Mauer Strasse. This station helps to feed the mains for incandescent lamps. It also supplies arc lights arranged in twelve circuits all arranged in parallel, each circuit having twelve arc lamps of 14 to 15 amperes. Here three Siemens machines of the old type are driven by a counter-shaft. Three Siemens & Halske new-type multipolar machines are also at work, and four Edison machines and six multipolar machines of low tension. 11,000 lamps of 16 candle-power can be supplied besides the arcs.

Sixty-eight feeders, or thirty-four pairs, start from this station.

The Friedrich Strasse station is small, and was the commencement of the present system. Here four six-legged old-type Edison machines, each of 75 horse-power, are driven by Armington-Sims engines, and feed the general system of mains. Fifty-two men are employed on the three stations in eight-hour shifts. The Berliner Elektricitäts-Werke paid last year a dividend of 5 per cent.; the Allgemeine Elektricitäts Gesellschaft, which does the central station work, paid last year 7½ per cent. In special districts 800 or 900 lamp-hours per annum are used; in residences only 400 to 500 lamp-hours per annum. Load diagrams showing the current used at different hours of the day in each month, and the proportionate cost from different causes, fixed and variable, have already been published, but are here reproduced (Plate 3).

## MILAN.

The central station in Milan is not of the highest importance so far as we can learn anything from it, except on one important point. The principal streets throughout the whole town are lighted by Thomson-Houston arc lights to the number of 350. This lighting gives us some idea of the manner in which this metropolis will soon be lighted; and anyone who sees it and

realises that this clear, white, brilliant illumination may be made to brighten our wider thoroughfares and to penetrate our darkest slums, will realise that when London is as far advanced in adapting these possibilities of civilisation as Milan is, we shall lead healthier, happier, and purer lives, and our streets will be less notorious throughout the world as the hotbeds of immorality and midnight assassination.

I will briefly refer to the points which struck me as being most worthy of notice.

The station has 14,000 incandescent lamps of 16 candle-power. The maximum load is 60 per cent. of the number of lights installed.

At this station it has been found quite practicable to use Siemens ratchet arc lamps of only 4½ amperes, the upper carbon being 12 mm. diameter and 200 mm. long, the lower one 7 mm. diameter and 200 mm. long, lasting 7½ to 8 hours. For longer duration larger carbons are used up to 15 hours. This is the first station where I have seen an arc lamp of so low a current in use. A certain resistance is put in series with the lamp, and two lamps are put in series on the mains feeding the Edison lamps.

The La Scala Theatre has 2,800 glow lamps of 16 candles and 30 arcs on the same mains.

Edison meters are used and have given great satisfaction. The price is fixed on a sliding scale varying from 7d. to 1s. per Board of Trade unit. Gas now costs about 6s. 9d. the 1,000 feet. Before the electric light was introduced it was 10s. 6d. the 1,000 feet.

Two of the theatres are supplied by two Zipernowski machines, each of 40 amperes and 2,000 volts. For these circuits Siemens' concentric cables, of which a specimen is exhibited here, are used, the section being 28 square millimetres. Four Thomson-Houston machines are supplying 35 arc lights each, and four more are supplying 30. Half of these are single and half double carbon lamps, the single ones being put out at midnight.

I should mention that the coal used costs 27s. 6d. per ton, but labour is cheap.



Ten Babcock & Wilcox boilers supply the steam. The engines

are all non-condensing. The low-pressure mains are on a two-wire system. Twenty-eight cables, or fourteen pair of feeders, supply current to the mains. No pilot wires are used, but a man reads the ampere-meter and sets the volts by a table. There are ten Edison machines of what is known as the Jumbo type feeding mains on the two-wire system. They were originally supplied each with a separate regulator for adjusting the exciting current, but connecting gear has now been introduced which works them all simultaneously. A fan is provided for driving air through holes in the poles of the field magnets. The whole of the low-tension system is antiquated, and we have little to learn from it except what to avoid. I found the arrangements for switching dynamos in and out worked very well. An artificial load of lamps is used at one part of the room, a main going thence past each dynamo. Thus two men are required to put in a new dynamo. It is first put on to the lamps, the machine being excited to the same extent as the other ones. The load of lamps is changed until the volts are the same as on the working circuit. A signal is then given, and the dynamo with its load is switched on to the working mains. The load of lamps is then removed. This is simpler than at Berlin, owing to the fact that the exciting current is known beforehand, but less simple owing to the dynamo switches being distant from the lamps and voltmeters. Ten machines work thus in parallel without trouble, which is a very good performance.

At Milan the average cost for wiring houses is only £1 per lamp. Wages are one-fifth of the total working expenses, coal is one-half, lamp renewals (undertaken by the company) is 7 per cent. The capital is £120,000, of which £24,000 has been spent in mains. The company has paid a dividend for several years. It is steadily increasing, and was 4 per cent. last year. There is a large reserve fund.

Plans are exhibited, one showing the positions of the arc lamps, and also the alternate-current circuits (Plate 4, Fig. 1), and the other showing the low-pressure mains and feeders, with the junction boxes and distributing boxes (Plate 4, Fig. 2). Another drawing shows the station in section, with the engines

and dynamos on the ground floor, and the boilers on the first floor (Plate 4, Fig. 3). Another shows the plan of the engine and dynamo room (Plate 4, Fig. 4).

## Rome.

The station at Rome, started by the gas company in that city, is the finest example of an alternating-current central station which I have yet seen. But it has been very costly to establish. Many of the details contain hints of the greatest use to those who are at work on this subject. The whole plant has been contracted for by Messrs. Ganz, of Buda-Pesth.

A general plan is shown (Plate 1, Fig. 2) of the electrical plant in this central station—all on the ground floor—the different parts being indicated by the following numbers and letters :—

Nos. 1 to 8.—Eight Babcock & Wilcox boilers, of 164 H.P. each, already in place. Total, 1,312 H.P.

Nos. 9 to 14.—Space where six other boilers, similar to above, will be erected.

A and B.—Two self-exciting dynamos, 2,000 volts × 45 amperes, and 150-H.P. engines.

I. and II.—Two dynamos, 2,000 volts × 160 amperes, and 600-H.P. engines.

III. and IV.—Space for two other dynamos and engines similar to I. and II.

*n*, *n*, *n*.—Three exciters, and Westinghouse engines.

C.—Place where are four feed-pumps for boilers.

M.—One 50-H.P. gas engine, with dynamo, 30,000 watts, and exciter.

There are two self-exciting alternators of 150 horse-power each, and two independently excited ones of 600 horse-power each. These give 42 amperes and 160 amperes respectively, and 2,000 volts. At present 9,000 glow lamps of 16-candle power are supplied, and over 200 arc lamps. All the machines are capable of working in parallel, even the large and small ones together. The number of alternations is 5,000 per minute, or 2,500 complete periods. The greatest distance to which current is at present supplied is 4½ kilometres, or about 3 miles. The feeders all go to

the Piazza Venezia, and then branch off into three mains in different directions (Plate 1, Fig 3). The loss in the secondary circuits due to loss in the feeders at present is only 0·6 volt at the maximum, and that due to the mains is the same, the length of feeders being 1,600 metres.

Siemens' concentric conductors are used throughout. The feeders are each 220 square millimetres section, or 0·35 inch. Drawings of the junction boxes by Siemens & Halske are shown (Fig. 2), indicating the means of attachment by clamps of the

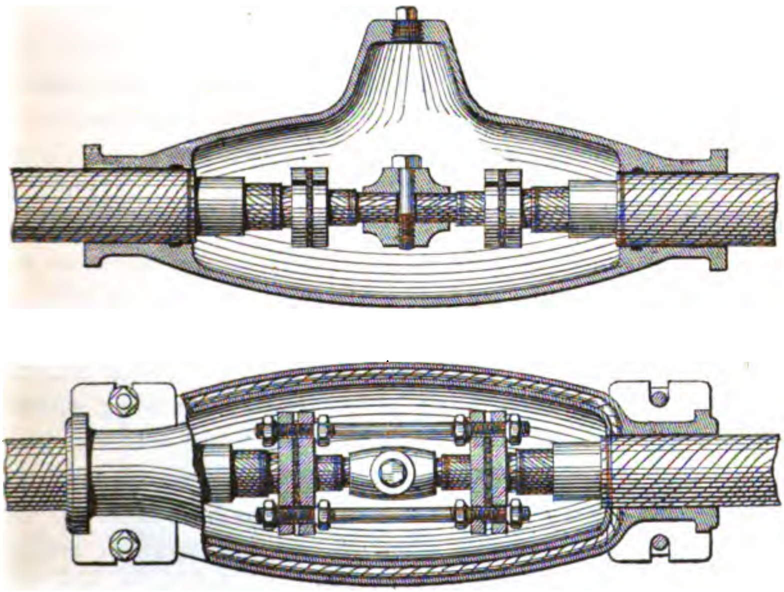

Fig. 2.—Junction Box by Siemens & Halske.

two ends of the inner and outer concentric cores. The cables are laid in a wooden box, which is then filled with cement. 17 kilometres of cable have been laid up to January, 1889. The feeders have been working since September, 1887. The converters are all of 10 horse-power, with about 4 amperes in the primary and 75 in the secondary. 110 volts are used in the secondary, but the secondary coil has three terminals, one half-way along the length of wire. This is to enable a three-wire system to be used for the secondaries when arc lamps are used, consuming, with their resistances, 55 volts each.

The old type of Zipernowski converter, with iron wire wound round the copper coils, has been abolished. The iron part is built up of circular flat ring-shaped discs of iron. These are firmly clamped together by iron clamps, which form the support of the instrument. The copper coils are wound upon the segments of the rings between the clamps. The primary coil is underneath the secondary. The converter has an iron circular disc at top and bottom fixed to the clamps, and this enables the instrument to be easily rolled about without injury, which facilitates handling. On the top is a porcelain disc, on which are placed the terminals and fusible cut-outs. These are short strips of thin metal held in pasteboard, and can be slipped in and out with the greatest ease. Drawings of these converters are shown (Figs. 3 and 4).

The small machines are intended to work 1,000 lamps of 60 watts. There are twenty poles, and 250 revolutions per minute. A derivation is taken from a commutator to excite the machine. Sulze engines are used for these dynamos, acting direct on the dynamos. The steam pressure is 120 lbs. A photograph of these machines and engines is shown (Plate 2, Fig. 3).

The automatic resistance for regulating the exciting current, invented by M. Blathy, is very effective. The exciting current passes through a solenoid actuating an iron rod which is supported at one end of a balanced lever. The bar is weighted with a float in water, and water can be put into the float. On the top of the iron bar is a cup of mercury which, in falling, breaks contact with the ends of wires dipping down into the mercury cup. These ends are at different depths, so that the wires are cut out of circuit in succession. The wires go to resistances. This automatic adjustment acts well over a considerable range.

The exciters for the large dynamos are driven by Westinghouse engines. They are governed by hand for the large changes, and then the automatic regulation comes in. Such a method of regulation is necessary with machines having so large an amount of self-induction. This is the only satisfactory regulation of the kind which I have seen at work. It is one of the small details on which the success of a station depends, but

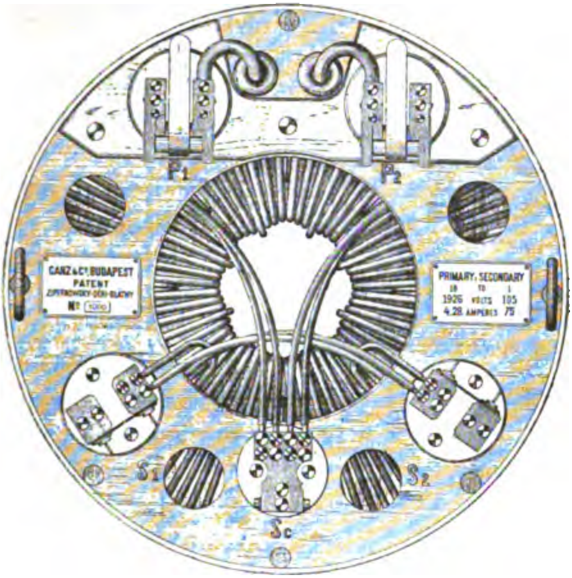


**Fig. 3.—Zipernowski 7·5-Unit Converter.** (Scale $1\frac{1}{16}$ inches to the foot.)

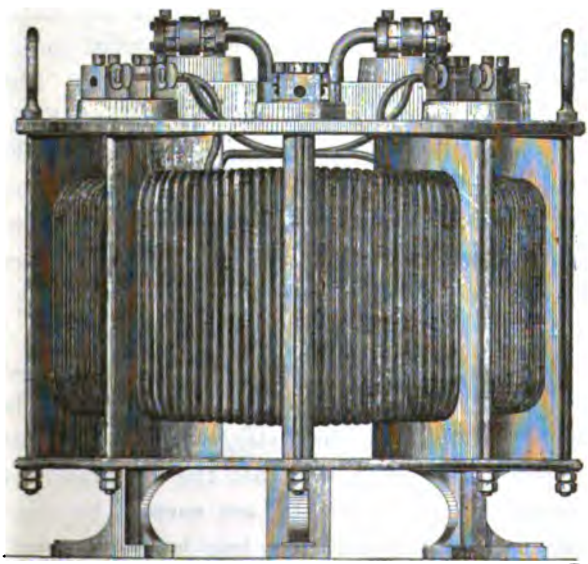

**Fig. 4.—Zipernowski 7·5-Unit Converter.** (Scale $1\frac{1}{16}$ inches to the foot.)

which are often forgotten by the inexperienced until the want is felt.

The two large machines have forty poles each on the revolving field magnets. Both field magnets and armature are divided into two series in parallel. When the machine is illuminated by an arc light, to which it supplies the current, a curious optical effect is produced. The arc being periodically made and broken, the revolving magnet-poles are seen fixed in position, and the amount of lag with different loads can be seen directly. The efficiency of these machines is said to be 90 per cent., including the exciting current. These machines are so constructed that the cover which contains the armature coils can be drawn away, by means of a handle, shown in the photographs exhibited, over the electro-magnet ring, without the necessity of using any hoisting tackle or other machinery. Moreover, the construction of the fixed armature is such that any single coil can be removed in a few minutes, or exchanged. The iron of a section of the armature is in this case removed with the coil. The iron plates of which these armature sections are made are T-shaped, so that the centre of the coil is filled with iron. In the smaller machines there is not nearly so much iron in the coils. In the 600-horse-power machines the energy absorbed in exciting at full load is said to be $3\frac{1}{3}$ per cent. of the full load. Photographs are shown of the large engines and dynamos (Plate 2, Figs. 4 and 5).

The T-shaped iron plates of the armature ($K^1$) (Fig. 5) are pressed together by strong bronze press plates ($S^1$, $S^2$) and a screw ($C''$). The wire bobbin is then slid on and held by screws. These segments are fastened to two traverses (T) by the screws (R), which traverses are in turn screwed with an insulating layer (U) to the side-plates of the framework of the machine.

The field-magnet wheel is constructed as follows:—U-shaped soft iron sheets (K) are so arranged together as to form a star, to which is added a similar star, with an insulating layer between, placed in such a way that the interstices between the sheets composing the lower star are covered by the sheets of the upper star. In this way many iron laminæ are put one upon the other until there are sufficient to form the whole electro-

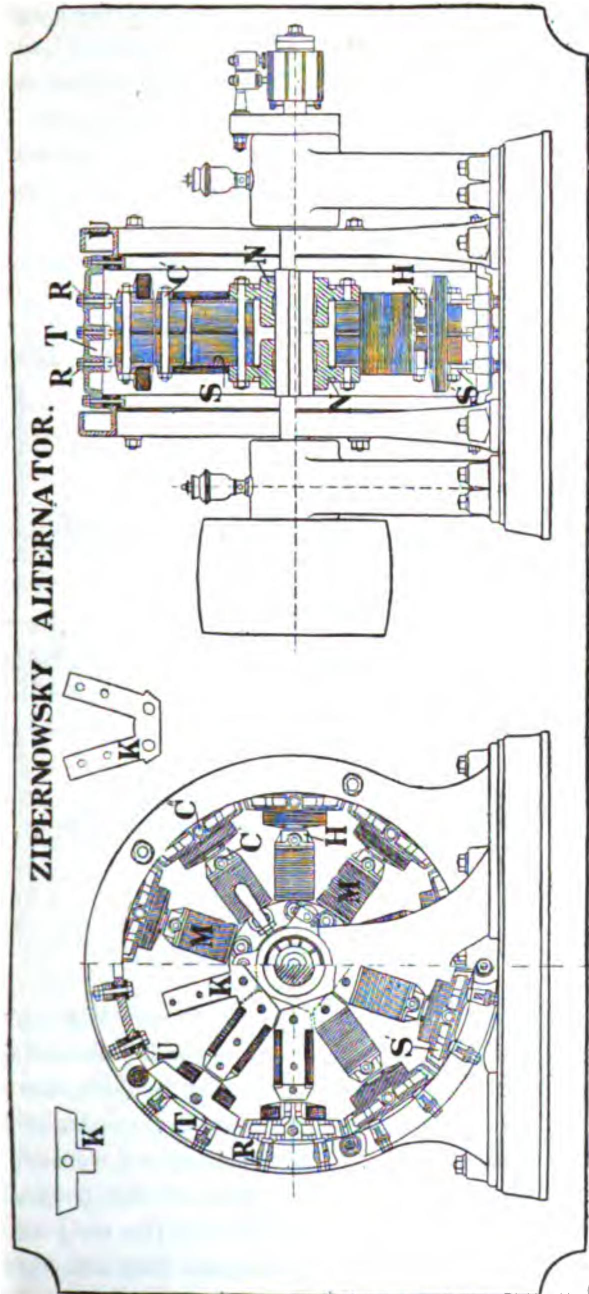


Fig. 5.

magnet wheel. The pile so formed is then pressed together to form a compact whole by means of two stiff discs (S), two bosses (N), and the screws (C). The bobbins (M) are slipped on to the core (K), and held down by means of the bobbin-holder (H) and screws (C′). The subdivision of the iron in the field magnets is necessitated by the teeth of iron in the armature projecting inwards through the coils.

The large engines are by Ganz & Co. They are carefully balanced, and are provided with starting gear.

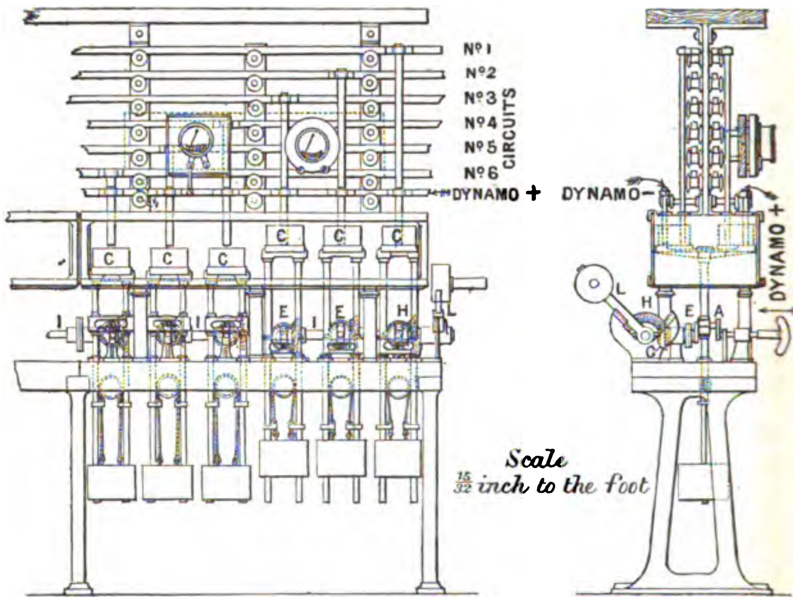


Fig. 6.—Rome—Mercury Switch-Board.

A drawing is exhibited of the switch-board (Fig. 6), which is very ingenious, but very expensive. The board is about 8 feet high and 17 feet long. It is divided into four sections, one for each dynamo, and each section is divided into six, one for each pair of feeders. A pair of 4-foot horizontal bars runs along each dynamo section, connected with the two poles of the dynamo. Two vertical bars descend from these at each of the six parts allotted to the six circuits. Six pair of horizontal bars are connected to the six pair of feeders (only three are as yet in use), these pairs

being one over the other. At each feeder section a pair of vertical copper bars descend from the horizontal feeder bars corresponding to that section. Thus we have at each feeder section two vertical dynamo bars and two vertical feeder bars, the lower ends all being at the same level. Mercury cups can be raised to connect each dynamo bar with a feeder bar, or lowered to disconnect them. The chief object of the switch-board is to enable us to switch any number of dynamos in or out of connection with any feeders simultaneously and instantaneously. I will now describe this action, referring to the diagram. In the normal condition the crank (A) and handle (B) would be as shown in the side elevation, one crank to each mercury cup (C). If it be required to put on to the dynamo the feeder No. 6, and take off No. 1 from the dynamo represented by this part of the switch-board, the handles (B), with cranks (A), corresponding to these feeders are pushed bodily to the left; the web of each crank is thus made to fit in a vertical slot in the disc (E), which disc is connected to the bevel wheel (G) by a hollow spindle to which (G) is keyed, this bevel wheel gearing with the bevel wheel (H) keyed on to the shaft (I), running the whole length of the switch-board. The crank of No. 6 would be diametrically opposite that at No. 1, since one was disconnected and the other connected to a circuit. The lever (L) is then thrown over against the other stop, causing the crank No. 6 to be raised from its lowest to its highest position, and that of No. 1 to be correspondingly lowered.

The voltmeters in this station by which the pressure at the lamps is kept constant are regulated by means of two circuits, one being a shunt on the dynamo terminals, and the other carrying the main current. These are passed through converters, and the secondary currents actuate a solenoid in opposite directions. This compound-wound voltmeter indicates the pressure at the lamps, and is maintained constant. Professor Mengarini has devised an ingenious recording voltmeter for marking in a daily curve the pressure at the dynamo terminals at each minute of time. These show variations of 12 volts at different hours. Reproductions of the registered curves are now exhibited (Figs. 7 and 8).

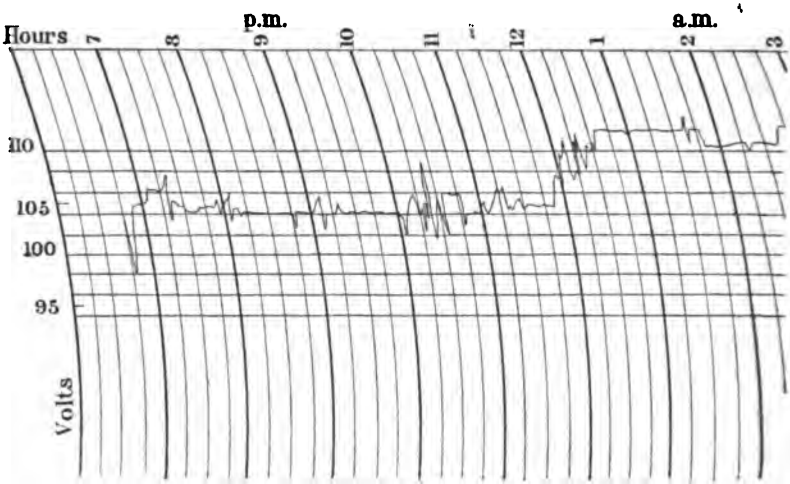


**Fig. 7.—Curves taken on Professor Mengarini's Automatic Recording Voltmeter.**

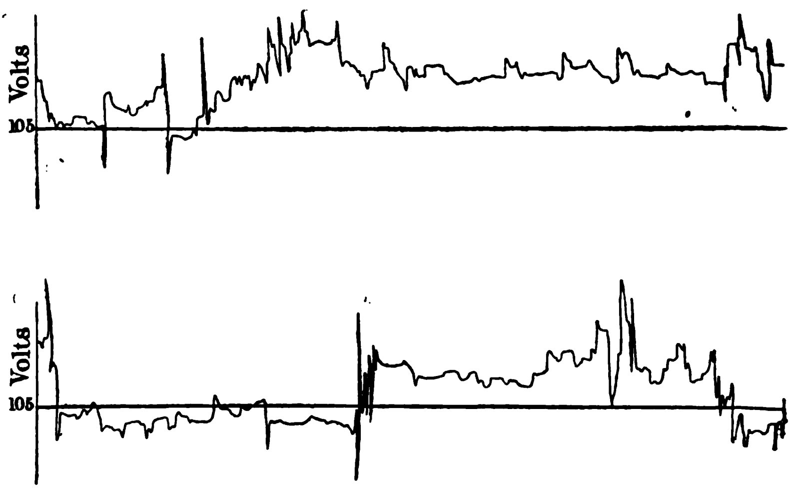


Fig. 8.

There are fifty converters now at work, each of 10 horse-power. The efficiency of these converters is 95 per cent. at full load.

That of Messrs. Ganz's 5-horse-power converters is 92 per cent., and the 2½-horse-power is 88 per cent. This is far higher than with many of those now made in England. The House of Parliament is lighted by four banks of five converters each.

## *Part II.*

## LESSONS TO BE LEARNED FROM FOREIGN CENTRAL STATION LIGHTING.

I propose now to deal with some of the problems of central station lighting which we in London have now to face.

I judge by the practices I see common in this country that I shall, as to my opinions on many points, be at variance with some of my brother engineers, because in the work which has hitherto been done I see that the experience of foreign countries has very often been neglected; and the rule seems to be to neglect the opportunity of utilising the experience gained at great expense of time and money in foreign countries, while our progress was being retarded by the Electric Lighting Act. But my conclusions have been arrived at after a most careful consideration of the successful work which has been done in Europe and America, and I am ready to condemn foreign practice when necessary. All I say is, that we ought to benefit by the experience abroad, and I know that those who have the best interests of electric lighting in England at heart agree with me.

This neglect of the principles laid down in other countries I find to exist whether the central station be used for continuous or alternating currents. I foresee great danger in store for us if we do not discuss the difficulties fully at the outset, and I trust that the discussion that this part of my paper will give rise to will assist to make the progress now being made in England a thorough success.

### THREE-WIRE SYSTEM.

My first remarks will be about the direct system of supply at low pressure—a system which may be much more frequently em-

ployed with advantage than many people seem to imagine. It is not suited for a large district, but it has some advantages. *First*—That the steadiness of the light can be increased, the fear of accidents diminished, and the amount of machinery reduced, by the employment of storage batteries. *Second*—If batteries are not used, the loss of energy in transformation is avoided.

Let us deal first with the case where all the dynamos are working in parallel at the same pressure. This is the Edison plan, and is very generally adopted. I have shown in my Cantor Lectures* that when we extend beyond a certain distance from the central station on the system at present used, the question of economy, as established by Sir William Thomson's law for the size of conductors, must be partially abandoned, and a different size used, in order to prevent the variation of electrical pressure over the area being too considerable under the varying loads. With the "two-wire" system the law of economy holds only up to a very short distance; with the "three-wire" the distance is doubled. This is due to the fact that the pressure is doubled, and this is the principal advantage possessed by the three-wire system. In Berlin and Milan the two-wire system has hitherto been adopted, and the enormous amount of capital expended in copper is a proof of the costliness of the system. I hold that in no case of central station work at low pressure ought the two-wire system to be adopted. In this I am supported by the universal practice in America, where the two-wire system for low-pressure distribution is now totally abandoned. I am also supported in this by the practice in Berlin, where all future work is to be done on the three-wire system. In spite of these considerations, I am not aware of a single case where it is in actual use in England since the experiments at Leamington have been abandoned. The two-wire system is used at a good many places in this country. The result of experience probably is that the three-wire system can be profitably worked to a maximum limit of half a mile.

* Cantor Lectures, "On the Distribution of Electricity," *Society of Arts' Journal*, 1885.

## Feeders.

I now come to another principle of distribution which is strangely, and I believe very generally, neglected in this country. I refer to the use of "feeders." Let me distinguish between the use of feeders and mains. Mains are conductors from all parts of which branches may be led into the houses. Feeders are conductors leading from the central station to various points in the system of mains, and which are not tapped on the road. In Berlin and Milan the practice, first started in America, is followed of laying down a network of mains and supplying current at numerous points in them by means of feeders. This serves to equalise the pressure over the entire districts under varying loads. In England this practice is not generally adopted. The result is that if you wander over a district supplied by electric light in England, you find that when the supply of current is small the lamps are equally bright over the whole town, but when there is a large demand for light the glow lamps near the station are far too bright and those at a distance are far too dull. In Berlin, where half the capital spent in conductors has been used for feeders, the lamps are, all over the district, equally bright at all times. Not the slightest difference can be perceived at different hours of the day or night. I hold the opinion that a station with low-tension current without feeders is badly engineered.

## Using Dynamos at Different Pressures.

It is the universal practice abroad to make the size of the feeders such that the loss of pressure in these feeders is the same in all. This involves the use of very large feeders when their length is great, and great waste of energy when the length of feeder is small, but it enables all the feeders to be supplied by dynamos at the same pressure, which may therefore work in parallel. This arrangement is simple to work, but it is wasteful. I find that we can save enormously in copper at a slight extra trouble in working the central station. I would prefer to see the feeders divided into two or three groups, feeders in each group having the same length, and having the same loss of pressure.

All these feeders would be made of the economical section, according to Sir William Thomson's law. When the station is at full load the dynamos working long feeders have a higher pressure than those working short feeders. When the load is light all the dynamos would work at the same pressure, because the loss in the feeders is insignificant. At such times all the dynamos might be joined in parallel, or the feeders might all be upon one dynamo.

## Position of Pilot Wires.

In nearly all cases of low-pressure distribution the pressure is regulated by a "pilot wire" coming from the point where the feeder meets the main. These pilot wires indicate the pressure at those points upon a voltmeter at the station, which is kept at constant reading by an attendant. Sometimes all the different pilot wires are grouped together at the station and indicate the average pressure on the voltmeter. This is done at Berlin. At Milan, however, the pressure is varied according to a tabular rule when the current varies. The use of pilot wires is, however, very desirable, but I hold that hitherto the pilot wires have generally been placed in the wrong position. Suppose that a 4 per cent. variation exists over the district of mains supplied by any one feeder, and that the point where the feeder strikes the mains is kept at constant pressure, then the most distant lamp supplied by that feeder has a variation of 4 per cent. If, however, the pilot wire came from a point half-way between the most distant lamp and the feeder, and this point were kept at constant pressure, the maximum variation of pressure would be reduced to one-half, and would be only 2 per cent. It appears, then, that by properly placing the pilot wires the variations in pressure can be reduced to one-half; or, keeping the variations of pressure the same, the quantity of copper in the mains may be reduced to one-half.

In these suggestions as to dynamos at different pressures and as to the position of pilot wires I am supporting a course which has not been generally adopted anywhere, but the improvement ought only to be mentioned to receive general recognition.

### LAMPS OF DIFFERENT VOLTAGE.

It has often been proposed to supply different parts of a district with lamps of different voltage. In the ordinary plan of using feeders and pilot wires the maximum pressure at the most distant lamp supplied by any feeder is the same as the pressure in the mains at the feeder. The minimum pressure may be, let us say, 4 volts less. Suppose 100-volt lamps are used, those near the feeder will always have 100 volts, but the most distant ones will have pressures varying from 96 to 100 volts. 96 volts would make the lamps very dull; and if at the distant points lamps of 98 volts were used the pressure would never vary more than 2 per cent. from the normal, and a better light would be obtained. This would be a very good arrangement if we could lay out a district from the beginning with all the positions of the lamps established, but it is found that as the station progresses a house which would have required 100 volt lamps yesterday may require a limit of only 99 volt lamps to-day, owing to the introduction of additional lamps between it and the feeder. Experiments which have been made in America on this plan have led to disastrous results and hopeless confusion in completing the scheme. This plan has been completely abandoned, and in the present state of the industry I think that this abandonment has been wise.

I do not think a greater variation of pressure than 2 per cent. up and down should ever be allowed if you wish to supply a satisfactory light. In Berlin it is only ¾ of a volt.

I consider that success or failure of many electric light companies now doing central station work is very much dependent on their following or neglecting the proper rules for distribution, and I will illustrate the importance of these rules by a few examples.

1. A plain network by the two-wire system, fed at the central station with the economical size of copper conductors according to Sir William Thomson's law, say of 1,000 amperes per square inch, and going to a distance of 960* yards, would have a difference of pressure to which the distant lamps would be subjected of 48* volts.

* Since this paper was read the figures marked by an asterisk have been changed, to remove an error pointed out by Mr. Lant Carpenter. It may also be noticed that the foreign practice is to make the neutral wire equal in section to the positive and negative wires.

2. On the three-wire system this would be 12 volts with one-fourth extra copper.

3. With the same amount of copper as in the last, properly arranged with feeders, the feeding points being 320* yards apart, the maximum variation of pressure in any lamp over the district is 2 volts, if the pilot wires come from the feeding centres.

4. Everything remaining the same as this last, except that the pilot wires are placed in the proper position instead of at the feeding points, the maximum variation is only 1 volt.

Putting it in other words, the quantity of copper required to limit the variation of pressure over the district to a certain figure is in the last case one-twelfth part of what it is in the second case, and one-thirty-eighth* part of what it is in the first case.

### High-Pressure Distribution.

Before I leave the question of distribution, let me now apply the same rules to the "Gaulard & Gibbs" system of using alternating currents with high pressure in the mains and low pressure in the houses, the reduction in pressure being effected by means of an induction apparatus called a "converter." At the present moment these converters are usually put in parallel upon the mains. There is a general impression in this country that it is not necessary to have feeders supplying the mains, even though the distance extends to several miles. Here again the experience of other countries has been set at defiance, although in a large town like London the importance of this rule is greater than in most American cities, or in Rome. If such views continue to be adopted, the result will be as disastrous in the future as it has been in the past. In the most extensive system in this country, when there is a large demand for current, the distant lamps look like red-hot hair-pins, even though the near lamps be at full brightness. This is due to defective distribution.

As already stated, there is a general belief in this country that to whatever extent the Gaulard & Gibbs system is used no trouble comes from variations of electric pressure over the district,

* See note on preceding page.

even when the converters are placed in parallel. Let us look at this question a little closer. Returning to the two-wire system for low pressure, if we use 1,000 amperes to the square inch as the economical section for our conductors, the fall in pressure is 5 volts for 100 yards; the loss of pressure due to the house wiring amounts very generally to 2 volts: hence the total variation between the periods of maximum and minimum output is 7 volts per 100 yards, or about double what is permissible (4 volts). Let us see how this distance is extended by using a pressure of 2,000 volts in the mains and 100 volts in the secondary. With the same density of current the loss of pressure in the house will be now 5 volts for 2,000 yards, to which must be added 2 volts for the house wiring and about 1½ volts for loss in the converter, making 8½ volts loss of pressure in 2,000 yards.

If we allow 4 volts variation in the lamps, which is quite a maximum, this allows only ½ per cent. variation in the mains, and then the greatest distance to which conductors can extend is 200 yards. The only means of extending the district is by the use of feeders. In America the practice prevails of allowing only ½ per cent. loss of pressure in the mains. I have taken a concrete case of 1,000 amperes per square inch, which may not always apply to actual conditions, but it shows the necessity of adopting the universal foreign practice of employing feeders even with high-pressure mains.

### Pressure Indicators on High-Tension Circuits.

It would be highly inconvenient to use pilot wires with the Gaulard & Gibbs system, such as are used on low-tension circuits; for this reason compound-wound voltmeters are used, which indicate the pressure at the far end of the feeder. I have described those in use at Rome and Milan; I have also described elsewhere those used by Westinghouse in America. The principle of these indicators is to have one winding connected like an ordinary voltmeter with the terminals of the dynamo, and another winding in reverse direction to carry the main current. The diminution in the indication owing to this second winding is always equal to the fall of pressure in the feeders. Different

inventors have applied different classes of apparatus for accomplishing this result, but it is needless to say that the working out of such instruments takes a long time, more especially in such an instrument as that employed by the Westinghouse Company, which is made interchangeable for all circuits by the insertion of six different plugs, representing six different losses in the feeders, converters, and house wiring, varying from 3 per cent. to 10 per cent., besides four plugs for the different maximum currents in use with any feeder. I draw attention to this point because I consider that the successful working of such a central station depends largely upon the use of such an indicator of pressure; and I find manufacturers in this country who have had little or no experience in central station work prepared to equip central stations with all the necessary measuring instruments, and I wish to feel confidence that these manufacturers have realised the importance of such instruments; and I wish earnestly to impress upon them, solely in their own interests, the necessity of working out such instruments if they wish to redeem electric lighting in the metropolis from the condition into which some of the so-called pioneers of this industry have reduced it.

### Insulation.

We have now to consider a very important, and perhaps the most difficult, part of the problem of central station lighting—How are we to preserve our insulation? It must be noticed that we do not require a high insulation for the sake of getting a small loss of current, but only because with many insulators the high insulation is more permanent, and permanency is the quality of insulation which is most to be desired. I am not going to discuss at present the whole question of insulation, but rather to draw conclusions from the experience of the past. At the present moment vulcanised india-rubber and okonite are the substances which have the highest reputation for durability and high insulation under all conditions. These, like other cables, are sometimes covered with lead, but in their case it is only as a mechanical protection, especially during the process of laying, because the destruction of the lead would not destroy the insulation. Of late

years a class of cables has been much praised, about which I wish to speak. The copper conductor, stranded or otherwise, is covered with any fibrous material, preferably jute, impregnated with bituminous oils. This covered core is enclosed in the lead pipe. In this class of cable the insulation depends upon the lead being water-tight. We all know of many places where this type of cable has done good work for a limited time, and many of us were inclined to believe that it was quite satisfactory; but I think I am right in saying that none of us know of its having worked successfully in any place for three or four years. I have said that a cable of this kind has hitherto been exclusively used in Berlin, the lead being covered with layers of tape or braid soaked in a preservative compound, and the whole armour-plated with two closed spirals of iron ribbon. I regret to have to tell you that this insulation has been a failure. It goes on well enough for about three years, then the lead gives way, the covering on the copper is permeated, and chemical or voltaic action sets up in the copper, which itself becomes disintegrated. So far as experience goes, this type of cable seems to me to be unsuitable for permanent work with the electric light. I trust that in the discussion we shall have some facts brought out. I had hoped, from what Berthoud & Borel, and also Waring, had done, that this would not be so, but the Berlin experience leads me to think otherwise; and on looking through the testimonials of makers I do not find that these cables, when placed underground, have ever worked electric light circuits satisfactorily beyond the three years fixed by the Berlin people as being destructive. I have been told, but I cannot believe it, that there is one company in London actually proposing to put such lead-covered cables without covering into iron pipes. Of course everyone knows they would soon be eaten into by galvanic action.

At Berlin and elsewhere I believe there is the intention of trying some modification of the system of using bare conductors, first practically used by Mr. Crompton at South Kensington. Taking economy into account, it is probable that for low-tension circuits this is an admirable plan. It is probable that it may also be adapted to high-tension circuits by using oil insulators to

support the bare conductors. When copper strip is used it is necessary to raise the cover of the trough in order to increase the number of conductors: this is very expensive. For my own part, I would prefer a drawing-in system, if it were otherwise equally good.

No information has ever been given as to (1) the maximum or (2) the minimum insulation resistance of the conductors at Kensington Court, but perhaps this information will be given during the discussion.

At the present moment it seems to me that the only types of underground cable proved suitable for permanent work are either bare copper supported on insulators, or else vulcanised india-rubber, or perhaps okonite. Especial care must be taken to avoid an insulator which is injured by the gases which permeate the soil of a town, or which has the property, like pitch, of becoming viscous, and so letting the copper become decentralised.

## Meters.

The next lesson which the experience of foreign stations in Europe and America has taught us is the importance of charging the consumers of electricity by meter, like gas. I find that at present most companies in this country are charging the larger proportion of their consumers by contract, making an annual charge for each lamp. This appears, from evidence collected everywhere, to be a fatal mistake. It is the universal experience that those stations pay best where meters are used. In many stations it is found to be the best plan to make a fixed annual charge, which may be put down as the rent of meter, and then an additional charge proportionate to the quantity of electric current supplied. This has the advantage of preventing customers putting in place a large number of lamps which during the greater portion of the year are idle, but which on festival occasions may all require current. Such idle lamps are very expensive to the suppliers, because a large reserve of machinery must be lying idle during the whole year to be ready for such occasions.

Within the last month I have described before the Society of

Arts* the different meters which are valuable, without pretending to estimate their relative value. For continuous currents we have the Edison and Aron meters, for alternating currents we have the Schallenberger meter, and for both classes my own "Windmill" meter.

### CONVERTERS.

The induction apparatus used with alternating currents is called a "converter," "secondary generator," or "transformer." The reduction of pressure from the primary to the secondary circuit is generally 20 to 1, but sometimes more. A large number of types have been invented. I have elsewhere described the Westinghouse converter, which I have tested and found to be very efficient, even though the largest one he is in the habit of supplying is only for forty lights.

Only the larger size of Zipernowski converter is so efficient. Three sizes are made, varying in efficiency from 95 down to 88 per cent., though the latter is a converter suitable for twenty-five lamps. Many of the types which are made in England I feel sure are far from efficient. There is a general opinion that the loss in the converter is necessarily small. This is by no means the case. A great deal of care must be taken in the design to get a satisfactory result. I think there is a general idea that any of the converters now on the market in England will give an efficiency for the average number of lamps in use of about 90 to 95 per cent. I should be much surprised if there are more than two types in use in this country which have an efficiency for the average number of lamps over 70 per cent. Yet, so far as I am aware, no maker in this country ever tests the efficiency of the converters which he supplies. The efficiency of a converter falls when it is under-loaded. If the converter in a house is adapted for 100 lamps, and only three or four lamps are in use, then, with many converters, the current which is being used is double or treble what is required for the lamps. This defect in some converters is clearly shown in the load diagram (Fig. 9) of the Grosvenor Gallery installation, where during the hours of

* *Society of Arts Journal*, January 25, 1889.

minimum supply the current sent out from the station is seen to be abnormally high—far above that of the load diagram of other central stations—and at the hours from 4 to 6 a.m. in October current sufficient for 4,000 lamps is indicated on the station meter. This curve shows a loss of 20 per cent. The type of converter which Westinghouse has perfected is one of low magnetic resistance. The worst ones now being made in England are of

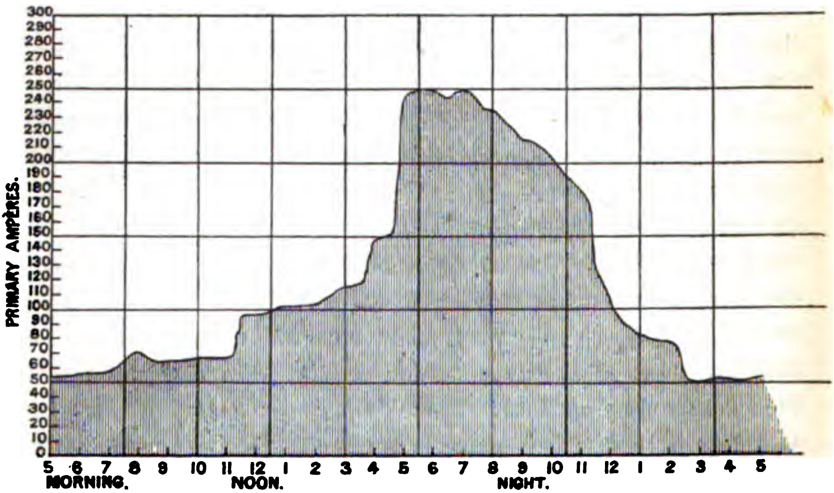


FIG. 9.

high magnetic resistance. The waste in a converter, independent of magnetic friction, varies as the square of the magnetic resistance.* The waste due to magnetic friction varies as the length of the magnetic circuit, if the induction in the iron is the same. These high magnetic resistance converters require a great deal of copper. The magnetic friction is so great that they would probably work better with an open magnetic circuit, as shown by Lord Rayleigh (*Phil. Mag.*, 1886).

## SLOW- AND HIGH-SPEED ENGINES.

I need hardly tell you that the most startling difference I found between the best European and American practice is the

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* See my formulæ for converters, *Journal of the Society of Telegraph-Engineers and Electricians*, vol. xvii., part 71, p. 153.

question of speed of revolution of dynamos. In America 1,000 revolutions a minute is the normal speed for an alternator of 250 horse-power; in Rome the 600-horse-power dynamos revolve at 125 revolutions per minute; at Berlin the 400-horse-power dynamos at 80 revolutions per minute. In Europe there is a tendency to condemn high speed as such, whereas high speed is more objectionable only in the type of machine where the revolving part is more massive, less perfectly balanced, less capable of standing centrifugal strains, and has a longer distance between the bearings. In every other point high speed is an advantage, especially in economy of plant. Now compare the Zipernowski and the Westinghouse dynamos, choosing of the former the one without Pacinotti teeth on the armature, as made by Elwell-Parker. These two machines are electrically identical, but in one the armature is fixed, in the other the field magnets. The armature can, and the field magnets cannot, be perfectly balanced. The weights are not very different. The armature is more capable of standing centrifugal strains than the coils of the field magnets. For all these reasons the Westinghouse may be run at high speed and gain all the advantages of economy. Of course the necessity of introducing belting has to be considered in this case. But then turn to the Parsons alternator, which drives direct, and being the lightest, best-balanced machine, with great capacity of resisting centrifugal strains, and with the only scientific bearings in the market. Here we have a machine where all the advantages of high speed may be safely acquired and great economy introduced. People are apt to forget that slow speed means large machines with no increase of output.

### Speed of Alternations.

Now I come to a very important point. What speed of alternations shall we use? In Rome they have 83 alternations, or 41 complete periods, per second. In America the Westinghouse Company have 267 alternations, or 133 periods, per second. What are the advantages and disadvantages, and why have these makers adopted different plans? I will answer this in detail to the best of my ability. *First*, Westinghouse went to rapid

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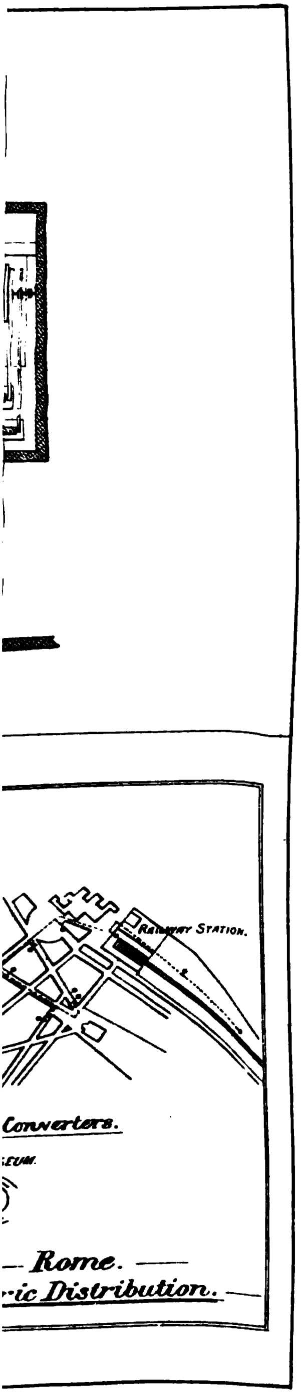


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ly to cut out special districts if a fire or other trouble occurs. lso gets over the trouble of increased resistance in large uctors with alternating current, dwelt on by our President s opening Address. If I have more than one station in a and I want one to assist the other, I would do so by trunk between the stations, to which a dynamo can be attached at nd and feeders at the other. Of course both stations must y the same type of converters; hence in such a system it not do to use dynamos of various speeds of alternations at fferent stations. I would certainly prefer parallel working ng simpler for the men at the central station, if no comons are introduced to make the machines work in parallel, the system were equally economical, but I do not consider ll essential. I venture to think, however, that the plan niversally proposed for making machines work in parallel long be tolerated. This is to introduce into the machine amount of injurious self-induction, thus diminishing the fficiency and rendering the equalising of pressure with loads very difficult. I have confidence in the ingenuity nventors, who will soon devise a less objectionable plan.

### AUTOMATIC OR HAND REGULATION.

question now arises whether the regulation of potential be accomplished by hand or automatically. In all low- systems, such as Berlin, it is done by hand. A workman s to know at what hours special attention must be paid to l on the low-tension system with shunt-wound or comound machines the regulation required is slight. Where g in personal attention is not very great it is always un- to trust to automatic dodges. The case of alternating- ynamos with high self-induction is very different, for the ressure then varies enormously with the current. In this n such a machine resembles a series-wound direct-current with high resistance. The changes are so frequent that omatic adjustment is necessary. Ferranti, Lowrie, and ve used the sagging of a wire to effect this, and they

regulate the pressure at the dynamo, not at the centres of distribution, which is wrong. The apparatus looks too delicate for an engine-room. But Ganz & Co. use a very practical type. They also regulate for the centres of distribution, which is right. I also prefer the method used by Ganz & Co. of regulating the large changes by hand, and only the small changes automatically. This is necessary, owing to the very large adjustments necessary with alternators of high self-induction.

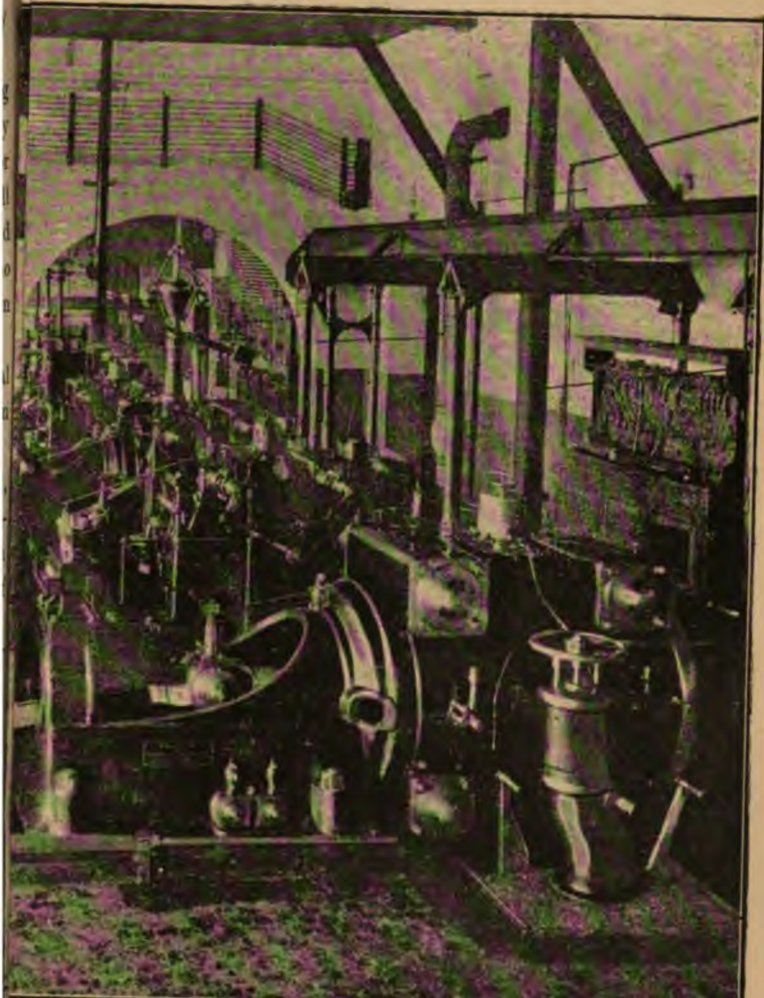
In conclusion, I feel confident that all those who are working to maintain England's supremacy in this department of industry will agree with me that the practical details necessary for successful central station working must be thoroughly well thought out beforehand, and that much saving of time and money can be effected by gathering hints from those who have spent years of time and tens of thousands of pounds on these details.

I frankly admit that I have learnt more that is of practical value in this line from the inspection of actual work done than by reading books.

But we must remember that the low price of gas in England, the cheapness of coal, and the relative cost of labour, all introduce special factors which will partially differentiate the English practice either from that of the continent of Europe or of America.

I wish now that the last words of this paper may be words of thanks to the gentlemen in other countries—whether in Europe or in America—who have the control of central stations, for the courtesy which they have extended to me in my endeavours to assist, to the feeble extent possible to me, in maintaining the position of our own country in the progress of this new and important industry.

The PRESIDENT: At this late hour of the evening I am afraid we cannot proceed to discuss the paper which we have just heard, and must defer it for the next meeting, which will be held on March 14th next.



5.

TRAL STATION ROME.

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# AGRAM


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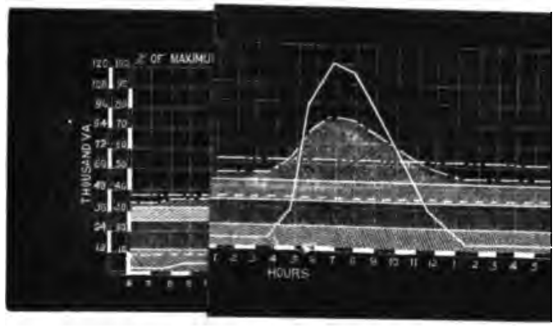
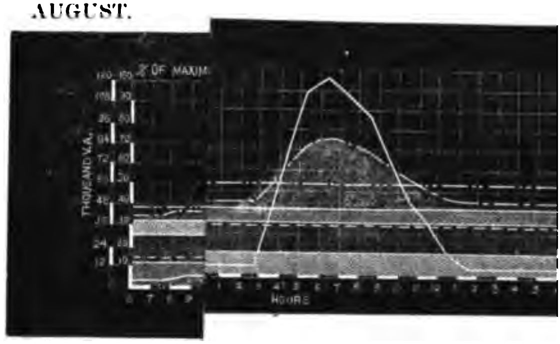
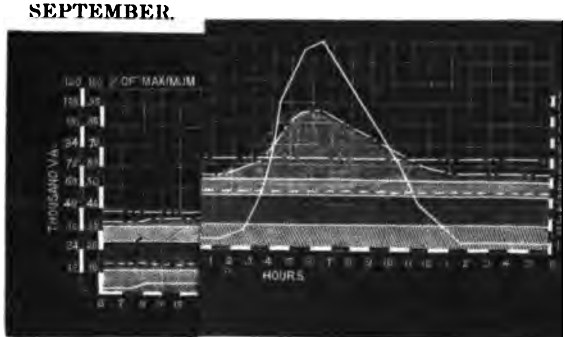
**JULY.**




**AUGUST.**




**SEPTEMBER.**




## RENCE.

**Top** Hatching = Cost of

**2nd** ,, = Cost of

**3rd** ,, = Depreci

**4th** ,, = Interest

PLATE 4.


*Milan*
*House Lighting.*

*Fig 2.*

*Milan C*

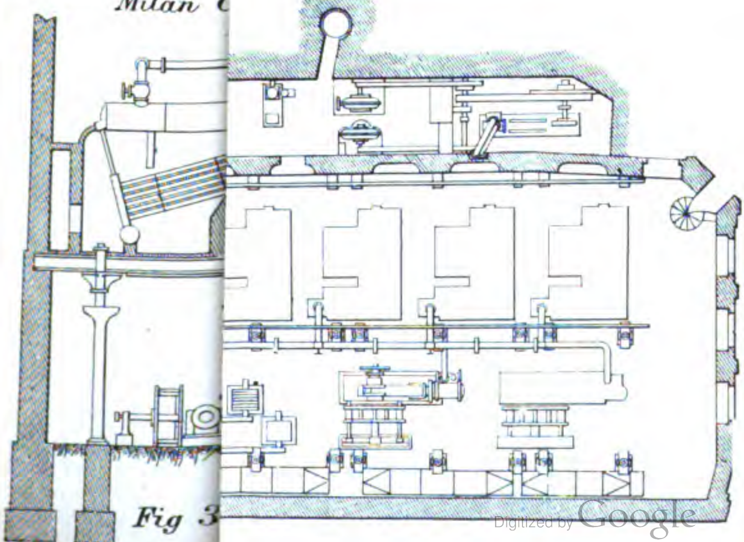


*Fig 3*

Mr. W. H. Preece: May I ask if in the meantime the paper will be printed and circulated?

The Secretary: Yes.

A ballot for new members took place, at which the following were elected:—

*Members:*

| | |
|---|---|
| Henry Graham Harris. | H. Whitby Smith. |

*Associates:*

| | |
|---|---|
| William Arthur Britten. | Fiennes Barrett Lennard, M.A. |

The meeting then adjourned.

# THE LIBRARY.

ACCESSIONS TO THE LIBRARY FROM JANUARY 1 TO MARCH 20, 1889.

(*Works marked thus* (*) *have been purchased. Of those not purchased or received in exchange, where the donors' names are not given, the works have been presented by the authors.*)

IT IS PARTICULARLY DESIRABLE THAT MEMBERS SHOULD PRESENT COPIES OF THEIR WORKS TO THE LIBRARY AS SOON AS POSSIBLE AFTER PUBLICATION.

**Bamber** [E. F.], Editor. The Scientific Works of C. W. Siemens, Kt., F.R.S., D.C.L., LL.D. 3 vols. 8vo.
Vol. I. Heat and Metallurgy. 496 pp. 47 plates.
„ II. Electricity and Miscellaneous. 493 pp. 37 plates.
„ III. Addresses, Lectures, &c. 458 pp. 11 plates.
*London*, 1889
[Presented by the Executors of the late Sir William Siemens.]

**Berly** [J. A.] [*Vide* Reynier.]

**Board of Electrical Control for the City of New York.** First and Second Reports. 8vo. 40 pp. and 78 pp. *New York*, 1888
[Presented by S. S. Wheeler.]

* **British Association.** Reports. 1831-1887. (1839 missing.) 55 vols. *London*, 1832-1888

* **Clausius** [R.] The Mechanical Theory of Heat, with its Applications to the Steam Engine, and to the Physical Properties of Bodies. Edited by T. Archer Hirst; with an Introduction by Professor Tyndall. 8vo. 376 pp. *London*, 1867

**Electrical Society of Japan.** Journal of. (Printed in Japanese.) July 25, 1888. 8vo. *Tokio*, 1888
[Presented by the Society.]

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**Harris** [Charles Hope]. [*Vide* South Australian Institute of Surveyors.]

**Hirst** [T. Archer.] [*Vide* Clausius, R.]

**Hospitalier** [E.] Les Compteurs d'Énergie Électrique. 8vo. 57 pp. *Paris*, 1889

**Indian Telegraphs.** Administration Report of the Indian Telegraph Department for 1887-88. Fo. 55 pp. *Simla*, 1888
[Presented by the Director-General of Telegraphs in India.]

**Iron and Steel Institute.** Journal of. No. 2, 1888. 8vo. 405 + lxviii. pp. *London*, 1888
[Exchange.]

**Kelland** [Rev. P.] [*Vide* Young, Thomas.]

**Kew Observatory.** Report of the Kew Committee for the Year ending Oct. 31, 1888; with Appendices containing Results of Magnetical, Meteorological, and Solar Observations made at the Observatory. 8vo. 28 pp. [*Proc. Roy. Soc.*, 1888.] *London*, 1888

**Kirchoff.** [*Vide* Rimington.]

**Maier.** [*Vide* Preece and Maier.]

**New York.** [*Vide* Board of Electrical Control for the City of New York.]

**Preece** [William Henry] **and Maier** [Julius]. The Telephone. 8vo. 498 pp. [Specialists' Series.] *London*, 1889
[Presented by Messrs. Whittaker & Co. (Publishers).]

• **Reynier** [Émile]. The Voltaic Accumulator: An Elementary Treatise. Translated from the French by J. A. Berly. 8vo. 202 pp. *London*, 1889

**Rimington** [E. C.] Kirchoff's Laws and their Application. [Reprinted from *The Electrical Review*.] 8vo. 59 pp. *London*, 1888
[Presented by Messrs. H. Alabaster, Gatehouse, & Co. (Publishers).]

**Siemens** [Sir C. W.], F.R.S., D.C.L., LL.D. The Scientific Works of. Edited by E. F. Bamber. 3 vols. 8vo.
Vol. I. Heat and Metallurgy. 496 pp. 47 plates.
,, II. Electricity and Miscellaneous. 493 pp. 37 plates.
,, III. Addresses, Lectures, &c. 458 pp. 11 plates.
*London*, 1889
[Presented by the Executors of the late Sir William Siemens.]

**South Australian Institute of Surveyors.** Report of Annual Meeting held Nov. 2, 1888. 8vo. 22 pp. [Contains Address by Chas. Todd, and Paper entitled "Developments of Terrestrial Magnetism affecting "the Compass," by Charles Hope Harris.] *Adelaide*, 1888
[Presented by Charles Todd, Member.]

**Swinburne** [J.] Practical Electrical Measurement. 8vo. 155 pp. *London*, 1888
[Presented by Messrs. H. Alabaster, Gatehouse, & Co. (Publishers).]

**Swinton** [Alan A. Campbell]. The Elementary Principles of Electric Lighting. 2nd Edition. 12mo. 39 pp. *London*, 1889

* **Tait** [P. G.] Lectures on some Recent Advances in Physical Science. 2nd Edition. 8vo. 352 pp. *London*, 1876

**Thomson** [Sir William], LL.D., F.R.S. Popular Lectures and Addresses. Vol. I. Continuation of Matter. *Nature* Series. Cr. 8vo. 460 pp. *London*, 1889

**Todd** [Charles]. [*Vide* South Australian Institute of Surveyors.]

**Treuenfeld** [R. von Fischer]. Index of Publications on Methods of Communication in the Field, and on Torpedo Warfare. 8vo. 71 pp. *London*, 1889

[Presented by Messrs. H. Alabaster, Gatehouse, & Co. (Publishers).]

**Tyndall** [Prof.] [*Vide* Clausius, R.]

* **Young** [Thomas]. A Course of Lectures on Natural Philosophy and the Mechanical Arts. A New Edition, with References and Notes by the Rev. P. Kelland. 2 vols. [Vol. I., Text; Vol. II., Plates.] 8vo. *London*, 1807

# ABSTRACTS.

## Dr. J. A. FLEMING—A DESIGN FOR A STANDARD OF ELECTRICAL RESISTANCE.

(*Philosophical Magazine, Vol.* 27, *p.* 24, 1889.)

The two chief points to be borne in mind in designing a standard of resistance are the material and the form of the instrument. Mercury, though it can be obtained and kept in a state of purity, is yet, for many obvious reasons, rather an inconvenient substance for a standard of resistance; it is better to use a wire of platinum-silver alloy, the permanence of which may be fairly anticipated from previous experience.

The ordinary B.A. form of resistance is too well known to need description; but some of its disadvantages may be alluded to. Owing to its size and shape, it requires some time for the wire to take up the temperature of the liquid in which it is immersed; and the heat produced by the passage of the current will not be easily got rid of. The old form also cannot be entirely immersed, as by so doing the water will short-circuit the two stout rods which form the terminal connections; hence the bottom of the coil may be at one temperature while the top is at a different one. If the resistances are used at the temperature of melting ice, dew will be deposited on the surface of the paraffin wax, and will thus tend to short-circuit the resistance.

The best form of coil will therefore be one which permits of rapid dissipation of heat, and of being entirely immersed without risk of short-circuiting. The improved form designed by the author consists of a flat ring, with an interior rectangular channel in which the silk-covered wire is wound double. The connections are by means of two stout copper rods soldered to the two ends of the coil. These rods rise up inside two brass tubes, from which they are insulated by ebonite rings. The bottom rings are plain washers; the two top rings are hollowed out funnel shape: into these two funnels paraffin oil can be poured, thus effectually preventing any creeping of moisture. In constructing such a coil, the wire is at first cut off a little too long; after the copper connecting rods have been soldered on, the resistance is measured; to reduce it to the standard, the silk covering is removed carefully at the bend where the wire is doubled on itself; the bared loop is then twisted together until exact adjustment is almost obtained; the twisted coil is then touched with solder, and the bared portion is re-insulated with silk.

## J. PARKER—THERMO-ELECTRIC PHENOMENA.

(*Philosophical Magazine, Vol.* 27, *p.* 72, 1889.)

The author imagines an arrangement of condensers and wires of various metals, which permits of a complete reversible cycle of operations being performed, as in Carnot's heat-engine, including the passage of electricity across

the junctions of the different metals without producing any thermal effect. From the consideration of Carnot's principle, and of equations established in a previous paper, it follows that, if two portions of the same metal, at different absolute temperatures $t$, $t_0$ respectively, be in contact, and if V, $V_0$ be the potentials which they assume, then

$$V - V_0 = \frac{1}{2} K (t^2 - t_0^2),$$

where K is a constant which has the same value for all metals. In other words, the difference of potential depends only on the temperatures, and is the same for all metals. If, however, unit charge be made to cross the junction, the heat that must be imparted to the junction to keep its temperature constant will be different for different metals. In general, if a thermo-electric circuit be formed of any number of different metals, and if $\delta$ be the abrupt rise of potential at any junction as we travel in the direction of the current, then evidently $E = \Sigma \delta$. If the junctions be all at the same temperature, we have $\Sigma \delta = 0$, and therefore $E = 0$.

---

## F. QUINCKE—ELECTROLYSIS OF CUPROUS CHLORIDE.

(*Annalen*, *Vol.* 36, *p.* 270, 1889.)

The author has repeated a previous experiment of Buff's on the electrolysis of cuprous chloride. The salt was prepared pure by boiling cupric chloride with copper filings in concentrated hydro-chloric acid; and was then, avoiding as far as possible all exposure to the air, melted into a porcelain crucible. This latter was provided with a cover of porcelain or mica, through which passed the two electrodes of copper wire made into spirals. The current from 20 bichromate cells was sent through a tangent galvanometer, a copper voltameter, and a silver voltameter, and could thus be accurately determined. It varied in different experiments from about 0·6 to 0·4 ampere. The amount of copper electrolysed was found to be from one and a half to two and a half times that calculated. The divergencies depend partly upon the action of the chloride itself on the copper electrodes, which produces a decomposition of the salt owing to a spontaneous electric current which varies very much both in value and direction, and is apparently caused by unequal heating of the salt.

The author concludes that cuprous chloride cannot be used for determining the electro-chemical equivalent of copper; and the question of a double equivalent for copper, accordingly as the electrolyte may be a cupric or a cuprous salt, cannot be settled by its electrolysis.

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## BELLATI and LUSSANA—PASSAGE OF ELECTRIC CURRENTS THROUGH BAD CONTACTS.

(*Beiblätter*, *Vol.* 13, *p.* 21, 1889.)

A small prism of iron pyrites was placed with its ends in two vessels o mercury, a current was passed through, and then on interruption of the current the pyrites was connected to a delicate galvanometer. The small

currents observed could not be taken as evidence of polarisation, but were due to thermo-electric causes. By means of a quadrant electrometer the fall of potential at two points on each side of one contact was determined, as well as at two points on the prism itself. The resistance of these latter varied between 737 and 740 Siemens units, and was constant; while the resistance at the contact increased, and was greater when the current passed from the pyrites to the mercury than when it passed in the opposite direction. If the current is kept on, the resistance gradually increases, while a reversal of the current is attended by a decrease. By exerting pressure on the mercury the resistance decreases, at first rapidy, and then more slowly. If a copper point is pressed against the pyrites by means of a weighted lever, the resistance will greatly diminish—*e.g.*, from 609·9 ohms with 25 grammes to 97·2 ohms with 525 grammes. Replacing the copper point by a sewing needle, the resistance was so great that it could not be measured. The direction of the current was always of importance; a rise of temperature also has the effect of diminishing the contact resistance.

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## E. DRECHSEL.—ELECTROLYSIS BY MEANS OF ALTERNATING CURRENTS.

(*Beiblätter, Vol.* 13, *p.* 28, 1889.)

The author claims priority for his publication of observations on this point over Messrs Maneuvrier and Chappuis (*vide* Journal, vol. xviii, pp. 79, 80). He remarks, besides, that the explosion of the mixed gases is not alone caused by the heating of the electrodes, as they assert, but also that it may be noticed when the electrodes consist of Wood's fusible metal, which subsequently showed no trace of fusion, and with platinum wire electrodes so arranged as to be capable of being gradually drawn out of the voltameter so as to present always the same surface to the gases. Even then, if the gas is given off violently, so that the bubbles of nascent gas form a layer between the electrode and the electrolyte, a spark will pass which determines the explosion. The same phenomenon may be noticed with electrodes entirely covered with the liquid, and when continuous currents are used.

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## D'ARSONVAL.—A UNIVERSAL DEAD-BEAT GALVANOMETER.

(*Bulletin de la Société Internationale des Electriciens, Vol.* 6, *p.* 6, 1889.)

The instrument, which was specially designed for physiological researches, is generally similar to Wiedemann's form of tangent galvanometer; by an easy alteration it can be made suitable for ballistic measurements.

On a base of wood is fixed a graduated bar, set on edge. The base rests on three feet, two of which are levelling screws, the line joining them being perpendicular to the bar, so that the galvanometer can be at once set up in position by placing these two screws in the line of the magnetic meridian. On the bar slide two slotted pieces, each of which carries a coil of wire; these

can be readily exchanged for others, according to the nature of the measurements to be made. The wooden cores on which the coils are wound are hollowed out on one side into semi-spherical cavities.

From the base rise two columns, right and left of the bar, carrying a cross-piece on which rests the closed cylindrical box containing the magnet, mirror, and damping arrangements; this case is capable of being rotated as a whole in the hole in the cross-piece, in which it rests. The rod carrying the mirror and magnet is suspended by a fibre passing through a tube fixed on the top of the box; the magnet is at the lower end of the rod, and is shaped like a horse-shoe. The rod passes down through a tube forming a prolongation of the box, and having a screw thread cut on its lower end, so that dampers of copper and brass, and of various forms, may be screwed on, enclosing the magnet. The suspension tube carries a directing magnet; if this be removed, the instrument becomes a simple tangent galvanometer. The galvanometer can be rendered astatic by placing a second horse-shoe magnet just under the mirror, on the rod carrying the latter and the lower magnet. Or the whole magnetic system may be replaced by an ordinary mirror with small magnets, as in Thomson's usual form. The readings may be taken either with a spot of light or with a telescope.

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## F. LARROQUE—PERMANENT CHANGES PRODUCED IN COPPER WIRES BY THE PASSAGE OF A CURRENT.

(*La Lumière Electrique*, Vol. 31, p. 161, 1889.)

It has been noticed since a long time that the continued passage of a current of electricity in a copper wire tends to render it brittle. Dufour noticed a decrease of more than 30 per cent. in the tensile strength of a copper wire which was traversed by a current insufficient to heat it sensibly. Wertheim thought that he had found that the decrease in the tensile strength was only temporary, and disappeared when the current was interrupted. Edlund and Streintz speak of a permanent elongation, but the experiments of Blondlot have demonstrated that this is an error.

So long as the small currents chiefly used in telegraphy were the only ones to be considered, their effect on their conductors was not of practical importance; but with the spread of the sphere of electricity for lighting and transmission of power, and the more general use of heavy currents for these purposes, the permanent effects produced by them in copper wires claim attention.

The breaking of an old electric light cable led the author to investigate the question. A piece of a wire used during twenty years to convey the current to a lighthouse could be easily broken up with a hammer; the broken surfaces showed clearly that the metal had a granular structure, such as is seen in copper deposited electrolytically, although when first put up the wire had been hard-drawn. Owing to the section of the conductor, it probably became somewhat heated by the passage of the normal current, though the rise of temperature could scarcely have exceeded 30° or 40°.

It seems that the long-continued passage of a current of electricity tends to bring about a molecular rearrangement, resulting in a granular structure. Moreover, as far as experience goes, it seems to be immaterial whether the current is direct or alternating, as the action of both is similar.

The secondary of an induction coil had become so brittle from long use that the wire could not be unwound, but broke repeatedly. The ratio of the resistance of the old wire to that of a new wire of similar length and diameter was found to be as 21 to 16.

From his own experiments, which were carried out with spirals and zigzags of copper strip, the author concludes—

(1.) That the elongations referred to are excessively small, and do not follow any definite law. The lengthenings and shortenings probably result from want of homogeneity, and are not permanent results produced by the passage of the current.

(2.) That the changes in the tensile strength depend on the strength of the current traversing the conductors, but the relation is not one of simple proportion; the passage of the current has more effect on hard-drawn copper than on annealed copper; alternating currents seem to act more quickly than direct currents.

(3.) A current of two amperes, especially when alternating, materially changed the elasticity of a copper wire three mm. in diameter, after a lapse of four years. It would certainly seem, therefore, that conductors carrying heavy currents such as are used in electric lighting are fated to undergo a kind of slow disintegration which will affect not only their tensile strength, but also their resistance.

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## Dr. A. v. WALTENHOFEN—EXPERIMENTS WITH THE ACCUMULATORS OF FARBAKY AND SCHENEK.

(*La Lumière Electrique, Vol.* 31, *p.* 142, 1889.)

The cells contained each seven positive plates and six negative. Each plate weighed 21·3 kilos., and had a surface of 3·77 square cm. The normal rate of discharge was 20 amperes, but for the purposes of these tests the cells were pushed far beyond this. In the first experiment the discharge was at the rate of 100 amperes, and lasted for 100 minutes. The total discharge was 165·87 ampere-hours, or 305·02 watt-hours. The potential difference fell from 1·87 to 1·78 volts per cell. The cells were then recharged with 100 ampere-hours, and again discharged with a mean current of 100 amperes for nearly an hour, or at the rate of 4·7 amperes per kilo. of plates. The efficiency in ampere-hours was 88·1 per cent., or in watt-hours 77·4 per cent. The decrease of potential difference was 5·22 per cent.; the decrease in current during the last ten minutes as compared with the first ten minutes was 2·32 per cent.

These satisfactory results induced the author to try the effect of a discharge at the rate of 200 amperes for 50 minutes, or 9·5 amperes per kilo. of plates. The decrease in potential difference was 26 per cent., the potential difference being at the beginning 2·26 volts per cell, and 1·98 volts at the end.

The total discharge was 166 ampere-hours, 131 of which were obtained before the potential difference fell 10 per cent.

A final experiment was made with a discharge rate of 270 amperes, or 12·83 amperes per kilo. of plates. This gave 120 ampere-hours, with a falling off of potential difference of 30·5 per cent.

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## C. HEIM—THE USE OF ACCUMULATORS IN TELEGRAPHY.

(*Elektrotechnische Zeitschrift, Vol.* 10, *p.* 41, 1889.)

If primary batteries are used, as is almost exclusively the case, and there are many circuits, their number becomes enormous, and the keeping them in order becomes very onerous. Dynamo machines have been tried—in some cases successfully, in others not—but there are several objections to their use; perhaps the chief are, that the engine and dynamo must be kept constantly running day and night, as current may be wanted at any moment, and that the plant must be duplicated so as to provide a reserve in case of accidents.

One of the chief causes of accumulators not being more used has been want of confidence in their durability; but this objection will fall to the ground as improvements in the manufacture lead to the turning out of more and more perfect cells, which can be counted upon for some length of time. The use of accumulators for telegraph work presents many advantages. Their E.M.F. of 2 volts is comparatively high, while they have a very small internal resistance; when at rest, no energy is spent; they can easily be kept in order, and require but little attention, and that at comparatively long intervals of time.

As a practical example, the case of an office with one hundred circuits may be taken. The maximum current on any one circuit will be at most 0·02 ampere, and the highest E.M.F. 200 volts. One hundred cells will therefore be wanted, of a capacity of 50 ampere-hours, and capable of standing a normal discharge at the rate of 6 amperes; the internal resistance of the 100 cells will be 1 ohm. If all the circuits are working simultaneously, the total current required will be about 2 amperes; and consequently the E.M.F. will drop about 2 volts.

In practice two such batteries would be provided, to be connected alternately to the circuits. From every fifth cell wires would be taken to the commutator board, thus giving a potential difference of 10 volts from terminal to terminal. The two batteries would be exchanged by means of a simple switch at a fixed hour daily, or every two days. The actual time during which the battery would be discharging current during a working day of twenty-four hours would scarcely exceed six hours, or the total discharge for that time would be 12 ampere-hours; and as the total capacity is 50 ampere-hours, the drop in E.M.F. would be very small, not exceeding 2 per cent. at most.

The charging can be effected in from four to six hours. All the cells will not have been equally discharged, owing to the different demands of the various

circuits; therefore the charging would have to be continued until all the cells gave off gas.

For working circuits with polarised instruments and such as are operated by alternating currents, a battery of 200 accumulator cells would be wanted; the connection between the 100th and 101st cell would be earthed, and the positive and negative currents sent from either end. Local circuits could also be supplied with current from the less used sets of cells, by which means the discharge would be more evenly distributed over the whole battery.

The author estimates the first cost of gas engine, dynamo, cells, &c., at £655; and the annual maintenance, allowing for interest and depreciation, at £200. A primary cell with an E.M.F. of 1 volt and an internal resistance of 5 ohms gives one-twentieth of a watt; an accumulator cell when discharged at the rate of 1 ampere gives 2 watts; hence 200 accumulator cells could replace $200 \times 2 \times 20 = 8,000$ primary cells, or allowing for the fact that the latter are not worked up to their full power all the time, some 5,000 to 6,000 primary cells

# LIST OF ARTICLES

RELATING TO

# ELECTRICITY AND MAGNETISM

Appearing in some of the principal Technical Journals during the month of FEBRUARY, 1889.

## I.—BATTERIES AND ACCUMULATORS.

E. LANDMANN—Bichromate Batteries without a Diaphragm.—*Beiblätter*, vol. 13, p. 94, 1889.

ANON.—Improvement in American Accumulators.—*Lum. El.*, vol. 31, p. 235, 1889.

C. HEIM—Effect of the Strength of Acid on the Capacity of Accumulators.—*El. Zeit.*, vol. 10, p. 88, 1889.

GRAWINKEL—Use of Accumulators in Telegraphy.—*El Zeit.*, vol. 10, p. 108, 1889.

## II.—DYNAMOS AND MOTORS.

E. GUINAND—Efficiency of Small Motors of the Zurich Co.—*Lum. El.*, vol. 31, p. 263, 1889.

E. MEYLAN—Thury's Automatic Regulator.—*Lum. El.*, vol. 31, p. 274, 1889.

ANON.—Desrozier's New Dynamo.—*Lum. El.*, vol. 31, p. 279, 1889.

E. MEYLAN—Rechniewski's New Dynamo.—*Lum. El.*, vol. 31, p. 301, 1889.

G. RICHARD—Details of Dynamo Construction.—*Lum. El.*, vol. 31, p. 314, 1889.

ANON.—The Kummer Moter.—*Lum. El.*, vol. 31, p. 329, 1889.

W. LAHMEYER—New Systems of Regulation.—*El. Zeit.*, vol. 10, p. 79, 1889.

## III.—ELECTRO-CHEMISTRY AND ELECTRO-METALLURGY.

BELLATI and LUSSANA—Occlusion of Hydrogen by Nickel.—*Beiblätter*, vol. 13, p. 95, 1889.

W. OSTWALD—Relation between the Composition of Ions and their Velocity of Motion in an Electrolyte.—*Beiblätter*, vol. 13, p. 96, 1889.

J. H. VAN T'HOFF and L. T. REICHER—Dissociation Theory of Electrolytes.—*Beiblätter*, vol. 13, p. 98, 1889.

A. MINET—Introduction to the Study of Electro-Chemistry.—*Lum. El.*, vol. 31, pp. 256, 322, 380, 1889.

ANON.—Production of Hydrogen by Electrolysis for Inflation of Balloons.—*Lum. El.*, vol. 31, p. 277, 1889.

M. K. LAURENT—The Hermite Bleaching Process.—*El. Zeit.*, vol. 10, p. 94, 1889.

## IV.—ELECTRIC LIGHT.

E. DIEUDONNÉ—The Edison Central Station at the Palais Royal at Paris.—*Lum. El.*, vol. 31, p. 201, 1889.

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Institution of Electrical Engineers.

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The One Hundred and Eighty-eighth Ordinary General Meeting of the Institution was held at the Institution of Civil Engineers, 25, Great George Street, Westminster, on Thursday evening, March 14th, 1889—Professor W. E. Ayrton, F.R.S., Vice-President, in the Chair.

The minutes of the Ordinary General Meeting held on February 28th were read and approved.

The names of new candidates for election into the Institution were announced and ordered to be suspended.

Donations to the Library were announced as having been received since the last meeting from the Executors of the late Sir William Siemens; the Director-General of Telegraphs in India; and Sir William Thomson, President; to whom a hearty vote of thanks was duly accorded.

The Chairman: The discussion will now commence upon the paper read to us at the last meeting by Professor G. Forbes on "Some Electric Lighting Central Stations in Europe, and their "Lessons." I will not delay you by any introductory remarks, as the importance of the subject is obvious to the meeting. Professor Ayrton.

Mr. Gisbert Kapp: I will not detain you for more than the ten minutes allowed to each speaker. Before entering upon the Mr. Kapp.

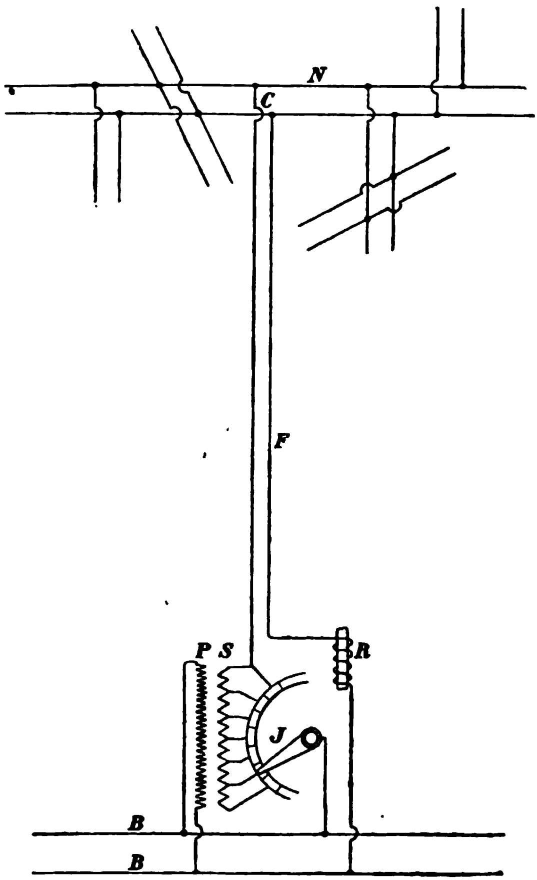
Mr. Kapp. paper itself, I would like to ask Professor Forbes a question. In describing the Berlin installation he says, "They supply 36,000 "lamps of 16 candle-power, and 144 arc lamps of 15 amperes." Later on, when he comes to the question of cost, he speaks of feeders in connection with the glow lamps, and says that the "cost of underground cables has hitherto amounted to about "£90,000, half of this being spent in mains and half in feeders." I should like to ask him whether this sum is inclusive of the underground arc-light wires or not.

After reading, and again reading this paper, I could not escape the impression that Professor Forbes has not a very high opinion of central station engineering as carried on in this country; and he charges us especially with having neglected to use feeders. Professor Forbes is a great authority on electric lighting questions, and therefore it is important to clear ourselves of what appears to be an unmerited censure. The question of feeders is one which has been considered from the very earliest days when central station lighting was thought of. Edison did it from the very first, and our first steps in England were taken mainly by trying to copy Edison as closely as possible, and everybody saw the importance of feeders and adopted them. I may mention that when I was employed by Mr. Crompton, some five or six years ago, we worked out together a station, the machinery for which was to be erected at Victoria, and the arrangement of feeders entered into our plans from the very first, We also designed the various boxes for facility of connection and testing. There were, first, feeding boxes; next, what were called distributing boxes, and a third kind which were called house or service boxes. We not only designed the station with the feeders, but also made provision to keep the pressure at the end of the feeder constant, and by the assistance of Mr. Willans we got out a differential regulator.

The Willans regulator, instead of being simply shunt-wound, as it is when you want to regulate the pressure close to the dynamo, was provided with a differential coil. By that arrangement there was in the main coil of the solenoid of the regulator a current proportional to the main current going out, and in the

shunt coil a current in the opposite direction which was proportional to the pressure at the terminals of the dynamos. This arrangement, as everybody knows, will keep the pressure constant at the end of the feeder, and it has been adopted in various stations, amongst which I may mention Leamington; so that, even in this detail, English engineers have been at least as early in the field as their foreign colleagues. I am sorry that Professor Forbes did not give us more details of the way in which feeders are worked. A person not understanding much about it, or a person who wishes to learn from this paper, would get the impression that all you have to do is to put plenty of feeders in and all will be well. That is, however, not so; it is not enough to have feeders, but you must also have some means by which you can regulate the current and pressure in those feeders. The earliest method of control has been by rheostats in the feeders, and here is a curious point which has been frequently overlooked. It is not sufficient to have a rheostat in the feeder of one pair, but you must have rheostats in both the outgoing and returning feeder. If you regulate only one feeder, you can keep the absolute potential at the end of that feeder constant, but not the difference of potential between the two feeding points. In this case you could only effect an equalisation to half of the difference which is due to the resistance of the feeders. To get perfect regulation with rheostats, you must have them in both feeders, and they must be mechanically coupled in such a way that the switches of both rheostats shall act simultaneously. I believe this is the system which has been actually adopted at Bradford. There is, however, a better way of regulating the pressure at the feeding points. The insertion of a rheostat in the feeder is a very crude device, because if the current is large the resistance of the rheostat must be exceedingly small, as it is not expedient to use up more power than absolutely necessary in the rheostats. If the current is small, the resistance of the rheostat must be very large indeed to make a sensible difference, and for this reason the resistance coils must contain a great deal more material than corresponds to their normal work. The better plan is to insert some counter electro-motive force cells, as has been tried at St. Mr. Kapp.

Mr. Kapp. Austell. With cells the regulation is less gradual, but you can regulate within one per cent. if you work at 100 volts at the feeding centre. You can either rise by two volts, or lower by two, so that you have a margin of error of one per cent.; but the system has the great advantage that it does not use up any energy and is effective over a very large range of current. With




an alternating current system a rheostat has been used—or, at all events, patented—by Mr. Westinghouse, and it is sufficient in that case to regulate one feeder only. But regulations can be obtained in a better manner by the apparatus shown in the diagram.

Imagine that these two bars, B, are the omnibus bars in a station from where all the feeders start. One feeder, F, only is shown, and this is shown feeding into a network of mains, N, laid along the streets of a part of the town which may be several miles away from the station. Let us say that we work at 1,000 volts. If we keep 1,000 volts at all times between the omnibus bars, and if the feeders are simply joined to them, we would have a drop in pressure at the feeding point, C, proportional to the current supplied. To make up for this drop, we can raise the pressure at the omnibus bars by a corresponding amount which could be indicated by a differential voltmeter, and thus for this one feeder we could get constancy of pressure in the limited area served by the network of mains connected to it. But we may have some 10 or 20 feeders going out from the station, and each giving rise to a different drop in pressure. This will especially be the case if the station supply districts of different character; one, let us say, containing residential houses and chambers, another warehouses or business premises, another theatres, and so on. If we feed all these districts from the same omnibus bars, it will be next to impossible to regulate the pressure so as to suit them all. In the office district no light is required after 7 o'clock, whereas in the residential district a maximum of light is required at that time. Again, in the residential districts the supply falls off very much between 8 and 10 p.m., whereas the theatres remain full on up to nearly midnight. To suit these different cases it is necessary to be able to adjust the pressure at the station end of the different feeders independently, which would either require the use of a separate machine for each feeder, or if they be worked from omnibus bars, the pressure between which is kept constant, it would require the use of some apparatus which will raise the pressure at the station end of each feeder independently of the pressure on the omnibus bars and other feeders. On account of its greater simplicity, the latter plan is preferable, and it may be carried out by inserting the secondary coil of a small transformer into one lead of every pair of feeders. The size of this transformer depends on the maximum current for which the feeder is intended, and on the most economical loss in the feeder. Suppose at a Mr. Kapp.

Mr. Kapp. standard pressure of 1,000 volts it would be economical to make our feeders of such a size that at full current they would lose 50 volts. The regulating transformer must in this case be so designed that it can add 50 volts to the pressure in the feeder when the full current flows. The arrangement is shown in the sketch; the primary, P, of the transformer is permanently connected to the omnibus bars, B; the secondary, S, containing only a few turns of stout wire, is subdivided into sections of which one or more may be inserted by means of a switch, J. This arrangement is provided to enable the attendant at the station to vary the amount by which the pressure in any feeder is raised. The secondary may conveniently be divided into five sections, each representing 10 volts, so that the pressure may be adjusted in steps of 10 volts. This is one per cent. of the standard pressure, and the greatest possible deviation at the feeding point can therefore only be ½ per cent.

It depends upon the position of the switch by how much the pressure will be raised by the passage of the current through the transformer; and in the only case in which I have as yet applied this instrument, the switch is set by hand, but I propose to work it in future by an automatic device which is controlled by some thermo-electric or magneto-electric relay, R. I must point out that the switch lever is divided into two portions, so that in passing from one to the other contact the switch never short-circuits any section of the secondary coil.

The only other point that I should like to draw attention to is a question upon which, unfortunately, I find myself at variance with Professor Forbes, and that is the question of frequency. He says that high frequency is better because you get a smaller dynamo and a smaller and cheaper transformer. This may be right in theory, but if we look to actual practice we find that it is not the case. Professor Forbes, at the Bath meeting of the British Association, gave a paper on the Westinghouse system, and he gave as the weight of a 40-light transformer 160 lbs., or 4 lbs. per lamp. Now, my own 50-light transformer weighs 200 lbs.: that is also 4 lbs. per lamp; but my frequency is 70, and that employed by Westinghouse is 133. You see that, although

the ratio of frequency is almost 1 : 2, the weight of the transformer is the same for both. The low frequency, Professor Forbes says, is advantageous for parallel working, because giving time for the machines to pull each other into step. There I cannot agree with him; the interval is too short (the 100th or 200th part of a second) for any single pull to have much effect. The action of pulling into step is due to the repeated tugs or pulls which the lagging armature receives from the current, and may roughly be expressed as the product of strength of one individual pull and number of pulls per unit time. How this product is made up is immaterial, and therefore I do not think that low frequency gives us any advantage for parallel working. Mr. Kapp.

Dr. J. A. Fleming: I will follow Mr. Kapp's example, and be very brief in the remarks I make upon Professor Forbes's interesting paper. Dr. Fleming.

The first point I note is the three-wire system. Certainly it is a matter of some surprise that though this system was devised independently in 1883 by Dr. Hopkinson in England and by Edison in America, and that whilst in America there are about 120 stations worked on the three-wire system, we have not a single one here. I have here a sample of the three-conductor tubing that is now manufactured in the United States, and used in those stations. It has not by any means been a simple matter to arrive at success in these conductors. Every one recollects, in the early days of the Paris and Crystal Palace Exhibitions, samples of Edison's tubing: semicircular pieces of copper rod were drawn into iron tubing and filled with bitumen insulation. At first the practice was to simply separate the copper strips with pasteboard distance pieces, and the whole put into cast-iron gas piping, into which the insulation was poured. The difficulties that then occurred in the use of those conductors were these: the copper rods very often were strained, and the bituminous insulation was squeezed out from between them, and the pasteboard distance pieces did not keep the conductors apart satisfactorily. More than that, the iron gas-pipe tubing was not sufficiently strong to withstand hard usage, and especially not strong enough to withstand the active attacks of the pickaxes of road repairers,

Dr. Fleming.

and it got damaged, and trouble ensued. But now the process is somewhat as follows:—The copper rods are respectively spun over throughout the whole of their length with dry string to separate them; the copper rods are then placed centrally in their proper position in the interior of stout steel tubing; a vacuum is made in the tube, and the insulating material is drawn in, and it penetrates into every portion. A difficulty which appears in the use of an extended system of this kind is the result of the stresses that are brought to bear on different parts of the conductor network buried in the roadway, owing to the different pressures that come upon the roadways at different times, causing severance of the connection with the junction boxes. In order to render the network of conductors more flexible, and in some way or other to make it give and take under the roadway, a system of ball-and-socket joints was devised by which the ends of the tubing were connected to the junction boxes by a kind of flexible junction, perfectly watertight, but, nevertheless, allowing a certain amount of elasticity in the network, which could thereby accommodate itself to slight changes of the level in the roadway.

With regard to the subject of feeders. As Mr. Kapp has said, the feeder system is very old indeed. Edison took out his first patent for feeders in the early part of 1880, and it was at once applied in the first district station laid down in New York. I know that Edison attached very great importance to that, even in the early days. With regard to the use of these feeders, and the manner in which they were used to control the potential in the different parts of the network, in 1883, in the New York first district station, the method employed was as follows:—A pair of small wires was run back from the ends of the feeders, where the feeders dipped into the network, to the central station, to enable them to at all times test the potential; and if they found that it was too high, a feeder leading to that point was disconnected, and they were thus enabled to keep the potential constant. I am not aware what at the present time is the method adopted in this central station, but perhaps we shall obtain information on the point in the course of this discussion.

With regard to the very important question of the permanence of the insulation, I think there can be no doubt that whilst our experience has been extensive on the insulation of continuous currents, there is a great deal of experience which will have to be bought expensively upon the insulation employed in dealing with high potentials on the alternating system. When we consider that electric excited energy is conveyed, not along a conductor at all, but along a dielectric, I think it becomes a very important question to discover whether the insulators are of a class that will stand reversed electrical stresses many times a second for years; and if the dielectrics now used will not stand that, then I think the sooner our attention is turned to others the better. Dr. Fleming.

With regard to the point raised by Professor Forbes on the waste of the primary current running through the converters. I think he has mentioned the case of the Grosvenor Gallery installation, where during the hours of minimum supply the current sent out from the station is said to be abnormally high; but it would be necessary to find out whether that current measurement is correct. Of course, if it is the fact that it is so, then what it indicates, I think, is that the converters are starved of iron [Professor FORBES: "Hear, hear"], and an economy has been perpetrated in the wrong direction.

I think those are the chief points upon which it occurs to me to remark. I would say that I have read the paper with great interest, and I am sure there is a large amount of information gathered together in it, which will help all who are endeavouring to solve this problem of the distribution of electricity.

Mr. J. SWINBURNE: The first thing that strikes one in Professor Forbes's very able and interesting paper is that a large proportion of his conclusions will not be applicable to English electric lighting. Abroad, distribution by alternating currents is carried out with a transformer in every house. In this country it is much more likely that a large transformer will be used for each block of buildings. Each will then have its own leads from the station, so that the pressure on the lamps of each block can be maintained constant. We start, unfortunately, late in the day, but, fortunately, with more modern ideas. Mr. Swinburne.

Mr. Swinburne.

Professor Forbes over-estimates the difficulties in coupling direct-current machines in parallel. All that is needed, when throwing a dynamo into circuit, is to regulate either its speed or its excitation till it gives the right, or nearly the right, pressure, and then to switch it in. The engine is regulated for load, and the excitation for speed. As a shunt, or separately excited, dynamo runs in the same direction when a motor, there is no accident, even if the pressure is a little too high or too low, on switching on. The complications described in the paper are really unnecessary.

The difference in German design of large machines may perhaps be due to labour being cheaper there in proportion to material. The fewer poles the better in a dynamo. A large machine cannot be made to work with less than a certain number, but it is best to keep this number as small as possible. It is satisfactory to find Professor Forbes against slow-speed dynamos. I have always advocated a moderately high speed, as it makes a machine more efficient, less liable to break down, easier to construct, and cheaper.

Compounding governors and voltmeters for loss in leads is quite well known in this country, and I think originated here.

We cannot assume broad general rules for the design or efficiency of transformers. They have to be worked out specially for each alternation frequency. A transformer, curiously enough, is not like a dynamo, in that you can approximately calculate the absolutely best form. The equations are very long and tedious to work out, but are not difficult. The transformers at present in the market are, I believe, designed at random: a skeleton is taken and an induction assumed, and the necessary wire wound on. There is thus room for considerable improvement in the design. The main question in design is generally overlooked—that is, not the efficiency of the transformer as a converter, but the variation of secondary pressure with varying load, and constant primary pressure. The 88 per cent. efficiency transformers mentioned in the paper would cause an enormous variation in the efficiency of the lamps on the secondaries, and it is light that people pay for. This fault cannot be corrected by any systems of

feeders or of pilot voltmeter leads, if several transformers are in parallel on each circuit. The transformers in such cases ought to be specially designed for as nearly constant secondary pressure as practicable. Mr. Swinburne.

I notice a slip where Professor Forbes says that some transformers would be more efficient if the iron circuit were open. As I fell into the same error myself, I may point out that the waste energy is $\int I \, d\, H$, and is independent of the source of the magnetic force; the demagnetising action of the ends does not lessen the energy wasted.

Professor Forbes seems to me to exaggerate the difficulties in running large alternators in parallel. I explained a simple "induction coupler" to this Society a few months ago.

Though I much prefer small frequency of alternation, as it makes the machine easier to make, better, and cheaper, I do not see why, other things being equal, a low frequency makes it easier to couple machines in parallel. It seems to be a question of the relation of self-induction to output, &c. The question of settling upon a uniform alternation frequency in this country is one of enormous importance, but it is too early to discuss it, as with our present knowledge we might easily tie ourselves down to a wrong frequency.

Mr. F. V. Andersen: Allow me to make a few remarks on the general question of low-potential distribution. Professor Forbes says that he does not think that it pays in England to use this system for more than a district with a radius of 400 yards. If this is the case, then the system of feeders needs not to be very complicated, or it will not be necessary to go to feeders with very different lengths, or to have rheostats in some of them. 200 yards will be about the maximum distance it will be necessary to carry the appliances for distribution into the houses. Suppose we have a district which has a radius of 400 yards; the mains—which are the two strong black lines in Mr. Kapp's diagram—may be laid in the form of a ring which surrounds the station. The station will very often come within the district, and sometimes near the centre of the district. In that case, if a set of Mr. Andersen.

Mr. Andersen. mains are run at a distance from the station of about 200 yards, with branches like those shown on the diagram, into the streets, none of these branches will be much more than 200 yards long, and in that case a current-density of 600 amperes to the square inch will, on the three-wire system, only give 3 volts fall of potential; the feeders may have the same length, and all the dynamos be run in parallel. This system of using all the machines in parallel is, I understand, the one adopted in Berlin, and one can very readily understand the great advantages that they have found to lie in this simple form of distribution. Of course it only holds for a limited area.

I am surprised to see that Professor Forbes against this system puts his own idea of having dynamos in various groups, and, sometimes, when the load is small, to run a few dynamos and have all the feeders on the one or two dynamos which are running, and change them into more groups as the load increases; because it is a complication, and at times, such as on foggy days, it will be very troublesome to change from one system to another. Professor Forbes mentions 1,000 amperes as the efficient density of current, but I think that it is not very difficult now to work out what the density should be according to Sir William Thomson's law. For Berlin load diagrams have been worked out, and published in excellent papers, for all hours of the day all the year round, and it is quite possible from these to solve the question of what should be the density in a system like this. I think that, with the present price of copper and coal, and the rate at which electricity can be produced, something like 600 to 700 amperes will be found to be an economical density.

As to the question of pilot wires, Professor Forbes indicates that they are generally attached in the wrong place, and that a great improvement can be obtained by putting them in the right place, which he says is midway between the feeding centres and the furthest lamp. I do not think that this can be right. From the one feeding centre on the mains to the other we shall have a very small fall of potential indeed, and therefore shifting the pilot wires along the mains has hardly any effect at all. But to go to the centre between the furthest lamp and the feeding

centre is a different thing, because it would mean to connect them to a branch, and this would not lead to a proper regulation of the district. They will have to go to the points carrying the maximum load, *i.e.*, to points near the feeding centres. Mr. Andersen.

There are some questions as to the average voltage. We find in the paper that "4 volts may be allowed up and down," and I cannot understand this in any other way than that average voltages are supposed to be used; for, if you can allow 4 volts fall, you certainly cannot allow 4 volts in one direction.

Now Dr. Fleming said he thought there was only one proper solution to the question, and that was to use the same voltage all through. I differ entirely from that view. As a rule, when we make an installation with a compound dynamo in a house, we compound the machine so that it shall give a volt more when the full load is on than it gives when there is a minimum load. That gives an ideal regulation in such a case; but in case of distribution from a central station, if a constant potential is kept on the mains (which is the best we can do), then regulation must be obtained in some other way; and the best result will be obtained by using average voltages. It may be quite possible to allow, at least at full load, 4 volts fall into the branches. But *that* installation which is close on the main has only a very small variation, say ·5 volt, which is due to the fall inside the house, while in the branches there will be a fall up to 4 volts; therefore, if it be right to give 100-volt lamps to the houses near the main, then we should give lamps for 98 volts to the furthest houses on the branch, if the fall from the maximum density amounts to 4 volts. The fall of potential depends upon distance and density of current, and nothing else, and all branches when laid down are designed to only go up to a certain density of current with the full load; and since neither this nor the distance can change, we can tell exactly the fall of potentials with which we have to deal. There can therefore be no objection to vary the volts by one or two. It is difficult to see how Professor Forbes can report that it leads to disastrous confusion, since it is only a question of a volt or two, and it certainly is a step towards perfection to use average potentials. Of course the voltage cannot vary, as Professor Forbes

Mr. Andersen.

says, in the way that houses requiring 100 volts one day should require 99 the next. This can only happen if the houses are moved up or down along the branches.

Mr. Siemens.

Mr. ALEXANDER SIEMENS: My remarks will refer to one only of the various subjects that Professor Forbes brought before us in such a very interesting and admirable manner at the last meeting—that is, to the insulation of the Berlin cables. These cables consist of a copper conductor surrounded by jute, which is impregnated with a compound and then enclosed in a lead tube. The lead tube is further protected by a covering of jute and compound, and then by an iron armature, to screen it against mechanical injury; and that, again, is wrapped round with jute and compound to protect it against rusting. Professor Forbes told us last meeting that cables constructed in this manner will certainly break down after about three years' use, and the explanation given him in Berlin was that a galvanic action was set up between the iron armour of the cables and the lead. Now this subject is very important, because those cables were designed to meet a great want for underground conductors. You are, of course, all aware that gutta-percha and india-rubber are the principal insulators for cable work; but gutta-percha for electric light wires is not at all suitable, because, whatever you may do, the electric light wires are occasionally over-heated, and that would make the copper conductor sink through the gutta-percha and touch the lead; so that gutta-percha, which would otherwise be the best, is out of the question. India-rubber is rather expensive, and there is at least a little doubt whether it will remain permanent. Therefore a good many experiments were made, and the insulation just described was settled upon as being the most suitable for these sort of wires. Knowing that Professor Forbes had been not long ago in Berlin, and that he obtained his introduction to the Allgemeine Elektricitäts Gesellschaft through Dr. Werner v. Siemens, I thought, when I heard this unfavourable account, that it had been verified by referring to Messrs. Siemens & Halske, but I find that such was not the case. The reasons why Professor Forbes was supplied with inaccurate data I do not know, but it is certain that this general

breakdown has not taken place. The history of these cables is that in 1882 about 7 kilometres of them were laid for the arc light circuits in the Leipziger Strasse. These cables were not armoured with iron at first. In 1885 the installation of the central station began, and at the same time about half of these unarmoured cables were exchanged for iron-armoured cables; and in 1886, 1887, and 1888 more cables were laid. 142 kilometres of these cables are at the present time underground in Berlin, and up to August, 1888, no interruption whatever occurred in this extended cable system. About this time the three separate stations shown on the diagram were united on a plan which had been designed by the Allgemeine Elektricitäts Gesellschaft, against the advice of Messrs. Siemens & Halske, because a testing of the cable system was made highly inconvenient—one could almost say impossible. Then, between August, 1888, and the beginning of November of that year, four different places in the system were found faulty. Altogether thirty-five cables are passing these four places, and of these only eight had to be partly exchanged in order to repair the damage done. One of the eight was pierced by a pickaxe; another was making earth by one of the screws in the coupling being badly fitted in and touching the iron of the jointer; a third became also faulty through careless fitting; and in the other cases the cables were so badly burned that it was not possible to find out afterwards what was really the first cause of the fault. Messrs. Siemens & Halske are strongly of opinion that in each case the mischief was started by some mechanical injury to the cable, and not a galvanic action as suggested to Professor Forbes. Another circumstance is that it was not the cables which were laid in 1885 which went wrong, but those which were laid in 1887 and 1888. This shows that the statements were made to Professor Forbes rather recklessly, and I regret his having put to us in such a very definite manner that the life of these cables was only three years, when none of the three-year-old cables had become faulty.

Mr. Siemens.

This corrosion of the armour covering which has been observed, and which has been shown to Professor Forbes, and

Mr. Siemens. also to Mr. Crompton, I think, has only taken place in cables which were electrically at fault; the other cables which were lying immediately alongside of the damaged cables were not touched at all, although they were exactly under the same conditions. Then another matter which Professor Forbes seems to have overlooked is that the mere juxtaposition of iron and lead, with jute between, would not give rise to any current at all. It can only give rise to a current if there is metallic connection elsewhere between the iron and the lead; and even if this, by some accident, had been effected, then the *iron* would be eaten into and corroded, but not the lead. There was, I believe, a good deal of friction between the Allgemeine Elektricitäts Gesellschaft and Messrs. Siemens & Halske about these faults being discovered; and as a matter of precaution a good many other places—I think about forty places—of the cable system were opened to ascertain the condition of the cables, an electrical test being impossible on account of the peculiar arrangement of the whole system. In all those cases the cables were found to be in perfect condition, only in several places it was found that the cables had been heated to such an extent that the external compound of asphalte had been melted together with the surrounding earth, without injury to the insulation: that is about the hardest trial which the insulation of a cable could be subjected to; and it is also a fact that these cables have, since November, 1888, given no trouble whatever after the faults were cut out by replacing about 250 yards out of 144,000 yards. On referring to Mr. Rathenau—the same gentleman who gave Professor Forbes that unfavourable opinion—he confirmed that the cable system is in perfect order at the present date, that there is nothing the matter with it whatever, and that the whole trouble was simply in those four places of which I have spoken.

I have thought it right to go so much into detail about this point because a reliable insulation for underground electric light wires is very much desired, and it would be a pity if this insulation were condemned on such very slender grounds.

In addition, I will only remind you that Professor Forbes has told us himself that in Milan and in Rome these cables have

given perfect satisfaction, and that in Rome concentric cables of this construction have been in use with 5,000 volts of alternating currents for two years. So that I really think there must have been some other reasons than real failure which have prompted the adverse opinion which Professor Forbes has brought over to us. Mr. Siemens.

The CHAIRMAN: As Mr. Shoolbred is interested in the Bradford installation, perhaps he can tell us why the three-wire system, which is so common in the United States, was not used there? Professor Ayrton.

Mr. J. N. SHOOLBRED: Amongst the many and important points of this interesting and instructive paper the one that particularly strikes my attention is that which has already been dwelt upon at some length by Mr. Alexander Siemens; viz., the wholesale condemnation of lead-covered cables for underground work which is contained in it. Professor Forbes uses strong language with regard to lead-covered cables, and condemns not only those used at Berlin, but also those of other makers, and which are largely used in America and in Europe. I myself would be glad if he would, in his reply, give us some evidence, and further information on the point. Probably during the discussion something may be forthcoming from the makers. Indeed, while making so sweeping a charge, and one which is most important in the present stage of electric lighting, I certainly think that Professor Forbes should have adduced some reasons, and also *some facts*, when stating that he regretted "to have to tell us "that this insulation has been a failure." Mr. Shoolbred.

With regard to the question which the Chairman asked just now about the Bradford installation. I have carefully avoided any allusion to it because I think it is hardly fair, either in that or in any other installation which is in process of construction, to refer specially to it until after it has been completed.

Respecting the inquiry, as to why the three-wire system was not adopted at Bradford. The impression generally seems to be that the Bradford installation is intended to be only a low-pressure one. It is ultimately intended to be a high-pressure installation, to be extended to long distances. The portion of the installation

Mr. Shoolbred.

now under construction is small in proportion to what may be ultimately carried out. Besides, in the present stage of electrical information it seems pretty generally conceded that, for high pressure, probably a two-wire system would be more convenient than a three-wire one.

The merits of the three-wire system were very carefully considered, not merely by myself, but also by the Electricity Supply Committee of the Corporation. But, after personal consultation with Dr. Hopkinson, they decided not to adopt the three-wire system—at present at least—as any economy in the cables, arising from its adoption at that early stage, would have been more than counterbalanced by the cost of the increased number of smaller dynamos which their series arrangement, in pairs, would have entailed.

Furthermore, the three-wire arrangement in the streets might lead to complications in premises where the proprietors preferred the two-wire system. Again, the facility afforded by the three-wire system for doubling the E.M.F. (which has already been referred to during this discussion, and as an advantage) would, most probably, have led to difficulties with the Board of Trade, owing to the limiting E.M.F. for houses having thereby been exceeded.

There is nothing, however, in the Bradford installation, as at present being laid down, to prevent, if thought advisable, the adoption of the three-wire system at a future time.

Mr. Robinson.

Mr. Mark Robinson: In showing us the excellent work which has been done abroad, Professor Forbes was careful to express a belief that Englishmen would in due time do work no less successful. I think he is right; and in one small branch of the subject, on which alone I feel able to say anything, I can give some reasons for endorsing his hopes. Professor Forbes laid stress, very justly, upon the question of economy in the motor. Whatever may be said about the first cost of apparatus, and about the cost of working, in the long run the main point to keep in view in electric lighting is the coal bill; the more electricity can be got out of a ton of coal, the larger, if other things are equal, the dividend will be. Special reference has been made

to the very fine 400-H.P. Corliss condensing engine in the central station at Berlin, and it has been mentioned that it gave an electrical horse-power for 15·5 lbs. of water evaporated. When the paper was read I was under the impression that Professor Forbes spoke of *indicated* horse-power; and as 15 lbs. per I.H.P. is considered good work for a large compound Corliss engine in this country, the mistake was not unnatural. But it appears that 15·5 lbs. was given to Professor Forbes as the consumption per *electrical* horse-power—a marvellous result, because it appears to point either to a consumption *below* 13 *lbs.* per I.H.P. in the engine, or else to a quite phenomenal efficiency in the dynamo. One cannot but wish for detailed figures of such tests as these, and for fuller particulars generally. Certainly we cannot match such figures in England yet; but figures can be given, as to which no doubt exists, which are at least of happy augury for our progress in the future. I have seen an English condensing engine giving an indicated horse-power upon 15·1 lbs. of water. Like the Berlin engine, it was direct driving; and it was coupled to a Crompton dynamo. The differences were these: it was not a large engine, indicating 400 horse-power, but a very small engine, indicating only 40 horse-power; it was not running at only 80 revolutions, but at 400; it was not, like the Berlin engine, designed as a condensing engine, but expressly for non-condensing work. Had it had the same advantages of size and of special adaptation, it seems not an unreasonable belief that its consumption of water might have rivalled that of the Berlin engine. Considering how much is gained in point of first cost by the much higher speed of the English engine, it will probably be conceded that, even as it stands, its record is a good one to show for English work; and as English engineers engaged in other branches of electrical work, such as dynamo-making, have no doubt relatively quite as good results to show, it is clear that Professor Forbes's hopes on our behalf are well founded. Mr. Robinson

Mr. W. Lant Carpenter: There are three points, Sir, that, with your permission, I should like to refer to in connection with this interesting and valuable paper. The points are—Feeders, the three-wire system, and insulation. Mr. Lant Carpenter.

Mr Lant Carpenter

I certainly felt, in hearing Professor Forbes's paper, and after reading it, that he was to some extent flogging a dead horse, or killing the slain. It so happens that within a year or two it has been my lot to hear details of the plans of about twenty central lighting stations in England, and in each of them feeders have been provided for. I was in Bradford some months ago, and saw them being laid there in the municipal installation, and I know people who have laid no less than 200 feeding mains to separate installations in town and country in different parts of England.

With regard to the three-wire system, I should like just to point out that the doubling of the pressure of which Professor Forbes speaks is not exactly the chief advantage, but it is only one of several advantages; and I am very sorry that Dr. Hopkinson is not here, so that we might have had the advantage of a little further exposition of it. But by feeding on this system, as is probably well known, one outside wire comes from the positive pole of one dynamo, and another outside wire comes from the negative pole of the second, and when these two are properly balanced the current in the third or neutral wire is nil; and hence, there being no current, there is no resistance to allow for in the return wire, and hence there is a great saving; and I venture to ask Professor Forbes whether, in his paper, where he is speaking of the drop of 480 yards with 1,000 amperes, the drop should not be 6 volts instead of 12.

Professor Forbes: No; the 12 comes in by the third wire.

Mr. W. Lant Carpenter: I am open to discuss it privately, but I adhere to my view of the case.* I would just point out that, so far as I am aware, in practice the best size for the neutral wire is one-third, and not one-fourth, that of the active wire.

Then with regard to what Professor Forbes has said about bituminous compounds. I think we ought to distinguish between the coal tar products of various kinds which are often called bitumen, or bituminous, and that bitumen which Dr. Fleming has alluded to as having been used by Edison, and which

* Professor Forbes ultimately admitted the correctness of my view.—W.L.C.

is, one may say, produced in Nature's laboratory, *i.e.*, is a natural product. I think that it is to the coal tar bituminous products that failures are generally due, so far as has been known. I venture to assert that with real bitumen no such effects will be got, and I could mention cables that have been thus insulated which have been laid for far more than three years without any sign of injury to the insulation. There are a large number of stations which I could mention, both in this country and in the United States, which have been so laid, only the list is so long that I should not like to name them, because we are so short of time this evening. Mr. Lant Carpenter.

With regard to the question of vulcanised rubber cables,—if such cables are covered with lead, the lead will be attacked by the sulphur as readily as untinned copper would be; so that if vulcanised rubber is to be used, then a large quantity of tin must be added to the lead, and that will make the cables hard to manipulate in the laying.

Mr. R. E. Crompton: While agreeing with Professor Forbes as to the vast amount to be learned by the study of foreign central stations, I must point out that he has, I think, in his own person shown us that there are disadvantages attaching to this mode of study, viz., that it is extremely difficult for any travelling outsider to get at the real facts when conflicting interests are at work. In this particular case of the behaviour of the lead-covered cables at Berlin, no doubt the officials at the Allgemeine Elektricitäts Gesellschaft put the case before Professor Forbes very strongly from their own point of view. They did so to me also; but, after what Mr. Siemens has recently said, it is probable that there is another side to the story. I first wish to ask Professor Forbes if he has not made an error in describing the output of the Berlin central stations. He gives it as if the majority of the output was in incandescent lamps; whereas I was told, and from my own personal observation it seemed to be correct, that the output from the low-tension mains was very largely in arc lamps: possibly two-thirds of the whole output was thus taken. I cannot speak too highly of the wonderful engines made by Van den Kirchove, the drawings of Mr. Crompton.

Mr. Crompton. which are before you. They are most perfect specimens of workmanship and design. Mr. Van den Kirchove has solved the extremely difficult problem of making engines of this class run with perfect quietness and freedom from shock at the time the connecting rod passes over the lower centre. They work perfectly noiselessly, the sole exception being the slight click of the Corliss valves. As an English engineer I must confess that our manufacturers of steam engines cannot teach Mr. Vàn den Kirchove anything in this respect. The previous speaker, Mr. Robinson, has, however, pointed out that these engines are very big and very costly for the work they have to do; and it is quite possible that the use of such large and costly engines will not be as profitable to the company as if they had used smaller, quicker running engines of the same power, but of the type we use in England. Speaking generally, the whole of the machinery at Berlin is carried out on a most magnificent scale; but I hardly think that, if we wish to get the best economic results, we shall copy this machinery in England. The economy of these engines, viz., 15½ lbs. of water per E.H.P., appears to be extremely high. Such economy is just possible, but I should like to know in what manner the tests were carried out. Similar tests carried out at Vienna showed that recently we have been obtaining the result of 22 lbs. of water per E.H.P., but in this case the tests were carried out by independent authorities. Before I leave the question of the Berlin and other central stations, I must ask Professor Forbes whether he thinks it is really good modern practice to use banks of lamps or other artificial resistance when low-tension dynamos are to be brought into parallel working. I have eight large sets of engines and dynamos running parallel at Vienna, and from the commencement we have not had the least trouble in throwing them into parallel working without the use of any such devices. Mr. Melhuish, the resident engineer of that station, has in one of the technical papers shown how easily and conveniently an additional machine is thrown into circuit by means of pilot lamps, which show when the E.M.F. of the fresh machine comes to be equal to that of the line. At this moment the circuit is closed, and the machine takes its share of the load without any flicker in the lights.

I now come to the three-wire system. I fully agree with what Professor Forbes has said about it; but I wish to point out that a great many engineers, including my own firm, have often been obliged to continue to use the two-wire system, although they know perfectly well that it would be economically advantageous to change to the three-wire one. Several stations that I know of were designed for the three-wire system originally, but so long as the demand was small and irregular it was found very much easier to work the station on one parallel only, the intention being to make the change as the station grew larger. In some cases, however, the demand has come on so suddenly that it has not been found convenient to make the alteration at once. The author has referred to Sir William Thomson's formula for the calculation of the copper in electric mains as being constantly in use. I must point out that, with every desire to make use of this formula, it has been extremely difficult to do so up to the present time, as it was next to impossible to obtain the time factor necessary to work it out commercially. For the first time I have got a time factor calculated from the average number of hours the feeder mains are worked at certain currents, but until I obtained this time factor it was useless to attempt to apply the law. On this point I could have learned very little from the study of foreign stations. The habits of the dwellers in Continental towns are so different from our own that the time factors for Berlin, Vienna, or Paris would be found to differ widely from those useful to us in London. In those towns people mostly dine out. A large portion of the lamps are used in restaurants or places of public entertainment, so that the hours of maximum lighting are spread over a period much longer than is the case in England. At Berlin, for instance, in December the load only varies slightly from 6 to 10 p.m.; whereas at Kensington it would reach a maximum at 7, and one hour later would be reduced nearly one-half. The maximum really corresponds with the time when everybody is dressing for dinner and the shops still remain open. As regards the point at which pilot wires should be coupled on to a network, I must agree with Mr. Andersen that I cannot see how Professor Forbes is going to couple on these pilot wires so as to Mr. Crompton.

Mr. Crompton. obtain the end that he seeks. The desirable object that he seeks to obtain has been before us all, but it apparently cannot be applied except in the case of distributing mains which are very completely coupled up into a network. If we imagine that the maintenance of the required difference of potential all over a network was something like that of maintaining a film of fluid of a given thickness all over a flat porous absorbing surface, the supply being kept up by means of feeder tubes spaced evenly over the surface, we should then find the film decreased in thickness towards that part of the surface which is midway between the feeders, and where the absorption is most considerable. It is evident that where Professor Forbes would wish to join on his pilot wires would be at the points midway between the feeders and the thinnest part of the film; but this appears to be impossible, except in very exceptional circumstances, partly owing to the difficulty of thoroughly connecting up and completing the network, and partly owing to the uncertainty of knowing at which points the demand will be heaviest.

I now come to the use of the air as a dielectric. Professor Forbes begs that I would state authoritatively what is the maximum and minimum resistance of the air-insulated mains at Kensington, because, he said, nothing had been stated definitely about it. This is true. The air-insulated mains at Kensington are so mixed with the continuously insulated cables that we have great difficulty in testing any considerable lengths of the former by themselves. We have four or five miles of these mains at Kensington, but owing to the fact that we never have the current off night and day, it has been found impossible to carry out delicate quantitative tests. We do know that the insulation is very high—far higher in the case of the air-insulated mains than in the case of the continuously insulated ones. Professor Adams, at my request, made an attempt last week to test a short length of main which is only partly finished. This section was recently laid, and in all probability its insulation was at its lowest, as the weather was damp and it was not in use, so that the surface of the insulators could not be dried by electrolysis, which, of course, is ...rmal condition of a working main. The insulation resist-

ance of a length (including one crossing made up of continuously insulated cables drawn in tubes) was at the rate of 380,000 ohms per mile. Although this result is fairly good, yet I do not think that it for one moment represents the insulation resistance of the bare mains, but rather that of the four joints on to the continuous cables at the extremities of the crossing. As most of you present well know, we almost invariably find great tendency to leakage at the joints of continuously insulated cables. It is a comparatively easy matter to get high insulation where the continuous insulation is unbroken; but every joint, whether it is a T joint for the house services or a joint on the main line, is always a source of leakage and trouble. We find that the continuous insulation of a line of conductors is usually so high, and that of the joints so low, that when we talk of the insulation of the whole line we are really speaking of that of the joints only. Mr. Crompton.

I wish to add a word of personal explanation. Some gentlemen have recently seemed to think that although last year I took up a position hostile to the alternating transformer system of electrical distribution, recently I have modified my views. I wish to point out that nothing of the kind has taken place. I never was hostile to the alternating transformer system. I pointed out that probably it was not so suitable or so cheap as the battery transformer system for the lighting of densely populated towns such as London, but that the alternating transformer system was well suited for less densely populated centres. I am of the same opinion still, and am consequently consistent in advocating each system for the purpose which suits it best. All, however, that Professor Forbes has put before us has only confirmed my own views that the alternating transformer system of distribution is not nearly so simple or so easy to manage as last year he wished to lead us to believe.

Mr. Wynne. Mr. FRANK WYNNE: Professor Forbes has referred to the Westinghouse pressure indicator in very high terms, for use in central stations. In Westinghouse's price list a description is given of this, but so slight and involved that to understand it I had to refer to *Engineering* of 22nd February, where a long description

Mr. Wynne. is given of it under the name of a "compensator." But neither Westinghouse nor Professor Forbes make any acknowledgment of the real author of this important instrument being Dr. John Hopkinson, who describes it at length in his well-known three-wire patent of 1882. Mr. Westinghouse no doubt deserves credit for the way he has worked it out; but the only difference that I can find is that Westinghouse uses the secondary wires from a converter to wind Dr. Hopkinson's instrument, instead of using the primary wires.

Mr. Preece. Mr. W. H. Preece: There is no practice more conducive to the welfare of this Institution than for our members to carefully inspect what is being done abroad, and to come here and give us the result of their inspection. I have been a culprit in this direction myself, and therefore I am rather timid at daring to criticise the work that has been done by Professor George Forbes. But I am strongly inclined to think that Professor Forbes has put the boot on the wrong leg. Instead of going abroad,—instead of spending his Christmas holidays in examining the central stations at Berlin, Milan, and Rome, and coming here and throwing down the gauntlet to English electrical engineers, and telling them that they did not know their own business,—he would have done wisely if he had visited some of our central stations, and gone to Berlin, to Milan, and to Rome, and told them what to do.

We are not altogether deficient in central station working in this country. It is perfectly true that we are a long way behind America, and in some things we are always prepared to follow the lead of America. There is a certain energy and a go-aheadedness about that nation that imparts itself to us when we go there, and we all feel better men when we come back again. We are always glad to follow their lead; but we do not like to be told that Italy is leading us in this particular direction.

I say that we are doing something in central stations here. Has Professor Forbes inspected the central station in Bond Street and under the Grosvenor Gallery since it has been under the charge of Mr. Ferranti? There is a kind of fashion to decry the work that has been done at the Grosvenor Gallery: we hear innuendos about the lamps being "like red hair-pins," and things

of that kind; but, gentlemen, the Grosvenor Gallery and its Mr. Preece. spirited proprietors have maintained electric lighting alive in England, and it is due to their energy that at the present time we find capitalists ready to open their pockets and support electric lighting in this country. It is scarcely a fortnight ago that a quarter of a million of money was wanted to complete the Deptford central station, and in less than one hour that money was found in the City of London; and if a million pounds were wanted to-morrow morning, a million would be forthcoming. Well, there are other central stations. What do we hear about Brixton? What do we hear about Holborn? Who is there that walks along Holborn and does not see lamps, arc and incandescent, springing up all round and about? What have we heard about Pritchett's station in Rathbone Place? Who is there that drives along Oxford Street and does not see those beautiful lamps lighting up shop after shop? Kensington Court is quite capable of speaking for itself; Whitehall Court is not to be scoffed at, nor the Cadogan installation at Chelsea. Again, there are two large stations each of which tells us a lot: one is at Silvertown, where the India-Rubber and Gutta-Percha Company have something like 150 arc lamps and 3,000 or 4,000 glow lamps going, and worked in a way that is a credit to any English firm; the other is an installation that people are sometimes apt to turn their noses up at—that is Paddington, where over 100 arc lamps and 4,000 glow lamps are lighted up, now under the charge of our respected Past-President, Mr. Spagnoletti, who will probably speak for himself on the next occasion; but is there anybody in this room who has heard of any breakdown there? No; it is only when breakdowns occur at a little place like Barnet that we see our papers narrating them with seriousness. Why, there are not so many lamps alight at Barnet as there are in my own house, while the vagaries of the Barnet lamps have occupied columns of our technical press. So much for London.

Are we doing anything in the country? Has Professor George Forbes inspected the central station at Liverpool? There, there are 4,000 or 5,000 lamps going. There is a place not far from his own native heath—Glasgow—where Messrs. Muir

Mr. Preece. & Mavor have a central station, which, by the bye, supplies our own Post Office. Then, coming further south, we have a central station at Taunton, and another which has met with a certain amount of obloquy—that is at Leamington. The reason that we hear so much of the Leamington installation is that contractors supplied 16-candle-power lamps for the streets, which was an act of folly. But apart altogether from street lighting, if anybody goes to Leamington and examines the light in the houses, or sees the illumination of the Town Hall, he will say that there is no building in this world that is more brilliantly or more beautifully illuminated than the Town Hall of Leamington. Let us go further south again. Has nothing been done at Hastings? Has nothing been done at Eastbourne? Why, for the last seven years, Mr. Sayers, Mr. Lowrie, and Mr. Hall have been working away with all the energy, nerve, and fervour of Englishmen, and have established an installation there that has been transplanted to West Brompton; and I do not think Professor Forbes could bring an instance from America which has been carried out more perfectly. We will take another place—Brighton—and I thought we should have heard to-night from Mr. Wright something of what he has done at Brighton. There are many who may know Mr. Wright, and it is a charming thing to go to Brighton and see the way in which that gentleman has applied, unaided, the most exquisite automatic apparatus to tell him everything that is going on. I will not deprive you of the pleasure of hearing him next time. He has promised to come and show us what an English engineer can do at an English central station.

There is one point upon which I should like you to have clear and definite ideas, and that is the question of the durability of lead. That is a point upon which Mr. Graves and I can speak with some authority. I have here a specimen of lead-covered wire. It was not laid down in the year 1887, or in the year 1886, but it was laid down in the year 1844; forty-five years ago this lead-covered wire was laid down in the streets of London. It was picked up last year, or the year before, I think in the neighbourhood of Vauxhall (Mr. Fleetwood will correct me if I am wrong), and it is as perfect as the very day it was put down.

The copper wire is coated with a mixture of pitch, resin, and beeswax, and the reason the wires failed and were replaced by gutta-percha-covered wires was not because the lead failed, but because the copper conductors fell through the pitch and came in contact with the lead. I have another specimen here that is well worth your examination also, and I am indebted for this specimen to Sir Albert Cappel, who is here to-night. It is a specimen of lead-covered wire which was dug up at Kiddapoor, near Calcutta: it was put down either in 1851 or 1854; it was taken up in 1885, and it also is quite perfect. Then, in the year 1854, Mr. Latimer Clark, who is here to-night to confirm what I say, started the pneumatic tube system for the transmission of telegrams. Tubes of lead were placed in iron pipes unprotected in any way or shape, and those lead pipes have remained in their protecting casing of iron from that day to this—1854 to 1889—thirty-five years; and I can say, in the presence of Mr. Graves, Mr. Bell, Mr. Fleetwood, Mr. Latimer Clark, and many other officers who have had plenty of experience, that we have never yet seen a sample of lead that has in any way deteriorated from its contact with iron.* Mr. Preece.

But I have had specimens of lead which have decayed. I remember a case where a lead-covered wire was laid through Windsor Park—it must have been about the year 1868 or 1869; it was known as "Marshall's cable," and was a lead tube with a copper wire run through it, the copper wire being wrapped with cotton and the tube filled with paraffin wax. That very speedily decayed. It decayed in patches, and it was found that wherever the lead-covered wire lay unprotected in decaying vegetable matter, then the lead was destroyed.

Well, now, gentlemen, the way in which lead is converted into the white lead used for paint is by burying grids of lead in mounds, as it were, or layers, of tannin, moistened with vinegar or acetic acid. The same action commences wherever there is lead in the presence of tannic acid; and we all know that the barks of

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* Lead-covered cables were placed in the sewers of Paris in 1862, and some of them are still in use. Those that failed, failed in insulation, and not in the lead.—W. H. P.

Mr. Preece trees are full of tannic acid, and it is from bark that we get all our tannin. Wherever lead passes through such decaying matter as the bark of trees or decaying leaves, then we get decay.

It is a very easy thing to protect the lead from the action of tannic or other acids, and I am prepared to stake my professional reputation that if a lead-covered wire, properly protected, is laid in iron pipes, it will last my life, and, I hope, the life of the Institution of Electrical Engineers.

There is just one other point connected with lead; it is a decided slip on Professor Forbes's part, which has been alluded to by Mr. Siemens. Professor Forbes tells us that there must be a galvanic action between lead and iron, and therefore it is very bad for the lead. It is not bad for the lead; it is bad for the iron, because iron is electro-positive to lead; and when the two things are brought together that which goes is the iron, and not the lead. I will not occupy your time any longer this evening, but, if I may be allowed, I may take up one or two points on the next occasion when we meet.

Professor Ayrton. The CHAIRMAN: It is almost impossible for Professor Forbes to reply this evening, and, I understand, moreover that there are several gentlemen who would like to speak. I therefore adjourn the discussion until Thursday, March 21st.

A ballot for new members took place, at which the following were elected:—

*Associates :*

| | |
|---|---|
| Sidney John Cluer. | William Patrick Henderson. |

Ernest Bonnell Hudson.

*Students :*

| | |
|---|---|
| Arthur E. Childs. | Wilfrid Bartholomew Lloyd. |
| Duncan W. Johnston. | Hubert H. Nalder. |

The meeting then adjourned.

The One Hundred and Eighty-ninth Ordinary General Meeting of the Institution was held at the Institution of Civil Engineers, 25, Great George Street, Westminster, on Thursday, March 21st, 1889—Professor W. E. AYRTON, F.R.S., Vice-President, in the Chair.

The minutes of the Ordinary General Meeting held on March 14th were read and approved.

The names of new candidates for admission into the Institution were announced and ordered to be suspended.

Donations to the Library since the last meeting were announced (three books) as having been received from the publishers, Messrs. Alabaster, Gatehouse, & Co., to whom a hearty vote of thanks was duly accorded.

The CHAIRMAN: We will now resume the discussion on Professor Forbes's paper, which advanced a certain length last time. We left Mr. Preece, we may say, speaking to us when the discussion was adjourned; and I will ask him, therefore, to complete his remarks now. Professor Ayrton.

Mr. W. H. PREECE: There is a very important point on which Professor George Forbes and I are at variance, and I think, as it is so important, it is just as well that you should have all the reasons *pro* and *con* brought clearly before you. The question is, What is the most advantageous frequency to give to an alternating-current system? and by the term frequency I mean the total number of complete periods that take place per second. A considerable amount of confusion arises because some persons speak of the number of alternations per minute, others of the alternations per second. When we speak of the alternations per minute, we mean the number of single reversals per minute; when we speak of the term frequency, we mean the complete period of the positive and negative current—the number of complete periods that pass per second. Mr. Preece.

Mr. Preece. It is perfectly clear that it is most desirable that this question shall be thoroughly and clearly thrashed out, for we have this strange variation in practice, that in America Mr. Westinghouse adopts a frequency of 133 per second—that is, 16,000 alternations per minute; in England, at Deptford, Mr. Ferranti is going to adopt a frequency of 67; while Professor Forbes has shown us that at Rome Mr. Zipernowski adopts a frequency of 42. Well, when we have such a strange difference of practice, it is clear that something requires to be threshed out to make matters straight.

The first fact that I should like to call your attention to is this, that those who have had the longest experience in working alternating-current systems have reduced their frequency; for, starting at the Grosvenor Gallery with a frequency of 150, Mr. Ferranti has come down to 67; Mr. Zipernowski, I think, commenced with 200—he has now come down to 42; but Mr. Westinghouse in America commenced with 133, and like a good old conservative, he sticks to it. There are reasons, mechanical and electrical, *pro* and *con* in this question. The mechanical reason is a very important one, and it is that which controls and guides the strength of materials. Every engineer knows that there is a certain peripheral velocity beyond which you cannot step, for if you do, you endanger the strength of your structure. In England we adopt, as a fair practical maximum, a velocity of about 6,000 feet per minute. Well, we hear of strange departures in rates of revolution. In America, Professor Forbes tells us that they adopt a rate of revolution of 1,000 per minute; in England the Willans and Robinson—the favourite engine of the day—has been brought down to from 350 to 380; while in Berlin, Professor Forbes told us, they have come down to as low as 80. Here, of course, mechanical questions control the day, and it is quite clear that with the same diameter the lower the number of revolutions the safer is your machinery; but, however, I do not rely upon the mechanical question, and will not consider it further. I will take the dynamo. Now in the dynamo we have one great disturbing element, a necessary evil, and that necessary evil is self-induction. Professor Forbes

has pointed out how necessary it is that we should be able to work these dynamos in parallel, and to work them in parallel the existence of self-induction is necessary. We know that the presence of this self-induction in alternating-current machines chokes or throttles the output of the dynamo, and the higher the frequency with the same E.M.F., the more does this choking exist, and the less is the output of the dynamo; hence in a dynamo the fewer the frequency the greater the efficiency of your apparatus. Mr. Preece.

Let us next take the conductor. When Sir William Thomson delivered his Presidential Address here, he showed how it was that when you use copper conductors for the distribution by your alternating-current machines, the presence of "time" restricted the entry, as it were, of the current into the body of the wire; it did not soak into the whole substance, and the result was that it was only a kind of skin-deep conductor, and that it really was the outside only that acted efficiently; and he showed, moreover, that that was dependent on the frequency. I have been in communication with Sir William Thomson on this matter, and I think his answer to my inquiry is so clear that I cannot do better than read it. He says: "The smaller the number of periods per second, con-"sistent with thoroughly good action of the transformer, the "better. I took 80 per second for my example, because that "is what Ferranti uses in the Grosvenor Gallery installation, "and I know he gets good action with his transformers there. "If good action can be got with less than 80, I should certainly "prefer less than 80, because not only in the generator, but "in all the conductors, both of primary and secondary circuits, "it is more difficult to get good economy of the copper the "greater the number of alternations per second; and you will "see from the appendix to my Address, that even with 80 it "is difficult enough to get good economy of copper in the "conductors for anything more than 100 amperes." So you see Sir William Thomson is very decided on the question of low frequency. Let us take the transformer—and here is the only point that Professor Forbes makes in his paper. He shows

Mr. Preece that practically the higher the frequency, the less the efficiency of the machine, and therefore that the higher the frequency, the smaller you can make your converter; that is Professor Forbes's argument. There is no doubt that that is perfectly true—the lower the frequency the more the loss of efficiency in your converter. But what is the amount of this loss, and how can it be remedied? The answer is given by Professor George Forbes himself. In a note that he gave to our Society last year, he pointed out that the frequency may be diminished without loss of efficiency if the resistance of the magnetic circuit be diminished, and if the mass of iron be increased; so that if we reduce our frequency from 133 to 100, we simply have to add a little more iron in order to compensate for the loss of efficiency, and the quantity of iron that you have to use is very easily calculated. In fact, I made some very careful experiments on this very point with a Lowrie-Hall converter, and it came out that there was a loss of efficiency of 1·2 per cent. for every thousand reduction in alternations per minute—*i.e.*, if you came down from 16,000 to 15,000, there was a diminution of 1·2 per cent., and if you came down from 16,000 to 12,000, it was virtually a diminution of 5 per cent.; so that you can compensate for that by increasing the weight of your iron somewhat in the same ratio. But while the bad effect of lowering the frequency has this one disadvantage, there is another great disadvantage in another direction, and that is when you increase your frequency you bring into action the operation of that mysterious property of magnetised iron that is called hysteresis, and the result of this is that as you increase your frequency you increase the temperature of the coils of your transformer; and it was this very question of temperature that was the practical reason that led Ferranti and others to bring down the frequency from the high figure that he originally used to the much lower figure that we have now; so that, gentlemen, we come to this conclusion, that you have all the objections that I have enumerated on the one side to high frequency, and you have on the other side only one reason, and that is that you have to increase the size of your converter to secure the same

efficiency. There is a second reason: it is that the high frequency is used by Mr. Westinghouse in America. I do not think the last reason is a good one, and I know my American friends so well that I am sure, when they weigh carefully the reasons that I have given, and other reasons that remain behind, that Mr Westinghouse himself is of that temperament and of that energy that the moment he becomes convinced, as he will, that 133 is wrong, he will come down to 80 or 100, and wipe out all those converters that are now in existence. That is one thing where the Americans beat us: the moment they are convinced that a thing is wrong, they do not hesitate whatever at chucking it away, and replacing by the better material. Mr. Preece.

Another point I want to put before you is, that I want to correct Professor Forbes on two historical facts. The one is that, in referring to alternating-current dynamos, he hints, not vaguely, but rather clearly, that the Elwell-Parker dynamo, to which reference has been made, and which is used a good deal now in London, is a mere copy of those that are described in his paper, and that are represented by diagrams on the wall. I do not think that Professor Forbes would have hinted this if he had read, as I have read, the patent of Elwell-Parker of 1882; nor would he have said it if he had seen, as I have seen, dynamos made in 1883, photographs of which I have here.

The other historical point is, that Professor Forbes has referred to the fact that Mr Westinghouse in America introduced a compound indicating device, which he says laid the principle of these indicators—that is, having one winding connected, like an ordinary voltmeter, with the terminals of the dynamo, and another winding in the reverse direction to carry the main current. He quite forgets to point out that this compound device which has been used by Mr. Westinghouse in America is the invention and the patent of Dr. John Hopkinson. There is another point in connection with Dr. John Hopkinson that has not been taken sufficient notice of. It is, that not only was he a simultaneous inventor of the three-wire system with Edison, but it is to Dr. John Hopkinson alone that we are able to work alternating-current machines in parallel. It is quite true that Mr. Wilde many years

Mr. Preece. before had referred to the matter, but it was in a lecture in this room, delivered to the Institution of Civil Engineers, that Dr. John Hopkinson for the first time brought to our notice the fact that it was possible to work alternating-current machines in parallel, and Professor Adams was the one who proved practically, down at the North Foreland, that such was the fact.

Mr. Stuart Russell. MR. STUART RUSSELL: There are one or two points on the question of underground cables upon which I should like to say a few words.

Several systems have been mentioned in the paper and in the discussion, and I would speak first of the lead-covered cables. It has always appeared to me that there must be a very great danger in the use of these cables, from the fact that the occurrence of small pinholes or cracks in the lead may escape detection at the time of testing, because, unless the cable is immersed for a very long time indeed in water, the moisture has not time to take effect on the fibrous material inside the lead. We have heard from America that one of the great complaints about these lead-covered cables has been that there is a continual and increasing leakage taking place, starting a very short time after the cable has been laid. This I have seen stated in many reports of discussions which have taken place at the meetings of the American Societies, when the question of underground cables has been discussed; and it appears to me that the readiest explanation of this is the fact that the lead is not perfect at the time of manufacture, and that these imperfections escape detection at the time of testing. There is also, I think, some danger owing to the comparative want of flexibility of a lead-covered cable—*i.e.*, that especially when dealing with heavy cables, cables with heavy conductors, the coiling and uncoiling and the handling necessary in laying the cable are apt, perhaps, to start small faults in the lead. These small faults in the same way may take some time to affect the insulation, but I believe myself that it is merely a question of time, and that sooner or later, if there is the slightest pinhole in the lead, moisture will creep in and the insulation will fall. It would be interesting to know if in Berlin any such effect has been noticed as has been

found in America—that is, a gradual falling of the insulation of these lead-covered cables. On the question of the perishing of lead we have really very little, or, at any rate, I have very little, experience at all; it seems to me to depend almost entirely on the soil in which it is laid. Undoubtedly, in America, in New York especially, where they seem to meet with very great difficulties, and where newly laid cables have frequently come to grief on that account, the acids or gases which are in the soil have attacked the lead. But this question is one which must be made a special study in each particular case; and the nature of the soil must certainly be taken into account in arranging the method of laying the cable. Mr. Stuart Russell.

An account was given us by Mr. Alexander Siemens of the actual number of failures in the Berlin cables to which Professor Forbes had referred, where eight cables had failed out of something like 35: that seems to me to be a fairly large percentage. Six of those failures were unaccounted for, and I will suggest that possibly, I may almost say probably, they were caused by this absorption of moisture coming in through the minutest fault in the lead.

Mr. Alexander Siemens: May I just correct a wrong impression? You must not put it that eight cables went wrong out of 35. There were four places at which there were 35 cables passing, and it was at those four places where the faults occurred. The other figure that I gave ought to be taken—that out of 144,000 metres 250 metres had to be replaced. Mr. Siemens

Mr. Stuart Russell: I beg your pardon. I understood you to say eight cables failed out of 35; of course that undoubtedly alters the percentage of failures very largely. I was going to refer to the failures which were attributed to mechanical injury, and that, I think, shows that the lead covering and iron sheathing is not sufficient to render such a cable really safe underground, and seems to me to be a very strong argument in favour of laying all the cables in conduits or in iron pipes. Mr. Stuart Russell.

In his paper Professor Forbes stated that he was not aware of any cable which had lasted more than three or four years.

Professor Forbes: Lead-covered.

Mr. Stuart Russell

Mr. Stuart Russell: Lead-covered; and at present, in this discussion, no gentleman has given any instance of this. Mr. Lant Carpenter said he knew of many cables which had been very successful; but I think it is a matter of regret that, for fear of taking up the time of the meeting, he should have abstained from naming the places. These cases would, I think, have been of the greatest value; because, were we informed as to the conditions under which these lead-covered cables had been successful, by comparing them with the conditions under which they have been known to fail, we should have got some very valuable information, and possibly have got a very good line to work on as to with what voltages and under what conditions it would have been safe to use them, and when not. Mr. Lant Carpenter also referred to the question of putting vulcanised rubber cables under lead, and there I think his fears were unfounded. He imagined that the free sulphur in the rubber would affect the lead. First of all, the insulation of the cable does not depend upon the lead covering, and many vulcanised cables have been laid without lead covering, and have been in constant use for periods up to three and four years. Secondly, the amount of free sulphur in rubber is very small indeed if it has been properly vulcanised, and, owing to the interposition of one or more compounded tapes between the rubber and the lead, the effect of the sulphur on the lead would be very small, as the compound would really absorb the sulphur and prevent it getting at the lead. I fancy, from experience with rubber-insulated cables covered with lead, that there is really nothing to be feared on this point, and, at any rate, that the destruction of the lead on the inner surface is very much less than that which takes place on the outer surface, even in the ordinary soil. Speaking of india-rubber cables reminds me that Mr. A. Siemens said that one of the disadvantages of rubber as an insulator was its variability. This appears to me to be really a question of manufacture, and to depend on the knowledge of the qualities of rubber that the manufacturer has at his disposal. Of course there are many different grades of raw rubber, and many different ways of treating them, and it is only natural to expect that differences in the manufactured article

will occur, just as they occur in other manufactured articles—dynamos, for instance, where the efficiencies of various makers are not all the same. But, however, I am informed by those who have made a special study of the particular question, that by using one of the four or five different kinds of good rubber, and by treating it exactly in the same way, there is no practical difficulty whatever in reproducing rubbers of the same quality, and therefore this disadvantage which is attributed to rubber is really not an important one at all. Mr. Stuart Russell.

I will now pass to another class of main altogether—that advocated by Mr. Crompton—and here I think the one point to look at is, that although air insulation is a very excellent thing in itself, there is always the danger of its perhaps at some time becoming water insulation. Overhead telegraph lines, I believe, are not always perfect, and I think that in many respects they are in quite as good a condition as a bare wire in a trench. We have heard great outcries about the breakdown of overhead lines through a snowstorm; but suppose we imagine a case, which is really not less probable than that of the snowstorm, viz., that of the trench getting flooded from exceptionally heavy rains or the bursting of a drain; I think that the outcry raised on such an occasion as that would be even more serious still.

We have to thank Professor Forbes for the suggestion in his paper which has brought forth from Mr. Crompton the statement of a test of these mains, but our information wants supplementing on this point. I remember, at a previous meeting in this room, that Mr. Crompton, with a frankness which really cannot be too much praised, stated that "of course he chose his time for "testing." Now the time chosen would depend upon what you really wish to get, and it would be interesting to know whether the result given is a maximum or a minimum, or whether we are to take it as the happy mean. The actual result of the test—380,000 ohms, I think Mr. Crompton said, per mile—does not seem very high, and Mr. Crompton explained that by saying that the greater part of the leakage he thought was at the joints where the ordinary covered cables were joined to his bare mains. If that be so, it seems to be a great disadvantage to the system, for

Mr. Stuart Russell in advocating it Mr. Crompton said that he was looking forward to the time when every second or third house would have a branch to it; and if the few branches which I understand were on the main tested, gave a resistance of only 380,000 ohms, when we come to get a branch every 20 or 30 yards we must expect the insulation resistance to be very much decreased indeed.

Another system has been mentioned (the Edison tube), and I have noticed with regard to it that although it has met with a very large amount of success, yet it is not, according to my mind, a perfect system. There has been considerable trouble in one or two Continental stations, so much so that the tubes have been pulled up, and I understand they are possibly to be abandoned and replaced by some other system.

I also find that in America some of the local Edison Companies have given up the use of the Edison tubes, and are using cables in place of them; and in France, in the recent installation at the Palais Royal, I am informed that india-rubber cables are being used by the local Edison Company, as also in the Opera House. I do not know whether this is a question of cost or of the efficiency of the cable, and possibly some other gentleman now present who is more acquainted with the Edison work may be able to throw some light on the subject.

I would conclude by thanking Professor Forbes for the paper that he has put before us: and although some speakers at the last meeting seemed to rather take exception to being taught by our Contintental brethren, I would suggest that, even though all their work had been a failure, which it undoubtedly has not, their experience would be of most undoubted use to us; as the next best thing to knowing exactly how to do it, is to know what to avoid.

Mr. Wright. Mr. A. Wright: Although I have come up from Brighton unprepared to speak, I cannot help expressing my disappointment at not hearing from Professor Forbes certain data and figures which would have been extremely valuable to central station engineers. The most valuable fact to be learned from the experience of foreign stations is the average cost of production of the electric light, or the average number of pounds of coal consumed per unit produced for the different systems employed; also, I looked for a

statement as to the ratio of the average current to the maximum required. These are facts that are extremely important, and about which we have not heard a word from Professor Forbes. I think, also, we might have had some information about the concentric mains, which is a system interesting a great many engineers now. I should like to know whether, in the towns where these concentric mains have been used, there has been any interference in the telephone or telegraph systems. Mr. Wright.

The question of the subdivision of plant is also a subject which I should have thought would have formed part of Professor Forbes's paper. How to divide the maximum load curve; into what units the generating power was divided—I hope that Professor Forbes, in his answers, will give us the benefit of these experiences.

If I had come prepared to speak to-night, I should have been very happy to give certain figures relating to the above problems as determined in Brighton.

The Chairman: The very information you mention in connection with Brighton will be most acceptable.

Mr. Wright: I am sorry I am not prepared to give the information from memory.

The Chairman: Surely, with your long and valuable experience, you can give us some information.

Mr. Wright: I may say that the average number of pounds of coal used throughout the year per unit on the old Brush multiple-series system was 24 lbs., while with the new alternating system, with Elwell-Parker dynamos and converters, driven from the same engines, this figure comes up to nearly 25 lbs.; showing that the Brush multiple-series system seems to be slightly more efficient than the modern alternating system with Elwell-Parker dynamos. I believe the higher coal cost of the alternating system is due to the excessive day and night magnetising currents required by the Lowrie-Hall transformers. The ratio of the maximum current to the average daily current at Brighton appears in December to be five, and the proper division of plant at Brighton would be by four equal units.



I should like to mention a fact about the cost of running

Mr. Wright. transformers empty, or of magnetising transformers in the daytime: this has, I consider, a very important bearing. It is generally supposed, from theoretical reasons, that the work in magnetising the transformer is practically nothing, the lag being nearly 90 degrees; but we have taken a long series of power measurements to find out whether the 4 or 5 amperes sent out in the daytime at Brighton meant work or not, and we find that 4 amperes with the secondaries open absorb as much power as 4 amperes and 2,000 volts running on dead resistance. I mention this because it seems a common impression that 4 or 5 amperes of magnetising current mean no work on the engine, whereas it means a great deal of work.

Mr. Wyles. Mr. F. Wyles: I have never had any difficulty in coupling machines in parallel.

Professor G. Forbes: Up to what sizes?

Mr. F. Wyles: I have had no experience with large machines.

Professor Ayrton. The Chairman: Will Mr. Eddison give us some information about lead-covered wire?

Mr. Eddison. Mr. R. W. Eddison: I did not attend this meeting with the expectation of saying anything on the subject of lead-covered cables, but I came rather in the hope of hearing from Professor Forbes the reasons why Lead-covered Cables were supposed to have failed so rapidly as he stated they had done in some instances. It would be interesting to know whether the cables alluded to had been drawn into lead pipes, or in what way the lead had been applied, and also the nature of the dielectric. I think there is no reason why the lead covering of a telegraph or electric light cable should fail any more readily than an ordinary lead water-pipe, if the lead be applied in a proper manner, the cables laid with ordinary care, and all joints properly made. My experience in the manufacture of lead-covered cables does not extend for "three years," but I shall be very glad in the course of another twelve months, to give particulars of lead-covered cables that will then have been working beyond Professor Forbes's limit. Up to the present time I have not found any of our lead-covered cables to fail either in lowering of the insulation (as mentioned by Mr.

Russell) or through deterioration of the lead covering, unless it has received external injury. Mr. Eddison.

The CHAIRMAN: Some notes have been sent, since the last meeting, by General Webber, who is unfortunately unable to be present this evening. Professor Ayrton.

The SECRETARY read the following communication from General C. E. Webber:—

"Most of the speakers in this discussion have either commenced "or finished their remarks with cordial expressions of admiration "for Professor Forbes's paper, but in the body of their remarks "have been very scant of anything but adverse comment. General Webber.

"The Professor has been twitted with oblivion, and neglect of "what engineers in this country have already accomplished; and "Mr. Preece has given us a list of companies who are suppliers "of electricity, and asks the author, have they not accom-"plished more than he seems inclined to give them credit "for.

"It is quite true that designs of systems which show the most "complete acquaintance with all the conditions which the Pro-"fessor seems to imply that English engineers are ignorant of "have been carefully elaborated by many of us.

"In my own case I was led to look up some papers connected "with Chelsea, and I find that a complete specification for a "system in the north-east part of that parish, requiring distribu-"tion by direct low-tension current on the three-wire system, was "printed for private circulation in 1884, and that my figures, &c., "were checked and approved at that time by Professor Ayrton. "The capital cost was £48,000. The chief difference between "that estimate and others made later, lies in an overestimate of the "number of householders who are likely to take the electric light "within such an area. We then estimated for every house, "whereas experience has since shown that one in three is nearer "the mark.

"Many here can point to similar labours, and can give the "well-understood reasons why so little real work followed.

"But I feel that we must *bona fide* congratulate Professor "Forbes on the way he has occupied the ground on the subject

General Webber.

"of distribution, of which, as we all know, he has made a special "study for many years.

"If it is the case that this part of electric lighting has received "little genuine development in this country, he cannot have done "us a greater service than by telling us what has been done in "that direction elsewhere.

"We have been called on, in the course of the discussion, to "admire what several enterprising companies have accomplished "in London and other towns; but I feel sure that Professor Forbes, "in his reply, will bear me out when I say that most of them, by "their *manner* of distribution, have done far more to hinder "electric lighting than to advance it.

"I do not refer to the unsightliness of overhead cables so "much as to the absolute want of system of any kind in arrang-"ing for their routes as a means of town distribution.

"I venture to put it thus: Would any one who has had "experience of this kind of construction for aerial electrical "conductors in towns, estimate the value of the lines we see here "and there in London, at more than their value as old material "after being taken down, supposing he represented a company "which is acting under the powers and regulations of a Pro-"visional Order that had been asked to take them over?

"I think not. I have never seen one, except perhaps for use "as a feeder, that would be of any assistance whatever to under-"takers who had seriously to establish a system of distributing "mains in a district. Unless they already existed, no one would "have anything to do with them even as feeders.

"Considering the absolute insecurity of tenure for the poles "and supports, and of way-leave for the conductors passing over "private property, I do not think that any congratulation can be "offered to those who have been beguiled to countenance their "money being spent in that way.

"I should be glad to know what arrangements have guided "those responsible for designing these systems other than hazard "and the caprice of the people who are applied to for way-leave. "Have they any feeding mains? Are there any points where the "electrical pressure is equally maintained? Have they any pilot

" wires? What precautions have been taken to secure the safety " of the public? Have they lightning protectors? Can part of a " system be isolated in case of a fire? Are the mains well out of " reach in passing other structures? General Webber.

"No wonder Professor Forbes indirectly criticises the work in " this country, by describing what is done in other parts of " Europe, when, *with few exceptions*, our distribution systems " bear little evidence of good engineering or scientific knowledge.

"At one time of my life no one was a stronger advocate of " that best of all insulators, namely, the air, in connection with " telegraph and telephone lines, and no one more believed that it " required better engineering skill to put up a good overhead line " than to lay one down underground.

"But the points to be reached by the wires for those purposes " are isolated and scattered. On the other hand, the houses to be " supplied with current for electric lighting, under the compulsory " provisions of a Provisional Order or license, are almost next door " to one another and in straight lines. Hence, while overhead " lines are suitable for the one service, they are eminently ill-" adapted to the other.

"We are told that we ought to be grateful to those who have " carried their conductors overhead from their stations in all direc-" tions to isolated wealthy customers.

"I challenge any one to prove that the industry has been in " any way benefited or promoted by the unbusiness-like proce-" dure I have described, or that the example of those wealthy " customers will promote the more general use of the light. On " the contrary, their indifference to price has, if anything, tended " to discourage their less wealthy neighbours, and to give the " impression that it is a costly luxury instead of a welcome " necessity.

" Professor Forbes has told us what the *concessionaires* under " Continental municipalities have been obliged to do. Any one " who has seen (as I have) many of the regulations under which " the right to light some of the Continental cities is conceded, " will recognise the wisdom of those charged with the preserva-" tion of the public safety and interests, and can only regret that

General Webber. "the extraordinary state of our municipal law did not long ago "give our local authorities similar powers.

"My contentions are very much supported by the fact that "some of those whom I take the liberty to style 'offenders,' "instead of benefactors, are now seeking to place themselves "under Board of Trade control, when, if I am not mistaken, they "will have to retrace many a weary step.

"C. E. WEBBER.

"*19th March*, 1889."

Mr. Bate. Mr. D. C. BATE: May I ask Professor Forbes what he alludes to in his paper when he says that at Berlin "the system of "feeders and mains has been adopted to produce as small a "variation of pressure in the mains as possible. This is accom-"plished by having a large number of converters."

Professor G. FORBES: I think that is feeders: it is a misprint.

Mr. D. C. BATE: As I thought: it was a direct-working system?

Professor G. FORBES: Yes.

Mr. D. C. BATE: I would just add a word or two to the discussion on the question of underground mains. It appears that beyond doubt some mains failed at Berlin, whosesoever they were, and that, as has been usually the custom, those mains were expensively insulated; probably when they were first put down, they tested to a high number of megohms of insulation resistance, and yet in something less than three years there were 200 and odd metres which had to be cut out.

Mr. Siemens. Mr. ALEXANDER SIEMENS: Please do not forget that it was not the cables that were laid in 1885 which failed, but those which were laid last year and the year before. Professor Forbes was uninformed on that point: it was not the old but the new cables that failed, and the cause was undoubtedly traced to mechanical starting of the faults.

Mr. Bate. Mr. D. C. BATE: I quite understood Mr. Siemens to have made it perfectly clear to us that it was not the old cables, but it was some cables that failed.

Mr. ALEXANDER SIEMENS: Yes; mechanically.

Mr. D. C. BATE: If it was the new ones that had been

worked for a short time only, I think that that only strengthens my argument that cables failed within a very short time, by mechanical injury in some cases; but in some cases—six, I think a previous speaker has said—they failed from some unexplained reason, which might be electrical or might not; at any rate, they did fail. The point I wish to bring out is this—that the ultimate court of appeal which will decide whether electric lighting is to be a success or whether it is not, is to be the pocket of the shareholder, and the dividend which the central station will pay. Now, Sir, if you are to put down mains in which your copper is but a small percentage of the total cost, and those mains are to fail, as these and other mains appear to have done, pretty quickly, you have only the copper as an asset to sell for old copper; the insulation is worth nothing after it has been put on to the cable, however much it cost to put it there, and however much it cost as insulating material. I therefore think that it is quite possible to overdo the cost of underground mains. I fancy that many well-known makers would make cables with an insulation of 100 megohms a mile in which the copper would cost about 50 per cent. of the total outlay. These, we will suppose, last three years; they are not going to fail all along the line,—they are not to come full of pinholes like a colander,—but they will fail in specific points, as, to quote Berlin again, where only 200 odd metres out of a very large number failed, and therefore it will be perfectly easy to replace them at a comparatively small outlay. If, however, you have to replace heavily insulated mains, even supposing they last double the time, they will cost treble the amount or more, and the interest on capital that will be expended will be so great that I venture to think it will probably overbalance the cost of repairs on a less insulated cable. One hundred megohms per mile seems to be a pretty good insulation if it can be kept, and I venture to say that probably ten would be sufficient for safety and good working. Suppose you put down a cable with 500 megohms a mile, is it going to last five times as long as one of 100 megohms would last? and if it does not, where is the advantage of putting so much money Mr. Bate.

Mr. Bate. underground to have to pay interest on capital, when a very small amount of repairs on a less insulated cable would probably keep it just as long as the better insulated one? I think I have heard that one central station in England uses a main which costs something like £180 a mile, whereas a cable of 100 megohms would only cost somewhere about £50 a mile. I am speaking of $\frac{7}{16}$ wire.

Passing from cables, I would remark, as regards the switching-in of large dynamos, that I have a pretty good memory that at the Edison station on the Holborn Viaduct, with Dr. Hopkinson's differentially wound voltmeter, we were enabled to put the two "Jumbo" machines, alternately or together, into the mains without having to get either of them up to full load before doing so. One high-resistance wire was put across the mains of the working dynamo, the second one was then started with its poles in connection with the other winding; as soon as the potential of the second dynamo became as great as the first, it switched itself in within a volt or two of the right potential, and either speeded up or slowed down as might be.

Mr. Mordey Mr. W. M. MORDEY: As to working large continuous-current machines in parallel, I may point out that it is rather easier to work such machines parallel, if they have a considerable drop in the characteristic. Most modern machines change from a generator to a motor within a very short range of speed, but machines such as the "Jumbo" Edison machines had, I think, a considerable drop, and a good many machines at present are made, perhaps purposely, with a considerable drop; and such machines, whether shunt or compound, are more easily put in parallel than machines that have a nearly straight characteristic. In his paper, page 172, Professor Forbes alludes to two 150 H.P. alternators that are self-exciting, and to two larger ones of 600 H.P. each which are independently excited. That is rather instructive. It appears to be the practice of those who have had most experience in alternators to use independent exciters for large machines; the old practice which originated with Wilde, or before him, was to commute the alternating current and use it for the fields. Here we have Zipernowski using large

machines of 150 H.P. self-excited. I have not had any experience with self-exciting alternators, and should like to know if Professor Forbes has any information as to the relative efficiency of excitation, and of the relative efficiency of the whole machine with self-excitation, and with independent excitation. With self-excitation there must be a good deal of inductional loss, and the difficulty of getting a good commutator collection is perhaps considerable. Mr. Mordey.

The Edison meter was alluded to by Professor Forbes as being largely used at Milan, and he also spoke very highly of it in his recent paper at the Society of Arts. I refer to the Edison electrolytic meter, and I wish to ask for some information about it, which I dare say a good many here would also be glad to have, as to how such meters are put into circuit. For constant-current work, where you simply want to put such a meter, for instance, across a group of incandescent lamps or across an arc lamp, and to get a constant effect as long as it is on, the problem is easy enough; but Edison in America, and the Edison people in Milan, use the electrolytic meter exclusively for parallel, where the conditions are not so simple. The difficulty that I have found, in a few experiments I have made, has been that unless a perceptible resistance is put into the circuit you do not get any deposit at all with very light loads. If you put such a resistance into the circuit as to give a sufficient deposit with light loads, then the fall of potential due to that resistance when the load becomes heavy is quite serious. I believe that that may have been overcome to a certain extent by using zinc and a pure zinc-sulphate solution, but I found that difficulty with ordinary copper sulphate; and I should like to know what the resistance is across which the cell is placed, what the electrolyte is, what the metals are, what is the percentage loss with the greatest current that is taken through those meters, and what is the greatest proportional range.

I fully endorse all that Professor Forbes has said about the beautiful effect produced in Milan by the arc lighting in the streets: it certainly is magnificent. The photographs on the wall show the square in front of the Cathedral. That is an ordinary

Mr. Mordey. case of lighting a square such as may be seen anywhere, with the lamps on posts; but the lighting of the streets is different. There are about seven main streets branching out from the Cathedral to the gates of the city, and the effect of the lighting in these streets is very fine. But we ought to remember that the working conditions in Milan are very different to the conditions existing here. In Milan they have the ordinary Continental arrangement of high houses, generally white or light coloured, and the lamps are hung in the middle of the road by steel cables passing across from one house to the other. Now, with our grandmotherly system of government, and with all the authorities and interests that we have to consult and consider, there would be considerable opposition if anybody spoke of putting a cable across the street to hang an arc lamp on; yet it is far the best system; it is not at all unsightly. The steel wire is a very small thing; it can be tightened up by a screw eyelet let into the wall, and the whole thing is as convenient as it can be made. The wires are run along the front of the houses under the windows, and are from there taken out to the lamps. The lighting of Milan is quite an example of what street electric lighting should be, but I am quite sure that it would not be permitted in an English town—not for any sound reason, but simply because we are behind in these matters.

I should also like to get some information as to an English installation about which much has been said—that at West Brompton. I find that they are putting lead-covered cables down in iron pipes, and that these lead-covered cables are put down singly. This is a thing that probably a good many others here have noticed: I know that Mr. Fricker has noticed it, and probably others. Instead of insulating the two wires and putting them into one covering, I am told that each conductor is put in a separate lead covering. In the discussion on Mr. Kapp's paper at the Institution of Civil Engineers the other day, one of the engineers of the West Brompton station showed some very pretty and ingenious arrangements for making a watertight and well-insulated cover joint at the T connections to the lead-covered cables. That arrangement showed that the wires were to be used

singly. Now, is it not a canon of underground work, where alternating currents are used, that the cables should be either concentric, or that they should both, if enclosed at all, be enclosed in the same metallic envelope, otherwise the maximum waste is incurred? I wish to ask if Professor Forbes has noticed this, and if he can support the practice that is to be carried out at West Brompton. If he cannot, I think it is best for all concerned that attention should be drawn to the mistake that is perhaps being made at West Brompton. Mr. Mordey.

Mr. W. H. PREECE: I should just like to say one word in reply to Mr. Mordey, to point out that the plan of suspending lamps in the centre of the streets was my proposition to the City authorities, and some five or six years ago I experimented on those lines at Wimbledon. The main thoroughfare at Wimbledon was lighted with lamps suspended over the centre of the streets, and there is no doubt it is the most efficient and the most effective way of lighting a town; but the practical, serious, and almost insurmountable objection is, that the wires and supports must be carried at a sufficient height to allow the fire escapes to pass through without any danger, and were it not for the existence of fire escapes we certainly should have our streets lighted in the way that they are lighted in Milan. Mr. Preece.

The CHAIRMAN: I am afraid it would be very difficult to go round a sharp corner with a fire escape in a horizontal position: it would probably be awkward for persons on the other side of the corner; however, it might be taken at a better angle than at present, and I like Mr. Mordey's idea of a front wheel, but I would not advocate his suggestion that the position of the escape in motion should be perfectly horizontal, otherwise perhaps, like Stevenson, who proposed to have trains going as fast as 10 miles an hour, he may thwart his own plans by the daring character of his proposition. Professor Ayrton.

As no one else appears to wish to make any remarks, I will, without detaining you very long, make a few suggestions in closing the discussion. In his paper Professor Forbes commenced by apologising for going abroad, seeing what is being done there, coming back and telling us. Why he should think it necessary

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to make an apology I really cannot understand. If he is willing to go abroad, to learn all he can, to come back here and immediately give us full information, I think he is to be extremely thanked by us, and so far from apology being necessary from him, our gratitude is due to him for his labours and for the information he brings us. He has been taken to task because he has not visited some of our London central stations; but we do not blame a Stanley or a Livingstone because they are not perfectly familiar with the sub-tropical garden in Battersea Park; so that, even if Professor Forbes has not seen some of the installations that exist in London, he does not merit blame at our hands on that account, especially as we possess the very simple remedy of going and seeing these for ourselves. He has, no doubt, as Mr. Wright has pointed out, not given us perhaps as much information as we should have liked to have had, but probably he has given us all the information he could obtain, as to the cost of working these various installations; but one thing he has given us which is of interest, and that is the cost at which the electric power is supplied to consumers. I have made a comparison to see whether it was cheaper or dearer than gas—*i.e.*, what was about the proportion in cost of electric light to gas in places where the electric current was generated on a far bigger scale than it has hitherto been done in, say, Great Britain—and I find that the proportion seems to be somewhat the same. In Berlin the price of the electric power is equivalent to about 8s. 3d. per 1,000 cubic feet of gas—that is, nearly twice the actual price of gas in Berlin, which is 4s. 10d. In Milan the price of electric power is equivalent to 8s. 8d. per 1,000 cubic feet of gas—gas there was originally a little more than the cost of the electric light, but it has been reduced to 6s. 9d. In England, with our largest proposed distribution—the Deptford installation—the price will be equivalent to gas at 6s. 3d. per 1,000 cubic feet. In these and other cases that I have been able to examine, it seems that the price of electric power is very nearly 100 per cent. more than the price of gas at the particular place. Whether that is because the consumers of electric power appear willing to pay about twice as much as for gas, and therefore the suppliers of

electricity have not unnaturally fixed that as the price, or whether that is the lowest price that suppliers of electric power can furnish the electric light for, in order to enable them to pay a fair dividend, I do not know. But it is really an interesting point to ascertain what is the lowest price that it is possible to supply electricity at on a large scale in a given place, compared with the price of supplying gas at that place, both being produced by the consumption of coal. Is it possible to distribute electric power on a very large scale cheaper than gas is sold at in the houses? Professor Ayrton

Reference was made more than once by Professor Forbes as to the non-adoption of Sir William Thomson's law, which he quoted as being so many amperes per square inch. As a matter of fact, Sir William Thomson's law should not be stated as so many amperes per square inch, but as so many inches per ampere—a distinction that might appear at first sight trivial, but which is all-important, seeing that the problem Sir William Thomson solved was:—Given a certain current, to find out what was the proper size of conductor to use with that current. That is the only way in which his law can really be applied. My colleague and myself have already pointed out that Sir William Thomson did not rightly interpret his own law in his Inaugural Address at York. The law that he worked out may be thus stated: If $A$ be the current in amperes that we have decided to use with a particular conductor of resistance $r$ ohms per mile, then the watts expended per mile are

$$A^2 r + \frac{t^2}{r},$$

$A^2 r$ being the watts expended in heating the conductor, and $\frac{t^2}{r}$ in the form of interest on money sunk in the copper conductor. $t$ is equal to

$$\sqrt{67 \cdot 84 \frac{i \times c \times \rho}{a}},$$

where $i$ is the rate per cent. of interest on money, $c$ the cost in pounds sterling of a ton of copper, $\rho$ the resistance of a mile of copper one square inch in section, $a$ the yearly value in pounds sterling of one electric horse-power for the number of hours the

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power is used. If, for example, we take $i$ as equal to 12, to allow for depreciation, $c$ as £125, $\rho$ as 0·04378 ohm, which is the resistance at 20° C., if the conductor has 98 per cent. of the conductivity of pure copper, and $a$ as equal to £15, then $t^2$ equals 297·3.

Now, if $A$ be fixed, then the minimum value of the above expression is obtained by making

$$r = \frac{t}{A},$$

that is, by making the two wastes equal to one another. This, for the particular value of $t$ that we have calculated, leads to $\frac{1}{393}$ square inch of copper per ampere, or a trifle over one-fourth square inch per 100 amperes.

Now this solution can only be used on the assumption, as we have already pointed out, that $r$ is the variable and $A$ is constant, and it cannot be used at all, and has no practical meaning whatever, if $r$ is fixed and $A$ is variable. If there be a given line laid underground or overhead, of a certain resistance per mile, and you wish to apply Sir William Thomson's law to find out what is the most economical current to employ, his law has no application whatever, for the simple reason that if you take the expression

$$A^2 r + \frac{t^2}{r}$$

and try to find out what is the value of $A$, that makes the expression a minimum, it is $A$ equal to nought, that is, do not use any current at all; that answer gives you the minimum waste, but not the conditions of maximum economy in the commercial sense of the word. Therefore, the conclusion which Sir William Thomson came to, viz., that the two wastes (the waste due to heat and the waste due to interest on capital sunk in conductors) should be made equal, is only applicable in the particular case that the current is fixed and the resistance is variable. It is not applicable in cases where, as people so frequently are inclined to apply it, the resistance is fixed on account of the line being already laid, and the current is variable; in fact, as we have shown, under these circumstances a different law must be applied,

for you do *not* then get maximum economy by making the two wastes equal; you can get greater economy, in fact, by making the two wastes unequal. What the exact solution is depends upon what is given. Suppose we have to deal with a line already made, then the length of the line and its resistance per mile are known. Let $V$, the P.D. at the dynamo end of the line, be also fixed, then the value of $A$, the current which gives maximum economy, is

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$$A = \frac{t}{r} \frac{\sqrt{V^2 + n^2 t^2} - nt}{V},$$

where $n$ is the *total* length of the conductor in miles.

If neither $A$ nor $r$ be fixed, but $V$ be given, and if, in addition, the power, $p$ watts, that has to be furnished at the distant end of the line be also known, then, as we have already shown,

$$A = \frac{p}{V}\left(1 + \frac{nt}{\sqrt{V^2 + n^2 t^2}}\right)$$

gives maximum economy.

In both these cases, then, *greater economy is obtained by making the interest on capital sunk in conductors larger than the annual coal bill* than by making these two sums equal.

If, on the other hand, the P.D. at the distant end be fixed, and not $V$, the P.D. at the dynamo end of the line, and if $A$ and $r$ be either, or both, variable, then

$$A\,r = t$$

gives maximum economy. Of course we have to thank Sir William Thomson for having initiated the kind of solution, for having suggested the method of solving such problems, and for having drawn attention to the fact that there were two things to be taken into account, viz., the waste of power in heat, and the waste in interest on money. I only, therefore, in alluding to the rule, wish to remind you that you cannot apply his solution except in the one case, that is, when the current is the given thing, and the resistance of the line is the variable.

I thoroughly agree with Professor Forbes's remark about insulation: "We do not require a high insulation for the sake "of getting a small loss of current, but only because with many "insulators the high insulation is more permanent, and perma-

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"nency is the quality of insulation which is most to be desired." That remark is especially pertinent in reference to the paper we recently had read to us as to the insulation resistance proper for electric light circuits, and where it was proposed to take a fixed resistance per mile, irrespective of whether the leakage was due to leakage in the cable, or leakage in the fittings. Several speakers then pointed out what a wrong method that must be, because an insulation resistance which might be fairly good, if the leakage was partly in fittings and partly in the cable, would be an extremely bad resistance if all the leakage was in the cable. Only a few days ago I had an example brought before me, since the last meeting, on this very point. A line some miles long, having a number of outdoor arc lamps on circuit, had a resistance which varied from about, for the whole line, 1,000,000 to about 3,000, so that it would be several, perhaps four or five, millions per mile on a very dry day, and only a few thousands per mile on a very wet day. Now, of course, if the resistance went down to 3,000 or 4,000 ohms in the cable, the cable would be condemned and cut out; but as the tests showed that the fluctuations from day to day were due to the leakage in the fittings of the street arc lamps, the installation was not condemned in spite of its occasional low insulation.

Reference was made by Mr. Preece to the question of speed of alternation, and he pointed out that at high speed the output of a dynamo in consequence of self-induction is seriously diminished, and therefore a low speed may be better. I presume his remarks apply to a dynamo in which there is iron in the armature. I cannot imagine that if you take a dynamo like the Ferranti or Mr. Mordey's, in which there is no iron in the armature, that the output would be increased by lowering the speed: the increase of electro-motive force obtained by increasing the speed would far more than make up for any loss due to self-induction, and therefore you would certainly have the greatest output at the greatest possible speed you could run the machine at. Indeed, I can hardly imagine his conclusion can be true for any alternate-current dynamo, even if there be iron in the armature.

Also, Mr. Preece referred to the loss of efficiency in different

transformers, and stated how the efficiency varied when more or less iron was put in. He was dealing with very small differences of percentage—1½ or 1·1 per cent. I should like if he could tell us how the efficiency was measured. It is very easy to make a mistake of several per cent. in measuring the efficiency of a transformer, in consequence of the fact that you cannot apply a voltmeter, ammeter, or such meters, and say that the product of their readings gives the watts supplied. I may mention, in passing, in connection with the use of wattmeters for alternate-current circuits, that some of our students, on making the calculation, have found that the coefficient of self-induction in a non-inductive resistance box of the Zipernowski wattmeter specially intended for alternate-current circuits, is quite comparable with the coefficient of the self-induction of the suspended coil, and that people who have been making calculations have been wrong in merely assuming that the coefficient of self-induction of the fine-wire circuit of a wattmeter was that of the suspended coil only. Indeed, for delicate measurements of self-induction, it becomes necessary not to use ordinary double-wound resistance coils at all, for even those made by the best makers have a very considerable amount of self-induction. In our Wheatstone bridge at the Central Institution, we have to take into account the self-induction of the various double-wound coils, and you cannot by any means assume that this is nought. The best way to make the self-induction of resistance coils really nought would be by taking a platinoid wire, covering it with silk, then winding platinoid outside that again, and covering that again with silk, getting, in fact, concentric conductors of platinoid; you then wind them up in a coil or leave them straight, as you like, and you have a current going one way in a wire, and back by an insulated concentric outer tube, producing a circuit which is practically non-inductive. Professor Ayrton.

It was properly pointed out by Mr. Wright that you cannot assume that no watts are given to a transformer when the secondary circuit is open. Indeed, in the experiments made by our students that were described in this room a year or more ago, attention was drawn to that, and the number of watts given to

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the primary circuit of a transformer was shown,—the true watts, not the watts measured in some imaginary way, but the true watts given for various currents in the secondary, when a fixed square root of mean square of volts was maintained at the terminals of the primary,—and it appeared that about one-twentieth of the maximum watts was given to the transformer when the secondary circuit was open: that is, one-twentieth of the maximum watts that was supplied on full load was entirely wasted in heating the transformer when all the lamps were turned off. It must indeed be a familiar experience to most of you that transformers during the day get very hot, and that therefore you cannot assume that there is no power wasted. I do not mean to say that the transformer system is a bad system, but I do mean to say that we must not neglect the waste that occurs in transformers with the secondary circuit open.

I am a little surprised to hear from Mr. Wright that they find that it actually takes more coal to supply power with transformers than with the Brush multiple-series system. The Brush multiple-series system, worked at about 1,500 volts, has an efficiency of 50 per cent., I understand—that is to say, you can get about 390 watts developed in lamps for one horse-power developed by the engine; that, I believe, is the result of actual experience, so that the total efficiency is about 50 per cent. One would have hoped that you would get on the whole more than that with transformers, but according to Mr. Wright's experience it requires more coal to work with transformers than to use the high-potential multiple-series system.

Seeing the lengthy remarks that we are likely to have, and which I hope we shall have, from Professor Forbes, I don't think I dare detain you a moment longer. I end, therefore, by thanking him for myself, and thanking him, I hope, in the name of the Society, for going abroad and getting information, and generously placing at our disposal this information which we are not able to get for ourselves.

Professor Forbes.

Professor George Forbes, in reply, said: In the course of the discussion of the last two evenings, I must say, in the first place, that I feel that I have been a good deal misrepresented, and a

Professor Forbes.

good deal misunderstood. I have not hoisted upon a pedestal everything foreign, and cast down into a hole everything British; on the contrary, I have simply told you what is being done abroad in some limited number of cases, and I have seen there a great deal that is to be disapproved of.

With regard to the foreign element, I would remind you that last evening, among the very first speakers, there were three gentlemen of foreign birth who spoke, and I think that they, in their speeches, taught us something that several of those who followed them might lay to heart. Those three gentlemen—Mr. Siemens, Mr. Andersen, and Mr. Kapp—showed that, in discussing an engineering question, it was possible for engineers to divest themselves of all personal feeling and stick to the engineering problem in hand. I shall try, as I would wish that some of those who followed them had done, to take that lesson to heart, and resist the great temptation that has been thrown out by several speakers, to lead me to reply in a vein of sarcasm. I will go through the points that have been raised in the discussion, separating the wheat from the chaff as much as I can.

Instead of replying to each gentleman in turn, and holding him up before you to ridicule or to admire, I have taken up all the subjects which have been discussed, in the order in which they were treated in the paper, and I have tried, as far as I am able to do, to divide the different speakers' subjects in classified order, and I shall take the different subjects in order, instead of the different speakers.

First, as to the object which I had in bringing forward this paper, on which there has been a deal of misunderstanding. What I have said is, that during the past few years the electric lighting industry in England has been delayed—it may be by the Electric Lighting Act, it may be by other causes—I say that it has been delayed in England. I say that during that time there have been hundreds of thousands of pounds spent abroad by people trying to learn the experience which is necessary to be learned before the thing can be made a commercial success. All I now say is that we should try to take advantage of the time and money which has been spent by those people upon those experi-

Professor Forbes.

ments. The only person who has ventured to oppose this line of argument which I have taken is Mr. Preece, who has maintained that I am quite wrong; that there has been no delay in the progress of electric lighting in England; that they have been going ahead faster in England than anywhere else; and that there is absolutely nothing that we can learn from foreigners, and that we can teach them everything. I am perfectly convinced that the English engineers can teach those abroad a very great deal. I know that the Americans, while they have been pushing on with their practice, have been deriving their theoretical notions from England, while practical progress was retarded here: they have confessed that themselves, and are perfectly willing to admit the obligations they owe to Englishmen. No greater advance has been made in practical work, I think I can say without fear of a doubt, than the advance that has been made by the Brothers Hopkinson when they published their paper on the design of dynamo machines—a paper which has been the basis of design in every part of the world. That and the discovery of the Gaulard and Gibbs system of distribution are the two greatest steps that have been made of late years—one making dynamo-design possible, the other making distribution possible on a large scale. This subject has also been dealt with by General Webber, in a spirit opposite to that of Mr. Preece, but I really need not waste more time on the subject; I feel perfectly confident that the general sense of those who have studied it is in favour of the view that I have expressed—that we have been retarded in our adoption of electric lighting.

Questions have been asked me with regard to Berlin, and as far as I can I will answer them.

I was asked by Mr. Kapp about the number of arc lamps which there are on the incandescent circuits. I am really not quite sure of the exact number, but it is nothing like so large a proportion, I believe, as he and Mr. Crompton have thought it was.

Then Mr. Kapp asks me whether the cost of the mains and feeders includes the arc lamps. It does.

It was assumed by Mr. Andersen, because I said there was

$1\frac{1}{2}$ per cent. variation of the mains, that that meant $1\frac{1}{2}$ per cent. up and down. That is not the case. There is only $1\frac{1}{2}$ per cent. variation allowed in the mains in Berlin anywhere—that is, $\frac{3}{4}$ per cent. up and down. Professor Forbes.

The engines were praised very much by Mr. Crompton on account of their absence from shock, noise, and so forth, and I quite agree with him that the engines are very good ; but I do not go so far with him as to say that we in England cannot produce engines as good as them. I feel confident that in that department of engineering we are still at the top of the tree.

I have been asked about the horse-power—the steam consumed per horse-power in Berlin. The exact statement is that the indicated horse-power is $13\frac{1}{4}$ lbs. of steam, the electrical horse-power is $15\frac{1}{2}$ lbs. of steam.

Complaint was made by Mr. Wright that I have not given some facts about the cost of production, as to the lbs. of coal used per unit, as to the ratio of the maximum current to the average current, and as to the use of concentric mains, and so on. I really thought that I had said as much about these points as I was entitled to in the limits of the paper. I have given a good many of these points, as he will find if he reads the paper.

With regard to Milan, Mr. Preece says there is nothing that Italy can teach us. I say that Milan—and Mr. Mordey has also stated so most positively—can teach us that a city lighted with electric arc lights is a thing very much to be desired, and that they can light up their streets right and left, all through the town, in a most splendid way and at a satisfactory cost.

I was asked by Mr. Mordey about the Edison meters which are used in Milan, as to their varying deposit with light loads, because if you do get any deposit with light loads then the resistance becomes much higher, or, in other words, the range is not large. The range of the meter is not large, as I stated in my paper at the Society of Arts, to which Mr Mordey has alluded. I have made very careful tests of it myself, and generally it varies from fivefold to tenfold range.

The excellent street lighting in Milan was also spoken of by Mr. Mordey, and I think that everyone who has visited that town

Professor Forbes.

must look anxiously forward to the time when we shall have a large portion of the city of London so lighted.

Mr. W. M. Mordey: Could you give us the range of loss of the E.M.F. in the meters?

Professor G. Forbes: Only a very small fraction of the current passes through the electrolytic cell, and the resistance of the whole thing is $\frac{29}{10000}$ of an ohm.

Mr. W. M. Mordey: That is the resistance that the meter is a shunt of?

Professor G. Forbes: Yes.

Mr. W. H. Snell: Might I be allowed to say that I had a letter from Edison's laboratory only yesterday morning, and it mentioned, among other things, that their latest meters have a loss of ·2 per cent. at full load.

Professor G. Forbes: That is an interesting fact.

That Rome was dealing with alternating currents, one would have thought would have been a point which would be freely discussed, but Mr. Mordey alone has noticed it. He has asked about the self-exciters, how they work, and whether their efficiency comes out well.

I never saw a self-excited alternator with so little sparking at the commutators; it certainly is extremely good. I remember, a few years ago, having one of the Lontin type under test for some time, and the sparking was something terrible in a self-exciting arrangement like that; but in these Zipernowski ones there is very little sparking at all. But, as a matter of fact, experience has shown, by the results of Messrs. Ganz and others, that it is better to use an independent exciter, and that the efficiency of working is really better under those circumstances.

As to the lessons to be learned, I will take these in order, and so I shall be able to get rapidly through the different points that have been raised.

I must say that every word almost that has dropped from any speaker, in whatever humour he has been speaking, or upon whatever part of the subject he has been speaking, has confirmed me in my belief that we have lessons to learn by studying the work which has been done abroad—lessons as to what to do, and lessons as to what to avoid.

First, as to the distribution, which most speakers were dealing with last evening. The first point is as to the three-wire system. Dr. Fleming and Mr. Crompton have both supported the statement, which I think ought to be a maxim, that the three-wire system ought always to be used in a low-pressure system of distribution. The only person who threw doubts on the question was Mr. Shoolbred, who in a very cautious way suggested that "hitherto "the success of the three-wire system in England has not been "sufficient to warrant its general introduction." That is a type of argument which is very apt to prevail in England, that because a thing has not been largely used in England yet, we had therefore better wait; and I think those very cautious words will indicate a great many of the reasons why more progress has not been made in this country. Professor Forbes.

The same thing applies to electric traction: we find that the same argument holds very much in regard to it. The railway engineers will tell you that the success of electric traction in this country has not been so great as to warrant its adoption. Yet, in America they have their 250 horse-power locomotive running on the elevated railroad in New York, drawing its complete quota of carriages, and doing its regular work with the steam locomotive every day; and they have their Richmond electric tramcars, and electric traction is going ahead splendidly in America.

The next point is about feeders. Mr. Kapp complained that I did not now describe to you the conditions of working feeders. Well, considering that I devoted so much time to them in the course of Cantor lectures to the Society of Arts, I hardly thought that this was the time to deal with these theoretical points about the principles of distribution. He pointed out that he and Mr. Crompton had considered the question thoroughly in their designs for the Victoria Station a long time ago, and he will find in the Cantor lectures that I fully went into these points, and showed how far the question had been dealt with up to that time by Mr. Crompton, and wherein his errors lay.

Feeders were stated by Dr. Fleming to be most important and essential to any system of distribution, and Mr. Lant Carpenter has said that it is like flogging a dead horse or killing the slain

Professor Forbes.

to speak of feeders. I wish, gentlemen, that every person in this country, and every engineer in this country, would hold the same opinion. I cannot tell you the amount of trouble I have at times in having to argue this point with most practical engineers who are opposed to using feeders with their mains. There are a great number of people who wish to lay down their central stations without feeders. The only person who spoke against feeders at this meeting was Mr. Andersen, who took some trouble to show that it was possible, in a low-tension system, on the three-wire system going up to 400 yards, to get a fair distribution of electricity without using feeders, and without any very great expense in copper. Granted that is so: undoubtedly we all admit that to Mr. Andersen; but Mr. Andersen has never attempted to show that he would not get a far better distribution with the same quantity of copper laid down along the same conduits if he used feeders to supply the mains; and there is not the slightest doubt, just as all the speakers have said, that feeders are absolutely essential for that, and as Mr. Andersen has not attempted to deny the fact that feeders would improve the system, I need hardly waste more time upon the question.

Mr. F. V. Andersen: I would like to say that all my remarks were certainly based upon the supposition of the use of feeders. I think if Professor Forbes will read my remarks he will find that so.

Professor G. Forbes: I beg your pardon most sincerely.

Mr. F. V. Andersen: I mentioned the mains in the feeding centres, of course, and I understood that the mains around the station would be supplied by feeders, and I mentioned the feeding centres.

Professor G. Forbes: I understood that the whole system of mains that were laid down were to be tapped by the houses along which they passed, and that no separate feeders would be led away untapped up to the centres of distribution.

Mr. F. V. Andersen: I must apologise if I have not made myself clear enough. My remarks were based on the principle of having radial mains, and feeding them radially from feeding centres.

Professor G. Forbes: Then you accepted the principle of feeders? Professor Forbes.

Mr. F. V. Andersen: Certainly.

Professor G. Forbes: That is all right. Then everybody here admits that feeders are to be used for low-tension systems: that is a point I am glad to find we are all agreed upon.

The next question is as to using dynamos at different potentials, and putting the pilot wires in a different position to that which has been generally adopted. Mr. Andersen has said that using dynamos at different potentials is a little complicated, and that is a question that must of course be considered in any case where it is proposed to do so. I may mention, however, that the complication, which consists in changing feeders from one dynamo to another, is an operation which is being daily performed in large numbers of central stations everywhere, and I do not think that there is any trouble in doing that at all. But undoubtedly it would be simpler, as Mr. Andersen very justly remarks, if we could do without it; it is only a question of whether the benefit to be gained is worth it. The suggestion that Mr. Kapp made, that a counter electro-motive force might be introduced in the form of a battery cell to change the pressure given in the different feeders, perhaps limits the complication that it would be necessary to introduce.

Objection was made by Mr. Andersen to putting the pilot wires into the branches. Now, pilot wires, wherever you place them, if you are distributing with all your dynamos at the same potential, your pilot wires must be grouped together, and you must take an average of the readings of the pilot wires; that is the practice universally adopted where pilot wires are used, and if that is the case, then the objection which Mr. Andersen has raised hardly holds. His objection is that your pilot wires, if they go beyond the feeding centres, will go into the branches. Well, let them go into the branches, and let them go into every one of the branches, but put them in at the half-way place which I have pointed out as the proper place, then take the average of them, and you will get very close approximation to the average pressure which you ought to supply those points.

Professor Forbes.

I have put aside a sheet of paper for each of my different heads, and I have not got any discussion note about alternate-current distribution, which I thought was about the most important thing to talk about, therefore I have nothing to answer under that head.

I think it was General Webber who was the only one who pointed out that feeders are not used at the present time in the largest central station in London, and I have pointed out in my paper that that is largely the cause of the unequal lighting which we have over London. Mr. Preece has called my statements in question, but I am sorry to say that that must be due to his not going about sufficiently in London to see these lamps. If he did, I have not any doubt about it that he would find that what I said in my paper is correct, and that the variation of potential over the district lighted from the Grosvenor Gallery central station is enormous at the times of maximum supply; and he would rightly come to the conviction that we have just arrived at universally, by general consent, that in any system of distribution feeders are essential to keep the potential uniform.

The next question is as to high-speed engines. I have had very few remarks upon that question too. Mr. Swinburne has backed me up by saying that in properly constructed machines high speed is good, and that we should always try to get as high a speed as is consistent with the safety and efficiency of the machinery. I decline altogether to accept Mr. Preece's statement which he made to-day, viz., that in this country we have got a definite peripheral velocity of so many feet per minute, and that it is all the same whether you have a complicated system of wire coils or a symmetrical mass of iron which is revolving. I decline absolutely to accept that as a maxim in engineering. The amount of speed which it is safe or desirable to use depends on the construction of the machine, and a machine which is well constructed—which is perfectly symmetrical, and of solid parts that are not likely to fly asunder—may be run at a far higher speed than a machine with complicated parts which cannot be easily balanced, and with parts of the machinery which will be liable to injury from the enormous centrifugal forces which are present.

The next question is that of coupling large machines together in parallel with continuous current. Now the general experience which has been related here has been that there is no difficulty in coupling continuous-current machines in parallel. I shall be very glad, indeed, if that is the case: I am certainly not prepared to say it is not the case, but I shall be very glad to see it proved that it is. I think that most of those who made this assertion, however, have been dealing with comparatively small current. Mr. Crompton says that there is absolutely no need of resistance or banks of lamps to equalise the output before switching in a dynamo in parallel; he says that children can do it, that in the Vienna central station it is done perfectly easily. I still adhere, however, to the belief that where this plan has not been adopted in the past, there is a disagreeable momentary flicker. At the Vienna central station I do not think Mr. Crompton has got any very large current. As far as I remember, the outputs of his dynamos are somewhere between 100 and 200 amperes. Professor Forbes.

Mr. R. E. Crompton: 250.

Professor G. Forbes: Well, it is when you are coming to 1,000 amperes or so, that anyone I have heard remark upon it, states that it is essential to have banks of resistances. It is stated in the paper that for 100 amperes it is certainly utterly unnecessary. I am of opinion that, if we had an automatic means of switching in, such as has been described by one of the speakers, it could be done quite well, even with the largest current. I believe that Mr. Andersen himself, though he did not mention it in his remarks, has designed an automatic switch which is perfectly successful, and will probably work with the largest current.

The frequency of alternations has been spoken about by Mr. Kapp and by Mr. Preece. With regard to Mr. Kapp's statement, I am sorry he is not here. Mr. Kapp has maintained here and elsewhere that the size of a converter does not depend upon the frequency of the number of alternations. I want to know where he gets this belief from. He brings as a proof of this that in taking the figures I gave in speaking about the Westinghouse converters, I had spoken about the weight per horse-

Professor Forbes.

power, and he finds in his much larger machines he has the same weight per horse-power. Now, I will simply state this fact—that when Mr. Kapp pointed that out to me some time ago, I explained clearly to him that in the weight which I had given for the Westinghouse converter, the weight of a cast-iron casing, weighing almost as much as the whole converter itself, was included. That puts a different complexion on the case. The saving thus becomes very appreciable, and the fact has been admitted by everybody else except Mr. Kapp. It has been admitted by Mr. Preece, it has been admitted by Dr. Fleming; it is admitted by those who have had large experience in manufacture; Mr. Zipernowski mentioned it to me the other day; I believe that Mr. Ferranti admits it thoroughly, and I know that Mr. Westinghouse does.

It was said by Mr. Preece that if Mr. Westinghouse knew what wonderful things are being done in England, he would change this and reduce the speed of his alternations. I will tell Mr. Preece a little fact about this. When I was in America a year ago, the question of running motors with alternating currents was one of very great importance. I had some reasons for thinking that probably a reduction in the number of alternations would assist the running of motors, and I asked at the time if they were perfectly sure that they had got the best speed of alternations for satisfactory working. Since that time they have been almost constantly at work in making proper tests of their converters with ice calorimeters, and they have finally decided, after a year's practical working and testing of that sort, that they will not reduce the speed that they have got—that the high speed which they are using is the best for converter work. Generally, as to the frequency of alternation, about which Mr. Preece has spoken to-night, the conclusion comes to very much as I said in the paper. If you want high efficiency use high speed of alternation, but if you want to work in parallel it is best to sacrifice that and reduce the frequency.

We were told by Mr. Swinburne that he has devised a means for parallel working. That is very interesting, and I am sure he will go on with it, and work it out, and get it into thorough working

order. But I do not think it has been proved in actual practice yet. I am sure no one will be more pleased than myself to see if it is possible to produce parallel working without the heavy self-induction that is at present necessary. Professor Forbes.

Now the important question of insulation comes on, and I have delayed my remarks on that till now. A good deal of valuable information, I think, has been gained upon this, and I am glad the discussion has been raised, if only to clear the ground a little; I am only sorry that it has been cleared so little. We have not arrived at anything very much more definite, I think, than what we started with. Let me take Mr. Siemens first, and, in doing so, let me again thank Mr. Siemens for the courteous way in which he spoke on the subject; and I must say that when I was raising this question I had no idea that I was treading on the toes of Messrs. Siemens & Halske; or that I thought that Messrs. Siemens & Halske would not really have given me the information I obtained, just as readily as those from whom I did obtain the information. But at the same time I have simply stated what I found to be the case in Berlin—at least what I believed to be the case in Berlin. Mr. Siemens, however, has told us that a general breakdown has not taken place. He says that eight cables were replaced: one was injured by a pickaxe; two were, I think he said, badly put into the joint box; and the five others are not definitely explained, except by the belief that they were due to mechanical injury; and that in other places where tests had been made it was found that the only injury was that high temperature had melted the insulation.

Mr. ALEXANDER SIEMENS: No; the outside covering. It had not interrupted the insulation; that is just the thing—the outside covering and the compound were melted, but the insulation was not damaged.

Professor G. FORBES: The insulation was not damaged! I cannot decide the question as to what is the true state of the case about these Berlin cables. But let me take exception to what Mr. Crompton said. Mr. Crompton said, in speaking of this, that it is his belief that when I am in such a position I am easily gulled. It has been with the utmost amusement that I have frequently

Professor Forbes.

noticed that he held this belief when he has been describing to me his latest electrical hobby, such as the Howell battery. When I first came to join the engineering profession in London, I was amused by the number of people who held that opinion; Mr. Crompton, I think, is the only one left, and he still continues to afford me that great amusement. I am not apt to take the first story which is told by interested parties; I am not apt to take statements without sifting: I invariably try to sift the evidence as much as I can, and, if I cannot get at it by actual test, I consider very carefully the probabilities. I think we must agree with Mr. Eddison that lead cables are on their trial. Let us hope they will come out successfully.

The question as to lead-covered cable is very important. Mr. Preece made a great point when he said that it has been working well in telegraphy, when he brought the experts of the Post Office here to support him in the statement that lead-covered cables have worked well in many cases. That is very good, but it must be remembered that these lead-covered cables were generally well insulated without the lead; it must be remembered that the modern electric light cables without the waterproof lead coating would be badly insulated; it must be remembered that in some cases lead-covered cables have failed when they have passed through made-up ground or decaying vegetable matter; it must be remembered that in telegraphic work you are not working with 2,000 volts—all these things must be considered, and the opinions that Mr. Preece's experts supported do not bear upon those points. But Mr. Preece has staked his professional reputation upon this. I grant the enormous value of that statement, and I certainly shall be led from that statement to myself carefully reconsider the question of lead-covered cables, which I had been inclined to look on with disfavour.

As to compound-wound voltmeters: of course Dr. Hopkinson was the first inventor, that is perfectly well understood, and other people have only been helping to work the instruments out in a practical way for special purposes.

In conclusion, let me say to the gentlemen who have spoken —let me say to Dr. Fleming, Mr. Kapp, Mr. Swinburne, Mr.

Crompton, Mr. Siemens, Mr. Andersen, and also let me say to Mr. Parker and the host of other gentlemen who have not spoken, but who ought to be mentioned in this connection—that there is no person in this or in any other country who can compete with me in the intense admiration I have for the enormous efforts that they are making to raise the standard of electrical manufacture to the proper standard which it ought to attain in this country, and that I look upon the advances which *are* being made in this country with the fullest confidence. I am as conscious as anybody can be, of what is being done in this country, and I value the efforts of these gentlemen as much as any person in the world can do. Professor Forbes.

A hearty vote of thanks was unanimously accorded to Professor Forbes for his very interesting and valuable paper.

A ballot for new members took place, at which the following were elected :—

*Foreign Member :*

Hidesuke Igarashi, M.E.

*Members :*

R. S. Erskine. | W. M. Shaw.
A. L. H. Palmer. | Arthur R. Simkins.

*Associates :*

M. S. Chambers. | Sydney Dobson.
Henry T. Thornburry.

*Student :*

Charles Oliver Lloyd.

The meeting then adjourned.

The One Hundred and Ninetieth Ordinary General Meeting of the Institution was held at the Institution of Civil Engineers, 25, Great George Street, Westminster, on Thursday, March 28th, 1889—Mr. ALEXANDER SIEMENS, Vice-President, in the Chair.

The minutes of the Ordinary General Meeting held on March 21st were read and approved.

The names of candidates for admission into the Institution were announced and ordered to be suspended.

The following transfers were announced as having been approved by the Council:—

From the class of Associates to that of Members—
Samuel S. Dickenson.

From the class of Students to that of Associates—
Charles Woodward Neele.

A donation to the Library was announced as having been received since the last meeting from Mr. Charles Streatfeild James, to whom the thanks of the meeting were unanimously voted.

The CHAIRMAN: I have now the very sad duty to perform of announcing to you the death of an old member of this Institution, the intelligence of which reached us in the Council Room only a few moments ago: Mr. C. H. B. Patey, C.B., the Third Secretary of the Post Office, and who had the management of the Telegraph Department, died at his residence at Bickley at five o'clock this afternoon; and I will call upon Mr. C. E. Spagnoletti to move a resolution which the Council desire to submit to you.

Mr. C. E. SPAGNOLETTI: Mr. Chairman and gentlemen,—I rise with feelings of very great regret and sadness to propose that our Secretary be authorised to write a letter of condolence to

Mrs. Patey on the very sad event of which you have just heard. I am sure that I shall have the heartiest support and sympathy of every member of the Institution in making this proposition. Mr. Patey, as you are all aware, was a gentleman in a very prominent position in the Post Office, being the Third Secretary, and he had raised himself to this position, and distinguished himself therein, by his excellent business habits, and by the performance of his onerous duties to the satisfaction of everybody during the time he held that position. His duties were principally confined to the Telegraph Department. On account of his excellent business qualifications and his great tact, he was appointed, in 1879, President of the International Telegraph Conference then held in this country, which I daresay many of you may recollect. I learn also that he was most active in urgently pressing forward and bringing about the adoption of the sixpenny rate for telegrams, which has proved such a boon to the general public. Mr. Patey was also engaged, and made very great reforms, in the Intelligence Department, by which the Press of this country has benefited very much, and by which all intelligence of public interest is now so well circulated over all parts of the globe. For his distinguished talents he was made a Companion of the Bath in 1886; and latterly he has been engaged, as I daresay many of you know, as the principal negotiator for the purchase of the Submarine Telegraph Company's property. Mr. Patey was taken ill on Saturday last, March 23rd, with congestion of the lungs; his friends were hopeful on Monday that his health would improve, owing to favourable symptoms; but, however, a relapse unhappily occurred, and he passed away at five o'clock to-day. It is very sad to think of a man of his age—only a little over forty—being cut off in the prime of his career, when he was doing such excellent work and making himself so useful to his fellow-creatures, and I am quite sure that you will all agree with me that it is our bounden duty to record the deep regret which we all feel on the occasion. I therefore beg to move—"That the Secretary be instructed to communicate to Mrs. "Patey the expression of the deep regret felt by the Council and

"members of this Institution at the decease of Mr. Patey, and of "their sincere sympathy with her in her bereavement."

Professor W. GRYLLS ADAMS, F.R.S.: I beg to second the proposition which has been made by Mr. Spagnoletti.

The motion was unanimously carried.

The CHAIRMAN: The first business of the meeting is the presentation of the Balance-Sheet for the year 1888, a copy of which, having been sent to every member in England, it will not be necessary to read at this meeting; I therefore call upon any member who may have remarks to make or question to ask in reference to the accounts to do so now.

After a pause,

The CHAIRMAN: As no one appears desirous of making any comment, I beg to move—"That the Balance-Sheet and Statement "of Accounts for the year ending 31st December, 1888, as now "presented, be received and adopted." *

Mr. LAUCKERT seconded the motion, which was carried unanimously.

The following paper was then read:—

## LABORATORY NOTES ON ALTERNATE-CURRENT CIRCUITS.

By Professors W. E. AYRTON, F.R.S., V.P., and JOHN PERRY, F.R.S., Member.

At the last meeting Professor Perry and I received an urgent request from your indefatigable Secretary to read a paper at this meeting, as unforeseen circumstances had prevented the authors of two other papers from sending them in in time. We pointed out that we had no paper ready; we were, however, urged to do something, and it occurred to us that possibly some short account of a few of the experiments that some of the students of the Central Institution are at present engaged upon might be of interest to the members.

You are of course all aware that, when you are dealing with

* For Balance-Sheet see page 322A.

varying currents, self-induction is of importance; indeed, if the alternations are only sufficiently rapid, self-induction completely replaces resistance, the resistance becomes entirely unimportant, and self-induction is all-important. In the case, for instance, of a well-made transformer, the resistance of the primary circuit may be practically neglected altogether, and all that you have to consider in order to determine what current will flow through the primary circuit for a given mean potential difference at the terminals, is the effective coefficient of self-induction. In spite of the importance that self-induction possesses, and in spite of the fact that the largest distributions of electric power in this and in other countries are carried out by means of alternating currents, there is a wonderful amount of ignorance possessed by us all as to what is the coefficient of self-induction of any particular circuit. We have, in fact, no instinctive feeling whatever as to the value of the self-induction of a coil or a circuit when we see it. Some time ago we were asked by a well-known physicist whether we could tell him what was the self-induction of a Thomson reflecting galvanometer of the ordinary form, having about 7,000 ohms resistance. He said, "I am not particular to "50 per cent., or even 100 per cent., but can you give me any "sort of idea as to what it is? Is it, for example, to be measured "in centimetres or in miles? and is it a large number of miles or "a small number of centimetres?" Well, that impressed upon us how little was any instinctive feeling as to the magnitude of the coefficient of self-induction of any particular coil called into existence by the appearance of the coil.

As an example, here is a coil wound so that you can see the number of layers, as well as the number of convolutions in each layer. The wire is thick, so that the total number of convolutions can be easily counted, and is seen to be 600. Now, although there is no iron in this core, and although the windings are practically symmetrical, we think that probably no single person in this room can say what is even the approximate value of its self-induction. Were we to tell you that the resistance of this coil were a megohm or a microhm, you would smile, because your experience tells you that the resistance must be of

the order of a few ohms. As a matter of fact it is 1·34 ohm. Possibly you could not say from merely looking at the coil whether its resistance was half an ohm or 5 ohms, but if there be no discontinuity nor short-circuit you are perfectly certain that a megohm is far above the true value, and a microhm far below. But if we were to tell you that its self-induction was a million miles, not a single brow would be raised in astonishment; or if we were to state that it was about 20 yards, you would receive that statement with perfect equanimity; or even if we were to say that it is about 100 miles, you would not smile,—well, you ought not to smile in that case, for it is about 100 miles, being in reality 92 miles.

Now, how have we got this clear notion with reference to resistance? Obviously by a large number of measurements having been made, by our having had to find out what was the resistance of all sorts of lengths of wires and of coils of various sizes and shapes, and therefore we know, from a sort of instinct which has grown up as the result of a large number of measurements, what is roughly the resistance of any particular wire when we see it. It therefore seemed to us that it might be of some interest to you to have the results that have been obtained by various students as to what are the actual coefficients of self-induction of some simple circuits.

The experiments have been made by various groups of students under the charge of Mr. Sumpner; in fact, for some time past, scarcely a coil has been allowed to remain peaceably at rest in our laboratory, but it has been operated on, and its coefficient of self-induction measured. Hence our students are just beginning to acquire that instinctive feeling of which we have spoken, regarding the magnitude of the self-induction of different circuits, and it is our desire to go a little way this evening towards imparting that feeling to you.

First, of course, we must be familiar with the practical unit of self-induction. We should like to be able to say that of course you are all familiar with it, but in view of recent experience we fear that that would be too great an assumption; for only quite recently, at a meeting of the Institution of Civil Engineers, a

very large mistake was made by a well-known electrical engineer regarding the magnitude of the unit of self-induction—a mistake which, we pointed out at the time, was very like that of confusing the distance between here and Charing Cross, which is about a mile, with the distance from here to the sun, which is about 100,000,000 miles. The practical unit of self-induction is $99{,}777 \times 10^4$ centimetres; it is not exactly the earth's quadrant, in consequence of the legal ohm not being exactly the intended or true ohm, and therefore, if you want the equation

$$r\,C + L\,\frac{d\,C}{d\,t} = E$$

to be true where $r$ is in ohms, $C$ in amperes, and $E$ in volts, $L$ must be expressed in a unit which is $99{,}777 \times 10^4$ centimetres, about 6,200 miles.

For want of a better name, we suggested some few years ago that the name "secohm" should be employed for this unit, it being of course a second × an ohm, so that a secohm is really $99{,}777 \times 10^4$ centimetres, or about 6,200 miles, or two-hundredths of a secohm is rather more than 300 miles.

When making that suggestion, we showed you an arrangement, which we called a secohmmeter, for enabling the coefficients of self-induction to be measured with the same facility that you have been accustomed to measure an ordinary resistance with a Wheatstone's bridge. Of course, self-induction only shows its effect when currents are varied. You cannot, of course, measure it by steady currents; but by performing certain operations which were explained to you some two years ago, you are able to make the measurements with alternating currents or varying currents nearly as easily as you are accustomed to measure resistances by the use of steady currents.

In the secohmmeter that we devised the operation that was performed was this: the battery circuit was alternately made and broken, and during one of these operations—say, during the make of the battery circuit—the galvanometer was rendered inoperative by being short-circuited, so that during every break of that battery circuit the battery was operative, while during every make the galvanometer ceased to be operative. In that way the effect

of self-induction was apparently to increase the resistance of a particular circuit by a definite amount, depending on the speed, and from the apparent increase of resistance and the speed the coefficient of self-induction was determined. Since then we have made various alterations in the instrument. One improvement was a very obvious one—to reverse the battery instead of making and breaking the battery circuit; and, lastly, to reverse the galvanometer instead of short-circuiting it; so that, at present, with the latest form of the secohmmeter we alternately reverse the battery circuit and the galvanometer circuit, and, as four reversals of each circuit occur for each rotation of the commutator spindle, the sensibility of the latest form is eight times as great as the first form that we had the honour of bringing before the Society two years ago.

The commutators can be driven at one or other of two speeds relatively to that of the driving handle. With one arrangement there are rather more than eight reversals of both the galvanometer and of the battery for one revolution of the handle, and with the other twenty-four reversals of each for one revolution of the handle. The apparatus is so constructed that the speed of the fly-wheel remains the same relatively to that of the handle, whatever speed ratio be employed, and hence the same uniformity of speed can be obtained with either speed ratio. The secohmmeter can be conveniently driven by hand so as to obtain a constant speed of reversal varying from 300 to 6,000 reversals per minute of both the galvanometer and the battery.

To shift from one speed ratio to the other, press down the end of the locking lever at the right of the secohmmeter, and slightly push in or pull out the handle, turning it slightly to assist the toothed wheels engaging properly; when engaged, let go the end of the locking lever.

With the original form of the instrument we proposed to use a speed indicator attached to the rotating spindle, but we pointed out that in some cases it would be desirable to dispense with the speed indicator, especially in view of the difficulty that exists in getting a good cheap speed-indicator; and so most of the measurements that we have to bring before you to-night have not been

made in accordance with the first method. We proceeded, not by comparing the coefficient of self-induction in terms of a resistance and a time, but by comparing one coefficient of self-induction with another. You know, of course, if you have a Wheatstone's bridge (Fig. 1) with two wires possessing certain resistances $r_1$, $r_2$, and

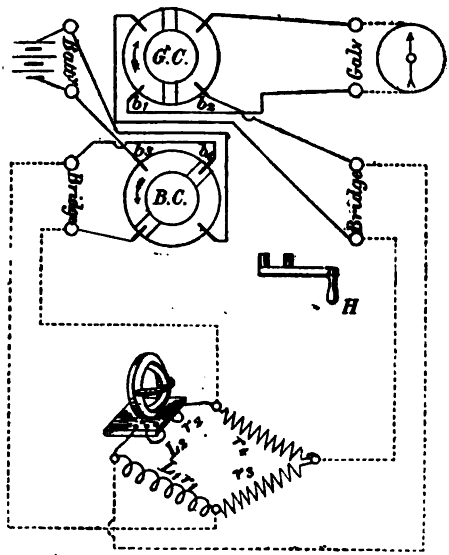



*Fig 1.*

coefficients of self-induction $L_1$ and $L_2$, and two other wires possessing resistances $r_3$ and $r_4$, but no self-induction, that if you first balance for steady currents, that is, satisfy the equation

$$\frac{r_1}{r_2} = \frac{r_3}{r_4},$$

you will also have balance for varying currents if in addition you make

$$\frac{L_1}{L_2} = \frac{r_3}{r_4}.$$

The second equation can be fulfilled without disturbing the resistances, that is, without altering the equality of the resistance ratios, if one of the arms contains an apparatus of adjustable self-induction; and further, if [the value of self-induction of this apparatus can be read off in secohms for each position of its

adjustment, we can at once use the secohmmeter for the measurement of a self-induction without the use of a speed indicator.

In Fig. 1, *BC* is the commutator for periodically reversing the battery connections, and *GC* that for periodically reversing the galvanometer connections, the thick lines in the figure representing permanent connections in the secohmmeter itself, and the dotted lines connections temporarily made outside it. When making such a comparison between an unknown and a known coefficient of self-induction, the speed at which the secohmmeter handle, *H*, is driven need not be known, but the greater the speed the more sensitive the test; the rate of reversal must not, however, be too great for the currents to reach their steady values between two consecutive reversals.

The variable standard of self-induction that we have been employing is a modification of that used by Professor Hughes and by Lord Rayleigh, and consists of two coils whose planes may be made to have any angle with one another. The speciality of our apparatus is that we have determined the exact value in secohms of the self-induction of the arrangement for various positions of the coils relatively to one another. The values are recorded on the dial fixed to the larger coil, and over which moves the pointer attached to the smaller coil. The instrument thus constitutes a direct-reading variable standard of self-induction.

The wire on both coils is of platinoid, so that the variation of resistance by temperature is practically negligible. The smaller and movable bobbin, which we call *A*, is wound with 299½ turns; the larger stationary bobbin with two coils, one of 49 turns, which we will call *B*, and the other of 147 turns, called *C*. *B* and *C* may be used separately or together, so as to help one another, or so as to oppose one another. There are, therefore, four combinations, *A* and *B*, *A* and $B-C$, *A* and *C*, *A* and $B+C$, and with each of these arrangements the smaller coil may be turned round through 180°. The range of the instrument is—

| | | | |
|---|---|---|---|
| $A$ & $B$ | steadily increasing self-induction from | 0·0135 to 0·0175 | secohm, |
| $A$ „ $B-C$ | „ | 0·0157 „ 0·0229 | „ |
| $A$ „ $C$ | „ | 0·0171 „ 0·0283 | „ |
| $A$ „ $B+C$ | „ | 0·0215 „ 0·0365 | „ |

the total adjustable range is thus from 0·01356 to 0·0365 secohm, or from about 84 miles to 226 miles. It will be observed that the various component ranges overlap one another fully, so that many of the values can be produced with two arrangements, which is convenient for the purpose of checking the accuracy of a measurement. The smallest coefficient of self-induction that can be obtained with the particular standard is by the use of coil *B* alone, which corresponds with 0·00096 secohm.

The apparatus therefore constitutes a direct-reading secohm standard. Although the coefficient of self-induction of the standard can only be made to have successively progressive values from 0·0135 to 0·0365 secohm, coefficients of self-induction which do not lie between this limit can be measured: for example, suppose the unknown self-induction $L_1$ of a coil of resistance $r_1$ ohms be about 0·5 secohm, then we add a resistance $r$ to $r_1$, so that

$$\frac{r_1 + r}{r_2} = \frac{r_3}{r_4} = 20, \text{ say,}$$

in which case balance for varying currents will be obtained when

$$L_1 = 20\, L_2\,;$$

and since 20 $L_2$ varies between 0·27 and 0·73 secohm, these limits include 0·5 secohm, and therefore balance can be obtained on rotating the secohmmeter.

The following are some of the results that the students, under the guidance of Mr. Sumpner, have obtained, using the secohmmeter and the adjustable secohm standard just described:—

Coil wound with copper wire 34 mills. in diameter, covered to about 48 mills., consisting of 48 layers with 13 convolutions in each layer, wound on a wooden core 2 inches thick and 4 inches long.

$$L = 0{\cdot}0147 \text{ secohm}, \quad r = 1{\cdot}34 \text{ ohm}, \quad \frac{L}{r} = 0{\cdot}011 \text{ second};$$

that is, the "time constant" of this coil is 0·011 second, which is the time required for any current to rise to 0·06321 of its maximum value after it is started in the coil by an application of a fixed P.D. between the ends of the coil.

Single coils of an ordinary Morse receiver, iron core about 0·31 inch thick and 3 inches long. Outside diameter of coil 0·94 inch.

$$L = 0{\cdot}0936 \text{ secohm}, \quad r = 32 \text{ ohms}.$$

Another single coil of an ordinary Morse receiver: iron core about 0·31 inch thick and 3 inches long, outside diameter of coil 1·25 inches.

$$L = 0{\cdot}444 \text{ secohm}, \quad r = 50 \text{ ohms}, \quad \frac{L}{r} = 0{\cdot}0089 \text{ second}.$$

The resistance of this second coil is only $\frac{5}{3}$ that of the former, but its self-induction is over four times as great.

The two complete coils of a Morse receiver in series, with iron sole-plate and armature—the armature, however, being rather a small one.

$$L = 0{\cdot}265, \quad r = 14 \text{ ohms}, \quad \frac{L}{r} = 0{\cdot}018 \text{ second}.$$

Coil of a low-reading magnifying spring voltmeter wound with copper wire on a brass bobbin, no iron inside. Length of coil 2·88 inches, external diameter 3 inches, diameter of brass bobbin on which wire is wound 0·38 inch.

$$L = 1{\cdot}462 \text{ secohm}, \quad r = 333{\cdot}5 \text{ ohm}, \quad \frac{L}{r} = 0{\cdot}0044 \text{ second}.$$

Single coil of an ordinary Thomson's reflecting galvanometer.

$$L = 2{\cdot}56 \text{ secohms}, \quad r = 2{,}700 \text{ ohms}, \quad \frac{L}{r} = 0{\cdot}0007.$$

Single coil of a very high resistance Thomson's reflecting galvanometer, the coil being of about the ordinary size.

$$L = 70 \text{ secohms}, \quad r = 100{,}000 \text{ ohms}, \quad \frac{L}{r} = 0{\cdot}001.$$

Comparing these two last results, we see that the ratio of the self-induction is about the same as the ratio of the resistances, which is correct, as each ratio is proportional to the square of the number of turns of wire when the coils are of the same size and shape. Hence the time constants are about the same.

An ordinary Ruhmkorff induction coil intended to give a 2-inch spark.

Secondary coil, $L = 51{\cdot}2$ secohms, $r = 5{,}700$ ohms, coefficient of mutual induction, 0·46 secohm.

Ferranti dynamo intended to give 200 volts and 40 amperes.

*Armature*, $L = 0{\cdot}0013$ secohm for one phase,

$= 0{\cdot}0011$ „ for another phase,

when no current is passing through the field magnets.

*Field Magnets* in series $L = 0{\cdot}61$ secohm for a small excitation, $r = 3$ ohms.

Mather & Platt shunt dynamo, intended to give 100 volts and 35 amperes.

*Armature* from brush to brush,

$L = 0{\cdot}005$ secohm, $r = 0{\cdot}215$ ohm.

*Field Magnets* in series,

$L = 13{\cdot}6$ secohms for a small excitation, $r = 44$ ohms.

When there is iron in a coil the value of the self-induction depends, of course, on the strength of the current employed when making the observation, and on the way in which this current is varied for determining the self-induction. This will be found very fully worked out, with numerous experimental illustrations, in a paper by Mr. Sumpner, "On the Variation of the Coefficients "of Induction" (*Proc. Phys. Soc.*, vol. ix., part iii., for July, 1888). The following are illustrations of the sort of results obtained:—

*Experiments on the Self-Induction of a Kapp and Snell 2-H.P. Transformer.*

Through one of the circuits of the transformer of approximate resistance 0·07 ohm a current was passed, and which was made to have successively the values shown on the table. On changing its value great care was taken that the change was steadily in one direction without any fluctuations. The self-induction of the other circuit of the transformer of approximate resistance 0·14 ohm was measured in two distinct ways. With the first method the secohmmeter was employed to rapidly alternate a small current of 0·01 ampere through this circuit, while with the second method the self-induction of this circuit was measured by starting a small current of 0·037 ampere through this circuit. After stopping this current the iron was made to go through the complete magnetic cycle before making the next observation or even repeating the former. For instance, supposing the previous measurement

was made by starting a current of 0·037 ampere through the one circuit when the steady current through the other circuit was − 2 amperes, then on stopping the current of 0·037 ampere the current through the other circuit was first increased up to + 10 amperes, then diminished to − 10 amperes, and lastly increased up to − 1 ampere, for which value the next observation was made.

*Currents steadily Increasing from Minus to Plus.*

| Steady Current in one Circuit, in amperes. | Cyclical Self-Induction of the other Circuit, in secohms. | Self-Induction of the other Circuit, in secohms, measured by starting a small positive current in this circuit. |
|---|---|---|
| − 8 | 0·010 | ·008 |
| − 6 | ... | 0·016 |
| − 4 | ... | 0·036 |
| − 2 | ... | 0·100 |
| − 1 | ... | 0·156 |
| 0 | 0·030 | 0·216 |
| + 1 | ... | 0·264 |
| + 2 | ... | 0·260 |
| + 4 | ... | 0·064 |
| + 6 | ... | 0·016 |
| + 8 | 0·010 | 0·016 |

*Currents steadily Diminishing from Plus to Minus.*

| Steady Current in one Circuit, in amperes. | Cyclical Self-Induction of the other Circuit, in secohms. | Self-Induction of the other Circuit, in secohms, measured by starting a small negative current in this circuit. |
|---|---|---|
| + 8 | 0·010 | 0·008 |
| + 6 | ... | 0·012 |
| + 4 | ... | 0·028 |
| + 2 | ... | 0·092 |
| + 1 | ... | 0·144 |
| 0 | 0·030 | 0·208 |
| − 1 | ... | 0·264 |
| − 2 | ... | 0·240 |
| − 4 | ... | 0·092 |
| − 6 | ... | 0·048 |
| − 8 | 0·010 | 0·020 |

As has been already pointed out, the presence of iron in a circuit causes the self-induction of that circuit to have very different values; and, further, it causes the value of the self-induction to depend on the rate on which any particular change in the magnetic induction is effected. For example, if the current through one circuit of the transformer be kept quite constant, and the secohmmeter be used to alternate a definite small current through the other circuit, the value of the cyclical self-induction diminishes somewhat as the speed of alternation is increased.

When we had the honour of first bringing the secohmmeter to your notice in 1887, we exhibited certain curves (Fig. 3, page 307, *Jour. Soc. Tel. Engrs.*, part 67, vol. xvi., 1887) showing the apparent increase of resistance of a circuit containing no iron when the secohmmeter was turned at different speeds, and certain other curves (Fig. 4, page 310) for the apparent increase of resistance when an iron core was inserted in the coil. The first set of curves are straight lines, while the second show a distinct curvature. This curvature we attributed at the time to the fact that with high speeds of the secohmmeter the time constant of the circuit when the iron core was inserted was too large for the current to reach its steady value when the battery circuit was closed, or for the current to die away when it was opened. But on Mr. Sumpner's subsequently attacking this subject both experimentally and theoretically, he found that the increase of the time constant produced by the insertion of the iron was not sufficient to explain the large amount of bending of the curves. Hence we concluded at the beginning of 1888 that the bending was mainly due to a magnetic lag. At about the same time we obtained evidence that when the speed of the secohmmeter was *very great*, and the lead very small, the insertion of the iron core in a coil actually diminished the self-induction of the circuit—a result that we feared at the time must be due to some defect in the coil, but which the recent investigations of Dr. Lodge now show to be quite possible. This subject we are at present investigating further.



Some time ago we drew attention to the fact that the accuracy of the measurement by means of a wattmeter of the power given

by an alternate current to a circuit possessing self-induction depended on the relative values of the time constant of the fine-wire circuit of the wattmeter and the time constant of the circuit the power given to which it was desired to measure.

Our result was criticised by Mr. Blathy in the *Electrician* for April 6th, 1888, who made certain calculations regarding the use of the wattmeter constructed by Messrs. Ganz & Co. tending to show that the correction pointed out by ourselves was an unimportant one. We take the opportunity of drawing Mr. Blathy's attention to the fact that in his calculations he has under-estimated the value of the self-induction of the suspended coil, and—what is equally important—neglected altogether the self-induction of the so-called non-inductive portion of the fine-wire circuit. Taking the actual specimen of the wattmeter we possess, we find that the suspended coil of resistance 2 ohms has a self-induction 0·00042 secohm. This is about twice what Mr. Blathy allows for it. Further, two of our students—Messrs. Lamb and E. W. Smith—on making a calculation as to the self-induction of the very carefully *doubly wound* resistance coil of some 980 ohms which is placed in the fine-wire circuit, find that it is 0·00025 secohm, which is more than half the value for the inductive coil. And if, in the hypothetical case taken by Mr. Blathy, a *doubly wound* resistance coil of 100,000 ohms be employed, made of the same wire as is used in the coil of 980 ohms—and this would be necessary, since, the suspended coil remaining the same, the current-density in the high-resistance circuit must remain constant—they find that the self-induction of the *doubly wound* resistance will be 0·025 secohm; so that the self-induction of the high-resistance circuit will be about one hundred times as great as is supposed by Mr. Blathy.

For the purpose of enabling practical men to calculate the self-induction of *doubly wound* coils made of conducting wire of diameter $d$, covered to a diameter $D$, and placed parallel to one another, with their insulating coverings in contact, we give the following formula:—

$$L\text{, in secohms,} = \frac{422 \times \log_{10} \dfrac{D}{d} + 173}{10^9} \times \text{total length of going and return wire, in yards.}$$

## A Shunt Transformer.

The following problem presented itself to our student Mr. Smith, whose name we have just mentioned; and as his solution depends on the lag produced by self-induction, we have thought that this may be a fitting opportunity for bringing the matter forward. The problem is as follows:—If a certain alternate potential difference be maintained between two mains, *M M* (Fig. 2), between which there are two incandescent lamps, *P* and *Q*, is it possible to make the sum of the mean potential differences

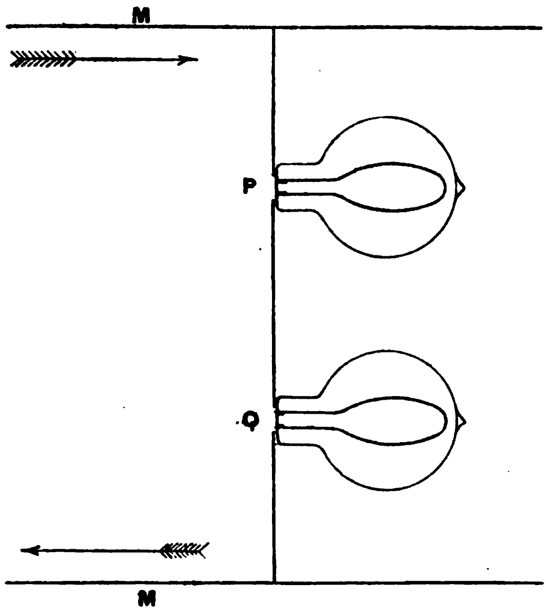



Fig. 2.

maintained at the terminals of the lamps greater than the mean potential difference maintained between the mains without disconnecting the lamps from the mains and inserting an ordinary transformer? For example, suppose that the mean potential difference between the lamps be 100 volts, and it requires a mean potential difference of 55 volts to be maintained between the terminals of each lamp to make it glow properly, can this result be attained without disconnecting the lamps from the mains?

Let 1 1 1 1 (Fig. 3) represent the curve of impressed potential difference: then this may be regarded as being composed of two

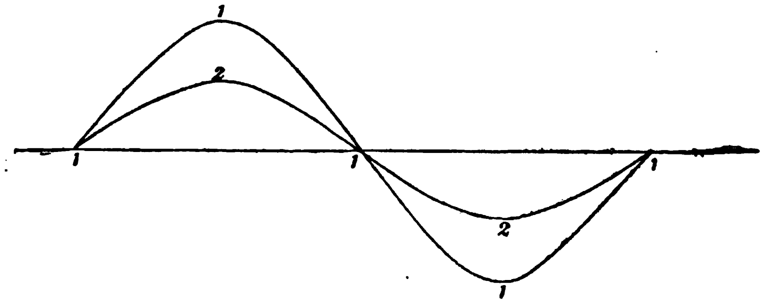


Fig. 3.

coincident curves, 1 2, 1 2, the ordinates of which are each exactly half the corresponding ordinates of the original curve 1 1 1 1. Now let it be possible to retard one of the smaller curves and accelerate the other so that they take some position like 3 3 3 3 and 4 4 4 4 (Fig. 4): then, since the sum of the corresponding ordinates of these

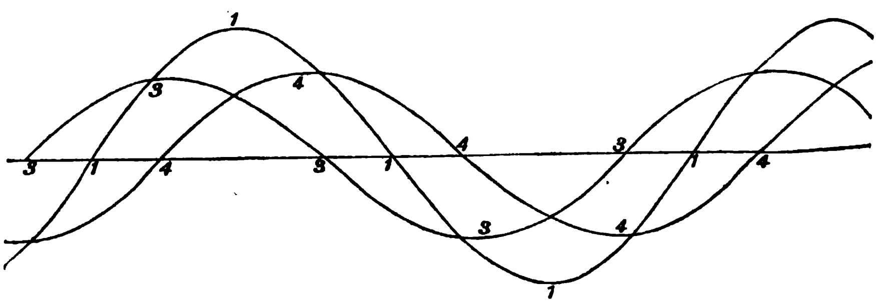


Fig. 4.

two curves is equal to the corresponding ordinates of the original curve, it follows that *the mean value of the ordinates of each of these curves must be greater than half the mean value of the ordinates of the original curve.* Mr. Smith's view was, if one of the lamps, $P$, were shunted with a high resistance possessing much self-induction (Fig. 5), and the other lamp, $Q$, with a non-inductive high resistance, the wave of potential difference at the terminals of $P$ would be accelerated, while that at the terminals of $Q$ would be retarded, as shown in Fig. 4; hence the result would be that the brightness of the two lamps would be increased by the application of the high-resistance inductive shunt and the high-resistance

non-inductive shunt—a result which experiment shows to be correct.

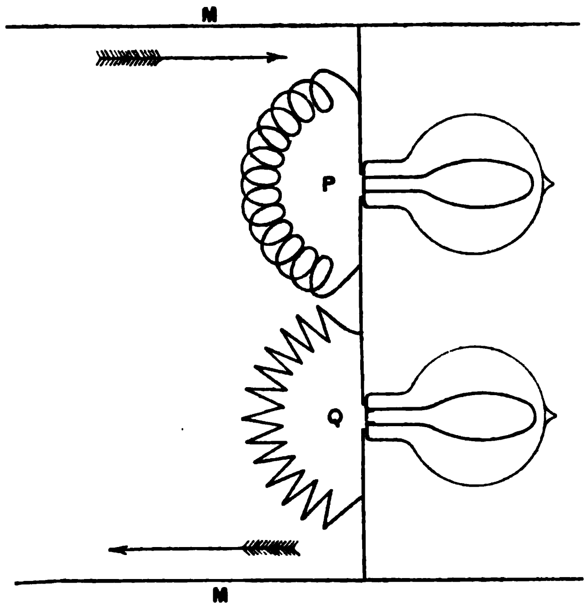



FIG. 5.

It will probably be objected to this solution that it cannot be employed without introducing a large waste of power, and therefore can have no practical value. But exactly the same objection was raised some years ago to the employment of the ordinary transformer. And just as the practical importance of using transformers has led to such improvements being made in them that the waste of power introduced by their use is far more than compensated for by the economy they introduce into electric distribution, so in the same way is it not possible that Mr. Smith's solution may be so improved that the gain in power arising from working incandescent lamps at their best efficiency may more than compensate for the loss of power in his "shunt transformer"?

## ON THE EFFECT OF SELF-INDUCTION IN DESTROYING RIPPLES.

In the discussion on Mr. Kapp's paper, and on former occasions, we have described to the Society the effect of self-induction in causing the current in an alternating-current circuit

to become more and more a sine function of the time. We assumed, of course, that there was a fixed self-induction of the circuit, and we were not considering the possibility of the presence of iron making the self-induction vary, as we know that it does. We have stated that the minor ripples or harmonics never wholly disappear.

The following is the mathematical investigation of this problem :—

The electro-motive force being any periodic function, it may be expressed in the shape—

$$E = E_0 + \sum_1^\infty a_i \sin.\left(\frac{2\, i\, \pi}{T} t - e_i\right) \quad \ldots \quad (1)$$

and as

(2) ... $E = r\,C + L\,\dfrac{d\,C}{d\,t}$, we have for $C$

(3)

$$C = \frac{E_0}{r} + \sum_1^\infty \frac{a_i}{\sqrt{r^2 + \frac{4\,\pi^2\,i^2\,L^2}{T^2}}} \sin.\left(\frac{2\,i\,\pi}{T} t - e_i - \tan.^{-1} \frac{2\,\pi\,L\,i}{T\,r}\right).$$

We may for any alternating current from any existing machine which is not in series with any constant-current machine assume that $E_0 = 0$. (It is of course obvious that as self-induction increases all the variable part of $C$ diminishes, leaving any constant part $\dfrac{E_0}{r}$ of more and more importance, so that, for example, self-induction in the circuit of any approximately constant current machine tends to make the current more constant, diminishing the ripples indefinitely as $L$ gets greater and greater.) If, then, $E_0 = 0$, and if we use $a_i$ to denote

$$\frac{a_i}{\sqrt{r^2 + \frac{4\,\pi^2\,i^2\,L^2}{T^2}}},$$

or

$$\frac{a_i}{r} \div \sqrt{1 + \frac{4\,\pi^2\,i^2\,L^2}{T^2\,r^2}},$$

then the values of $a_1$, $a_2$, $a_3$, &c., for various values of $\dfrac{L}{T\,r}$ are given in the following table :—

| $\frac{L}{T\,r}$ | $a_1$ | $a_2$ | $a_3$ | $a_4$ |
|---|---|---|---|---|
| 0 | $1 \times \frac{a_1}{r}$ | $1 \times \frac{a_2}{r}$ | $1 \times \frac{a_3}{r}$ | $1 \times \frac{a_4}{r}$ |
| ·02 | $\cdot 992 \times \frac{a_1}{r}$ | $\cdot 97 \times \frac{a_2}{r}$ | $\cdot 933 \times \frac{a_3}{r}$ | $\cdot 8936 \times \frac{a_4}{r}$ |
| ·1 | $\cdot 84 \times \frac{a_1}{r}$ | $\cdot 623 \times \frac{a_2}{r}$ | $\cdot 459 \times \frac{a_3}{r}$ | $\cdot 3695 \times \frac{a_4}{r}$ |
| ·2 | $\cdot 625 \times \frac{a_1}{r}$ | $\cdot 37 \times \frac{a_2}{r}$ | $\cdot 258 \times \frac{a_3}{r}$ | $\cdot 1951 \times \frac{a_4}{r}$ |
| 1·0 | $\cdot 16 \times \frac{a_1}{r}$ | $\cdot 079 \times \frac{a_2}{r}$ | $\cdot 055 \times \frac{a_3}{r}$ | $\cdot 0398 \times \frac{a_4}{r}$ |
| 10·0 | $\cdot 016 \times \frac{a_1}{r}$ | $\cdot 0079 \times \frac{a_2}{r}$ | $\cdot 0055 \times \frac{a_3}{r}$ | $\cdot 0040 \times \frac{a_4}{r}$ |

A glance at this table shows the way in which self-induction causes the overtones or ripples to diminish relatively to the fundamental tone.

For the purpose of seeing this more clearly, we will now assume that in every case the coefficient of the first term is 1; so that, calling

$$\frac{C\sqrt{r^2 + \frac{4\,\pi^2\,L^2}{T^2}}}{a_1} \text{ by the symbol } c,$$

$$c = \sum_1^\infty \frac{a_i}{a_1}\sqrt{\frac{1 + \frac{4\,\pi^2\,L^2}{T^2\,r^2}}{1 + \frac{4\,\pi^2 L^2\,i^2}{T^2\,r^2}}}\,sin.\left(\frac{2\,i\,\pi}{T}\,t - e_i - tan.^{-1}\frac{2\,\pi\,L\,i}{T\,r}\right) \tag{4}$$

of which, of course, the amplitude of the first term is 1.

If we denote the amplitudes of the various terms by $\beta_1$, $\beta_2$, $\beta_3$, &c., we have for the following values of $\frac{L}{T\,r}$ the following values of these amplitudes; we shall denote $a_i \div a_1$ by the symbol $\gamma_i$:—

| $\frac{L}{T\,r}$ | $\beta_1$ | $\beta_2$ | $\beta_3$ | $\beta_4$ | $\beta_5$ |
|---|---|---|---|---|---|
| 0 | 1 | $\gamma_2$ | $\gamma_3$ | $\gamma_4$ | $\gamma_5$ |
| ·02 | 1 | $\cdot 98\ \gamma_2$ | $\cdot 942\ \gamma_3$ | $\cdot 90\ \gamma_4$ | $\cdot 8515\ \gamma_5$ |
| ·1 | 1 | $\cdot 741\ \gamma_2$ | $\cdot 549\ \gamma_3$ | $\cdot 44\ \gamma_4$ | $\cdot 365\ \gamma_5$ |
| ·2 | 1 | $\cdot 592\ \gamma_2$ | $\cdot 411\ \gamma_3$ | $\cdot 312\ \gamma_4$ | $\cdot 2525\ \gamma_5$ |
| 1·0 | 1 | $\cdot 5000\ \gamma_2$ | $\cdot 345\ \gamma_3$ | $\cdot 25\ \gamma_4$ | $\cdot 2075\ \gamma_5$ |
| 10·0 | 1 | $\cdot 5000\ \gamma_2$ | $\cdot 333\ \gamma_3$ | $\cdot 2500\ \gamma_4$ | $\cdot 2000\ \gamma_5$ |
| 100·0 | 1 | $\cdot 5000\ \gamma_2$ | $\cdot 333\ \gamma_3$ | $\cdot 2500\ \gamma_4$ | $\cdot 2000\ \gamma_5$ |

We see that, however great $\frac{L}{T\,r}$ may be, the amplitudes of the overtones relatively to the fundamental cannot be reduced more than from

$$1 \quad \gamma_2 \quad \gamma_3 \quad \gamma_4 \quad \gamma_5 \quad \text{\&c., when } \frac{L}{T\,r} = 0 \qquad \text{to}$$

$$1 \quad \tfrac{1}{2}\gamma_2 \quad \tfrac{1}{3}\gamma_3 \quad \tfrac{1}{4}\gamma_4 \quad \tfrac{1}{5}\gamma_5 \quad \text{\&c., when } \frac{L}{T\,r} = \infty.$$

Hence, if the E.M.F. does not follow truly a sine function of the time, the current, although it may be made more nearly a sine function of the time, cannot truly become so, however great the self-induction may be.

*Example I.*

Let us take what is probably the most discontinuous periodic function which we can imagine as occurring in practice. That is,

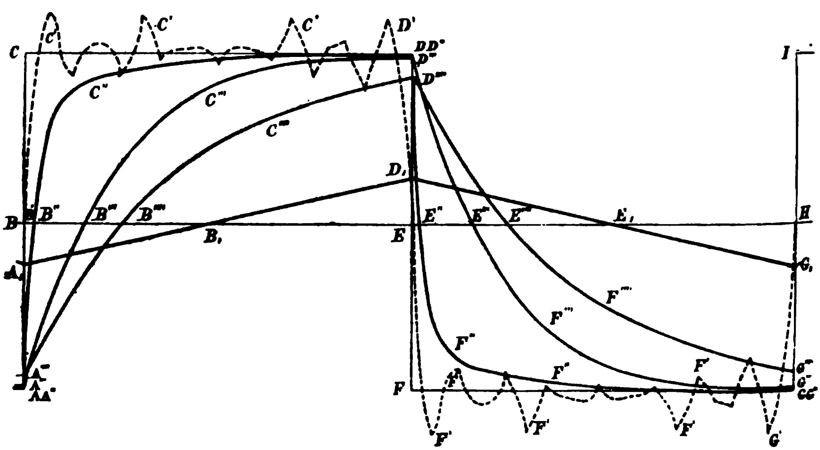



FIG. 6.

let us imagine the E.M.F. at time 0 to have suddenly increased from $-V$ to $+V$,

at time $t = \frac{T}{2}$ to suddenly diminish from $V$ to $-V$,

at time $t = T$ to suddenly increase from $-V$ to $+V$,

and so on, remaining constant except at times $0, \frac{T}{2}, T, \frac{3T}{2}, 2T$, &c. Such a variation of E.M.F. is shown by $A\,C\,D\,F\,G$ (Fig. 6).

It is easy by the use of Fourier's theorem to show that the E.M.F. satisfies the law

$$E = \frac{4\,V}{\pi}\left(\sin.\frac{2\,\pi}{T}\,t + \frac{1}{3}\,\sin.\frac{6\,\pi}{T}\,t + \frac{1}{5}\sin.\frac{10\,\pi}{T}\,t + \&c.\right) \quad (5)$$

so that the current satisfies the law

(6)
$$C = \frac{4V}{\pi\,r}\sum_{1}^{\infty}\frac{1}{i}\,\frac{1}{\sqrt{1+\frac{4\pi^2L^2i^2}{T^2\,r^2}}}\,\sin.\left(\frac{2\,i\,\pi}{T}\,t - \tan.^{-1}\frac{2\,\pi\,i\,L}{T\,r}\right);$$

care being taken that $i$ is an odd number.

The actual value of $V$ does not affect the nature of our results, so we have taken $V$ such that $\frac{4\,V}{\pi\,r} = 1.$

And if (6) is written in the shape—

$$C = \sum a_i\,\sin.\left(\frac{2\,i\,\pi}{T} - e_i\right) \quad \ldots \quad (7)$$

the following table shows the values of $a_i$ (the amplitude) and $e_i$ (the lag) of the fundamental and overtones for various values of $\frac{L}{T\,r}$:—

| $\frac{L}{T\,r}$ | $a_1$ | $e_1$ | $a_3$ | $e_3$ | $a_5$ | $e_5$ | $a_7$ | $e_7$ |
|---|---|---|---|---|---|---|---|---|
| 0 | 1 | 0 | ·333 | 0 | ·2000 | 0 | ·143 | 0 |
| ·02 | 0·99 | 7°·2 | ·311 | 21° | ·167 | 32° | ·107 | 42° |
| ·1 | 0·84 | 32°·2 | ·158 | 62°·1 | ·061 | 72°·4 | ·031 | 77°·2 |
| ·2 | 0·625 | 51°·5 | ·086 | 75°·1 | ·0316 | 81°·0 | ·0163 | 83°·5 |
| 1·0 | 0·16 | 81°·0 | ·0184 | 86°·9 | ·0066 | 88°·2 | ·0036 | 88°·6 |
| 10·0 | 0·016 | 89°·5 | ·00184 | 89°·7 | ·00066 | 89°·8 | ·00036 | 89°·9 |
| 100·0 | 0·0016 | 90° | ·000184 | 90° | ·000066 | 90° | ·000036 | 90° |

As a matter of fact, we have found it necessary to compute values of $a$ and $e$ as far as $a_{17}$ and $e_{17}$ for some of the smaller values of $\frac{L}{T\,r}$, but it is not necessary to give all such results.

It is here observable that, however great $\frac{L}{T\,r}$ may be, the amplitude of the first harmonic cannot be less than 1-9th of the fundamental sine function, nor the second 1-25th, nor the third

1-49th. We have plotted in Fig. 6 curves showing the way in which the current varies when the values of $\frac{L}{T\,r}$ are 0, ·02, ·1, ·2, 1. The distance $B\,H$ represents $T$, the periodic time.

When $\frac{L}{T\,r} = 0$ we know that the broken line $A\ B\ C\ D\ E\ F$ shows how the current ought to vary. The dotted line $A'\ B'\ C'\ D'\ E'\ F'\ G'$ shows the result of an attempt to calculate the current when $\frac{L}{T\,r} = 0$ from the formula, using, if we remember rightly, $a_{25}$ and $e_{25}$.

When $\frac{L}{T\,r} = \cdot 02$ we found that it was necessary to calculate $a_{17}$ and $e_{17}$ before we felt justified in neglecting higher terms. The current curve is shown in $A''\ B''\ C''\ D''\ E''\ F''\ G''$.

When $\frac{L}{T\,r} = \cdot 1$ the current curve, $A'''\ B'''\ C'''\ D'''\ E'''$, has departed further from the broken straight line shape.

When $\frac{L}{T\,r} = \cdot 2$ we have $A''''\ B''''\ \ldots\ G''''$.

When $\frac{L}{T\,r} = 1{\cdot}0$ we have $A_1\ B_1\ D_1\ E_1\ G_1$.

It will be found that as $\frac{L}{T\,r}$ gets greater and greater the current tends more and more to follow the law indicated by the curve—or, rather, straight line—shown in Fig. 7; and this is the

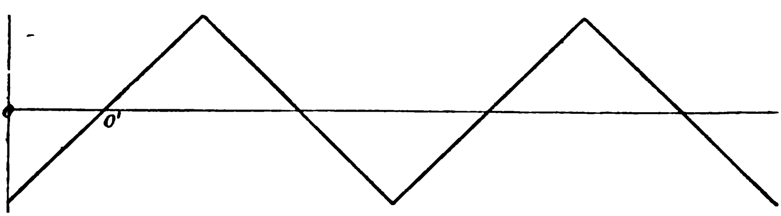



FIG. 7.

nearest approach to a sine function which the current can attain to with such a discontinuous E.M.F. as we have taken. In fact, in the limit, $C$ tends to follow the law

$$- C \propto \cos.\frac{2\,\pi}{T}\,t + \frac{1}{9}\cos.\frac{6\,\pi}{T}\,t + \frac{1}{25}\cos.\frac{10\,\pi}{T}\,t + \&c. \quad (8)$$

*Example II.*

We give another rather discontinuous periodic function as an example. We assume that the E.M.F. varies in the way shown on Fig. 7; but although this satisfies a law such as is shown in (8), we prefer to let time be measured from $O'$, and then when $V$ is the greatest + or − value of the electro-motive force,

$$E = \frac{8}{\pi^2} V \left\{ \sin. \frac{2\pi}{T} t - \frac{1}{9} \sin. \frac{6\pi}{T} t + \frac{1}{25} \sin. \frac{10\pi}{T} t \right.$$
$$\left. - \frac{1}{49} \sin. \frac{14\pi}{T} t + \&c. \right\} \quad \ldots \quad \ldots \quad (9)$$

Hence

$$C = \frac{8V}{\pi^2} \sum_{1}^{\infty} \pm \frac{1}{r\, i^2 \sqrt{1 + \frac{4\pi^2 L^2 i^2}{T^2 r^2}}} \sin. \left( \frac{2 i \pi}{T} t - \tan.^{-1} \frac{2 i \pi L}{T r} \right),$$

$i$ being odd, and the terms being alternately + and −.

For ease of calculation, taking $\frac{8V}{\pi^2 r} = 1$, and writing

$$C = \sum a_i \sin. \left( \frac{2 i \pi}{T} - e_i \right),$$

we have the following values of $a_i$ and $e_i$ for various values of $\frac{L}{T r}$:—

| $\frac{L}{Tr}$ | $a_1$ | $e_1$ | $-a_3$ | $e_3$ | $a_5$ | $e_5$ | $-a_7$ | $e_7$ |
|---|---|---|---|---|---|---|---|---|
| 0 | 1 | 0 | ·1111 | 0 | ·0400 | 0 | ·0204 | 0 |
| ·02 | ·99 | 7°·2 | ·104 | 21° | ·0388 | 32° | ·0153 | 42° |
| ·1 | ·84 | 32°·2 | ·051 | 62°·1 | ·012 | 72°·4 | ·0045 | 77°·2 |
| ·2 | ·625 | 51°·5 | ·0285 | 75°·1 | ·0063 | 81°·0 | ·0023 | 83°·5 |
| 1·0 | ·16 | 81°·0 | ·00613 | 86°·9 | ·00133 | 88°·2 | ·00051 | 88°·6 |
| 10·0 | ·016 | 89°·5 | ·00061 | 89°·7 | ·00013 | 89°·8 | ·00005 | 89°·9 |
| 100·0 | ·0016 | 90° | ·000061 | 90° | ·000013 | 90° | ·000005 | 90° |

We have drawn the curves representing $C$ for the values of $\frac{L}{T r} = 0, 0{\cdot}01$, and $0{\cdot}1$ in Fig. 8, the distance $A\,E$ representing $T$, the periodic time.

$A\ B\ C\ D\ E$ shows current when $\dfrac{L}{T\,r} = 0$;

$A'\ B'\ C'\ D'\ E'$ „ „ „ $\dfrac{L}{T\,r} = \cdot 1$;

$A''\ B''\ C''\ D''\ E''$ „ „ „ $\dfrac{L}{T\,r} = 1$.

The ordinates of this last curve (for $\dfrac{L}{T\,r} = 1$) have been magnified ten times.

It is evident that even when $E$ is such a very discontinuous

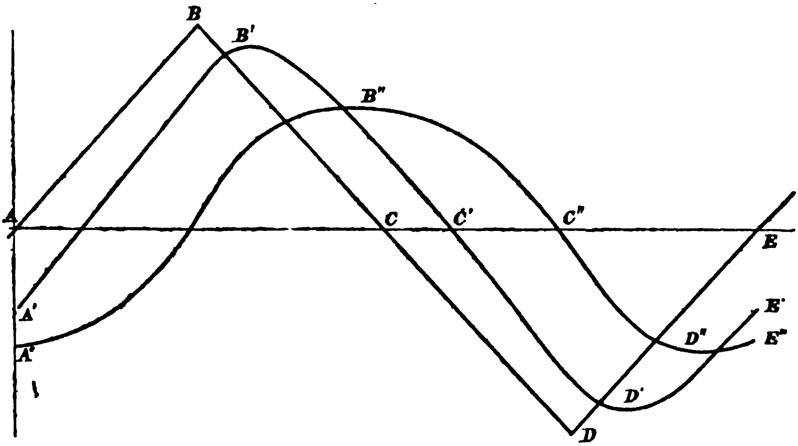



FIG. 8.

function as this, a moderate amount of self-induction makes the current what is very nearly a pure sine function of the time.

It is, however, obvious that, however great $\dfrac{L}{T\,r}$ may be, it is impossible to have the overtones smaller, in proportion to the fundamental, than we have in

$$C = \frac{4\,V\,T}{\pi^3\,L}\left\{\cos.\frac{2\,\pi}{T}\,t - \frac{1}{27}\cos.\frac{6\,\pi}{T}\,t + \frac{1}{125}\cos.\frac{10\,\pi}{T}\,t - \&c.\right\}$$

## ELECTRIC FREQUENCY METER.

Some time ago one of us suggested, during a discussion on transformers, that a method might be used for analysing an alternate-current wave which somewhat resembled the employment of Helmholtz's resonators for analysing a complex sound vibration.

The method then proposed consisted in sending through one of the coils of a dynamometer the current to be analysed, and through the other an alternate current of known frequency, and varying the frequency of the test current until an attraction was observed between the two coils. The smallest frequency of the test current that will produce such an attraction gives the fundamental rate of alternation of the current which is being analysed, and the magnitude of the attraction between the two coils of the dynamometer the amplitude of the fundamental rate of alternation. The frequency of the test current is then gradually increased until an attraction is again observed between the dynamometer coils, from which the frequency and amplitude of the first higher rate of alternation is determined, and so on for all the component vibrations.

This method has been used with a certain amount of success by Messrs. Lamb and E. W. Smith at the Central Institution for the analysis of the current produced by the Ferranti machine. Previous experiments had shown us that this current was not a simple sine function of the time, as the curve rises less rapidly and falls less rapidly than a true sine curve. The difference, however, between the two is not very great, and the unimportance of the harmonics compared with the fundamental rate of vibration in the current curve of the Ferranti machine renders the analysis of this current a severe test of the method, which, however, was sufficiently sensitive to show that the Ferranti current wave consists of a fundamental rate of alternation, no appreciable components either for twice or three times the fundamental rate of alternation, but sensible components having respectively four and five times the rate of alternation of the fundamental; that is, the alternation is made up of a fundamental and the third and fourth harmonic.

Quite recently two of our students—Messrs. Healing and Le Tall—have been experimenting on another resonance method of analysing an alternate-current vibration which we have suggested to them, and, considering how rough is the apparatus they have been using, and how short a time they have been engaged on this new method, the results are unexpectedly

satisfactory, and appear to show that this method, which is extremely simple, is one of great promise. The alternate current to be analysed is passed through a stretched wire, *V V* (Fig. 9), the length or tension of which can be varied as in a monochord.

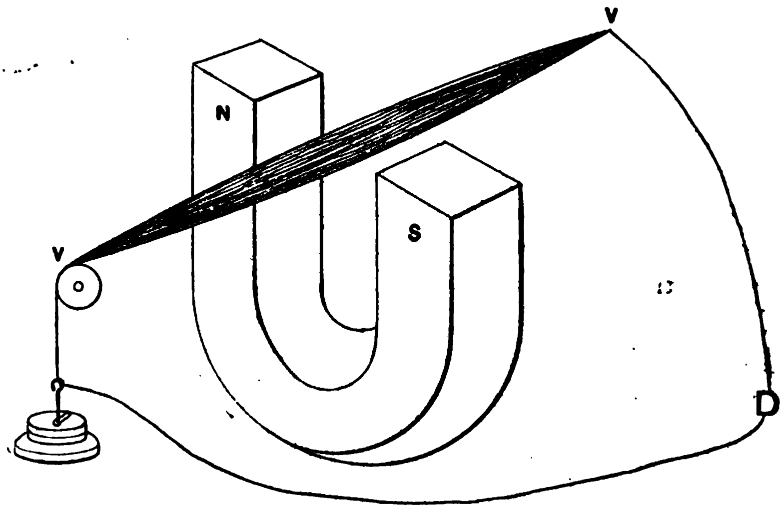



Fig. 9.

*N S*, magnet producing permanent field. *V V*, wire conveying alternate current, and tuned so that its natural period of vibration agrees with that of the alternate current. *D*, alternate-current dynamo.

This wire is in a magnetic field produced by a permanent magnet, *N S*, or by an electro-magnet, or, best of all, by a coil of wire surrounding the wire *V V* as a flat galvanometer coil surrounds the needle. As the current in the wire alternates the wire receives impulses backwards and forwards across the lines of force produced by the magnetic field, and if the length and tension of the wire be adjusted by trial until the natural time of vibration of the wire agrees with the periodic time of variation of the alternate-current, the wire oscillates vigorously with an amplitude from 1½ to 2 inches, emitting, of course, a musical note. The sudden way in which the wire starts vibrating when its length and tension are just right is very striking. The frequency can be determined from the musical note emitted by the wire; but as that would require tuning-forks or some other apparatus having

fixed rates of vibration, as well as a musical ear, we prefer to determine the frequency by using the well-known formula for a vibrating string, viz.,

$$n = \frac{1}{2\,l}\sqrt{\frac{t}{m}},$$

where $n$ is the frequency, $l$ the length of the wire in centimetres, $t$ the tension in dynes, and $m$ the mass in grammes per centimetre of the wire.

In the first experiments we commenced with the wire comparatively slack, and on gradually tightening it up it was seen to vibrate very slowly. Holding one's finger on the wire and counting the vibrations, they were found to correspond with the speed of the engine driving the alternate-current dynamo, $D$, which was sending the alternate current through the wire. On tightening up the wire a little more it responded to twice this rate of vibration, which may, perhaps, have corresponded with the number of piston strokes; then the wire required a good deal of tightening, when it suddenly burst into vibration, and on calculating the value of $n$ from the formula just given, a result was obtained closely agreeing with the electric frequency, which, with the particular dynamo employed, was eight times the number of rotations of the armature per second. On tightening the wire still more, shriller notes were heard, and smaller waves seen superadded on the fundamental vibration of the wire as a whole. We were at first afraid that the analysis of the harmonics of the alternate current would be troubled by the vibrating wire having harmonics of its own, but we were reminded by Mr. Lamb of the well-known fact—which we ought to have remembered—that the harmonics possessed by the vibrating string of a musical instrument are impressed on it in the act of setting the string in vibration, or, in other words, that a string has no harmonics of its own. Hence we may safely conclude that the higher rates of vibration seen and heard with our wire are really due to harmonics in the alternate-current wave itself. To determine the frequency and amplitude of each of these harmonics the length and tension of the wire are adjusted until its natural

period of vibration agrees with the particular harmonic sought for in the alternate current, and which agreement is evidenced by the wire vibrating vigorously when it is attained.

We have not yet gone beyond this point in our experiments, but some modifications and developments of the method that have occurred to us may be briefly referred to. Instead of tuning the wire to accord successively with each of the harmonics sought for, it may be found convenient to have a number of wires tuned to give out, and therefore to respond to, different notes, as in a harp, and placed in a magnetic field, then to send the alternate current to be analysed through them in parallel or in series (probably the latter would be better), and observe which of the wires vibrate, and what are the various amplitudes of vibration. Or, instead of sending the alternate current through the stretched wire or wires, it may be sent round a solenoid or electro-magnet, and a direct current sent through the stretched wire or wires, placed relatively to the solenoid so that a current in the wire or wires is deflected by a current passing round the solenoid. Or, in place of a vibrating wire, a magnetic tongue the natural rate of vibration of which can be varied by altering its length or its moment of inertia, or in some other convenient way, may be placed near a solenoid or electro-magnet round which flows an alternate current. Or a set of such tongues tuned like the vibrating springs of a mouth organ may be employed to analyse an alternate current sent round a solenoid or electro-magnet.

In the figure the alternate-current dynamo is shown near the vibrating wire, but it will be obvious that any one of these arrangements may be employed to measure the frequency of an alternate current at *any distance* from the dynamo. Or they may be used simply to measure the speed of the dynamo at any point of the circuit through which flows the alternate current produced by the dynamo, if the number of alternations per revolution—which is, of course, a definite number for each dynamo—be known. Or, generally, if there attached to any moving mechanism some arrangement for making and breaking a current, or varying the strength of a current, any one of these methods may be used at any

distance from the moving mechanism to measure the frequency, and, therefore, the speed at which the mechanism is moving. So that the arrangement can be used as a simple speed-indicator for indicating at a distance the speed of moving machinery.

If the vibrating wire consist of two or more wires mechanically joined together, but electrically insulated from one another so that different currents can be passed through them, then, if the currents be of the same frequency, we can use our method for comparing the mean value of the algebraic sum of the currents with the mean value any of one of them; and by comparing the mean value thus found of the algebraic sum of two currents with what the mean value would be were there no difference in phase, we can determine this difference in phase. And the same thing may be done by sending the currents round different wires on a solenoid placed so as to cause a magnetic tongue to vibrate.

In fact, we have ourselves hardly yet realised all the uses to which this "electric frequency meter" can be put.

The CHAIRMAN: After the expression of your feelings which you have just shown, I need hardly ask you to accord a hearty vote of thanks to Professors Ayrton and Perry for having given us these interesting notes at such a short notice. I would now call upon any members for their remarks, and would especially point out to the students who have attended Professor Ayrton's lectures at various times that it is now the time to turn round on him. Mr. Siemens.

Professor W. E. AYRTON: Perhaps the invitation might equally apply to those who *were* students as well as to those who are.

The CHAIRMAN: The subject of self-induction is such an important one at the present time that it would be very desirable to hear the opinions of many who are present.

Mr. W. P. GRANVILLE: Although not a student under Professor Ayrton in the ordinary sense, I have certainly been one to-night, and for the information gained I very much thank him. There is one point I wish to say a word upon, and that is in reference to the fact pointed out by Professor Ayrton, Mr. Granville.

Mr. Granville.

that the self-induction of a coil of wire is not increased by the insertion of an iron core *if the alternations of current are exceedingly rapid*, and that if the alternations are further increased the self-induction is even diminished. You will probably remember that Mr. Willoughby Smith, some five or six years ago, gave an experimentally illustrated paper on induction. With those experiments I was to a certain extent connected as his assistant, and on that occasion a commutator was used very much of the same form as that adopted by Professor Ayrton in his secohmmeter, viz., a double commutator mounted on one spindle, one part of the commutator reversing the battery, and the other part simultaneously reversing the galvanometer, so as to produce a steady deflection. In one experiment shown by Mr. Willoughby Smith, two flat spirals were placed about a foot apart, and by means of the double commutator an alternating current from a battery was sent into one of the spirals, and the other spiral was connected through the commutator to a galvanometer, so that the induction received by the second spiral was indicated by a steady deflection. It was then found that by interposing plates of different non-magnetic metals certain proportions of the current were cut off, but that when an iron plate was interposed, the proportion intercepted did not increase with the speed, as was the case with other metals, but remained almost constant, and even slightly diminished, when the rate of alternation was very rapid; and I think this is the same result that Professor Ayrton has so ably explained to us to-night.

Professor Ayrton.

Professor W. E. Ayrton: I am very much interested to hear the remarks made by Mr. Granville. I can only say that, notwithstanding I heard Mr. Willoughby Smith's paper, I was totally unaware of such a method having been employed; and I am quite sure also that Messieurs Ledeboer and Manœuvrier, who were engaged at the same time as Professor Perry and myself in developing easy methods for the *absolute* measurement of the coefficients of self-induction two years ago, were equally unaware that Mr. Willoughby Smith had employed a somewhat similar commutating device. Although it is not of very much conse-

quence, I may say that we do not reverse the galvanometer at the moment that we reverse the battery; that would not answer our purpose. The galvanometer is, in this particular apparatus, reversed midway between the reversal of the battery, because we want——I need not go into that now; but if you observe these commutators afterwards, you will see that there is that marked difference. I do not mean to say that that would make any radical distinction between the pieces of apparatus. That was mentioned, I suppose, in the paper? Professor Ayrton.

Mr. W. P. GRANVILLE: Yes; and also in the pamphlet called "Induction."

Professor W. E. AYRTON: I am very glad to hear it now, and I am astonished at my own ignorance.

Mr. W. P. GRANVILLE: There is a sketch in the pamphlet.

Professor W. E. AYRTON: That makes it more disgraceful still. I presume, however, that Mr. Willoughby Smith did not use his arrangement for the *absolute* measurement of the coefficients of self and mutual induction, which is the essence of our device.

Mr. W. B. ESSON: May I ask Professor Ayrton whether the curve he gives of the Ferranti machine represents the electromotive force induced, or merely the flux of lines through the armature for different positions of its coils? Mr. Esson.

Professor W. E. AYRTON: You are speaking of *this* curve: *this* is the actual electro-motive force of the machine; *that* is the self-induction. Professor Ayrton.

Mr. W. B. ESSON: Did I understand Professor Ayrton to say that in the Ferranti machine he found a wave having double the frequency superposed on the principal wave? Mr. Esson.

Professor W. E. AYRTON: No; four or five times. Not double or treble, but at four or five times the frequency. Professor Ayrton.

Mr. W. B. ESSON: I suppose the effect in a Ferranti machine which has no iron in its armature is not sufficiently marked, but there is a wave, due to the weakening of the field by the armature current, which has twice the frequency of the principal wave. Mr. Esson.

The CHAIRMAN: Gentlemen, you have already accorded your vote of thanks to Professors Ayrton and Perry, so I will just say

that Mr. Preece was so overcome by the sudden and sad news to which I had to allude at the beginning of our proceedings, that he felt he could not attend this meeting; I will therefore call upon the Secretary to read his communication, which is a very short one.

The SECRETARY then read the following paper:—

# ON THE DISTURBANCES ARISING FROM THE USE OF "EARTH" FOR ELECTRIC LIGHTING PURPOSES.

By W. H. PREECE, F.R.S., Past-President.

The magnetic field produced by a current flowing in a straight wire, and returning through the earth, extends to such distances that it is quite impossible to say where or when or how a telephone circuit in that field would be disturbed by any changes in that current. It is quite certain that if a single conductor between Deptford and London were subject to rapid alternations under a potential difference of 10,000 volts—the current returning by way of the earth—every telephone circuit in the metropolis would be disturbed, and probably rendered unworkable. The law determining the distance to which this influence extends was given by me in a paper read before the British Association at Manchester in 1887. It is given in the following equation:—

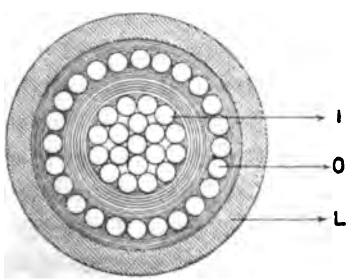
$$C_2 = \frac{C_1 \, l}{d^2 \, r_2} \times M;$$

$C_1$ being the primary currents, $C_2$ the secondary currents, $l$ the length of the primary, $d$ the distance between the wires, $r_2$ the resistance of the secondary, M a constant dependent on the frequency of the primary currents and the rate at which they rise and fall.

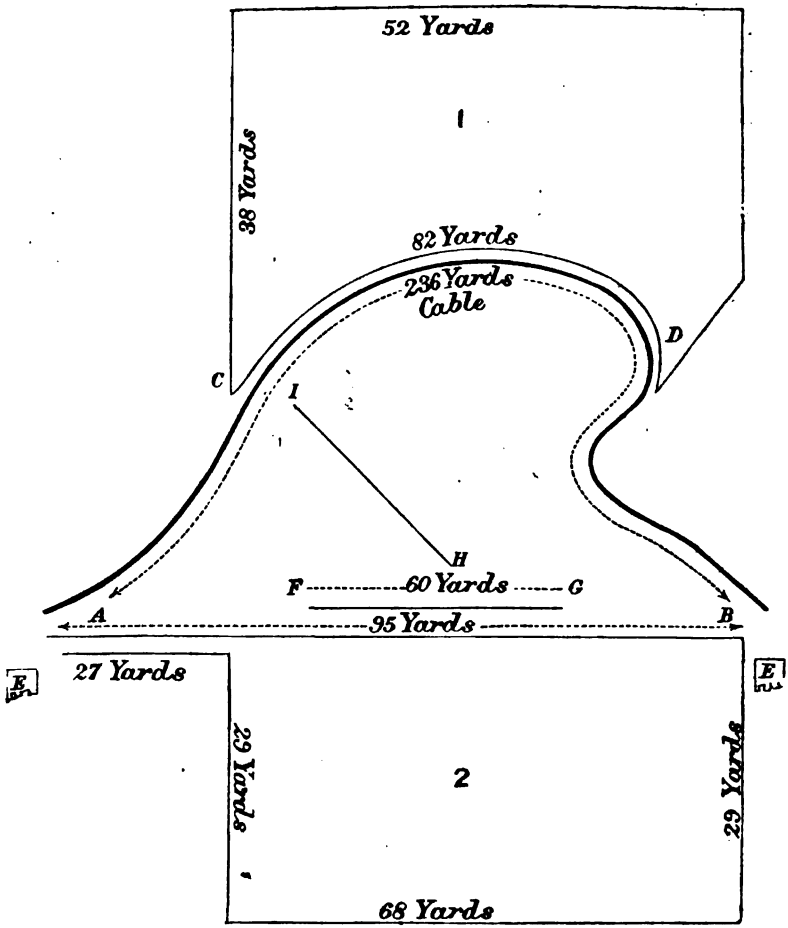
It follows from this formula that currents of rapid frequency on a conductor 10 miles in length, carrying 100 amperes, might be evident at a distance of 19 miles. It becomes, therefore, important to decide to what extent disturbing influences can be eliminated. It is well known that if either the disturbing circuit or

that disturbed be constructed of two wires carrying equal currents, so close to each other and so erected that the mean average distance between the disturber and the disturbed is the same, no inductive effects are evident, for the influence on one wire is entirely neutralised by the equal and opposite effect on the other. But if this mean average distance is not equal, or if the equality of the two currents be departed from, then a disturbance will arise which will depend on the difference between the two currents and their relative distances. Hence the most perfectly silent telephone circuit becomes noisy when leaks occur in the insulation, and hence it is that the earth plays such an important part in the efficiency of the system.

It was reported in the technical papers that the London Electric Supply Corporation contemplated laying a concentric conductor between Deptford and London, with the external copper conductor in contact with the earth. As it seemed to the Post Office authorities very doubtful whether such an arrangement would not create disturbance on the telegraph and telephone circuits, experiments were made to test the point. The experiments were made by me in an open space of two or three acres in extent. A concentric cable was constructed so that the outside and enveloping conductor could be insulated or not. The inner conductor was composed of a strand of nineteen No. 15 B.W.G. (72 mils.) copper wires. The outer conductor completely surrounded the inner, and consisted of twenty-four No. 16 B.W.G. (65 mils.) copper wires. Each conductor weighed about 1,600 lbs. per mile, and gave a resistance of ·56 ohm. The outside sheath was of lead. The following is a full-size section of the cable :—



The following diagram will explain the arrangement of the wires:—




Two earth-plates were buried at the points A and B, and between them a No. 7 gutta-percha wire was laid along the ground, telephones being inserted at each end. The electric light cable was laid between the same points by a circuitous route, as shown. At the points C and D two earth-rods were driven in, and another No. 7 gutta-percha wire was laid outside the area enclosed by, and about a foot from, the cable.

Beyond each of the gutta-percha wires, extension wires were

carried to enable metallic loops to be made up, forming approximate squares (1 and 2 on diagram).

No. of Experiment.

1. A Wheatstone transmitter placed at B was connected to the inner conductor (I) of the cable, the lead sheath (L) being used as the return wire. Reversals at top speed (a frequency of 80 per second) were sent through the cable from an E.M.F. of 20 volts, the current strength being about 1·3 amperes. Loud disturbance was heard in the telephones at A and B when the transmitter was running.

2. Earth-bars were driven in at the points F and G, and connected by a wire and telephone. Currents were picked up by the telephone through this section, their effect being apparently quite as loud as between A and B.

3. The points H and I were similarly connected, and with a like result.

4. The points C and D were next connected by the wire attached to the earth-bars, a telephone being inserted at D. Disturbances almost as loud as on the circuit A B were heard.

5. The wire C D was disconnected from the earth-bars, and a metallic circuit forming an approximate square was made up (1 on diagram). The interference was still heard, but was only about half as loud as when the earth was used at C D.

6. The insulated outer *conductor* (O) was substituted in place of the lead sheath as the return wire, and the disturbances in the loop C D ceased entirely.

7. The outer conductor (O) and the lead sheath (L) were coupled together, forming jointly a return wire in contact with the earth. The disturbance in the metallic square 1 reappeared, but was reduced in loudness to about half that in the fourth experiment.

8. The cable was next laid out straight from A to B, the telephone wire being parallel to it. When the lead sheath was used as the return wire, loud disturbance was heard in the telephones at A and B. When the lead sheath and outer conductor were jointly used as the return wire, the noise in the telephones was greatly reduced (to about one-fourth).

9. To prove that the result obtained in the last experiment was due to earth conduction, a metallic square (2 in diagram) was formed, and the disturbance in the telephones disappeared.

It follows from these experiments that the disturbance of the potential of the earth at A and B, and the establishment between these two points of a potential difference, cause currents to flow through the earth between A and B which destroy the equality between those in the inner and outer conductor of the concentric cable upon which equilibrium depends. It is therefore quite clear that the use of the external sheathing as part of the conductor in contact with the earth is quite out of the question; whereas if the enveloping conductor be insulated it is quite innocent of disturbance, and would not interfere with neighbouring telephones.

The question has arisen as to whether the currents experimented with (viz., those produced by a Wheatstone transmitter) are of the same character as those produced by an alternating-current machine. The frequency in the two cases may be alike. Taking the frequency at 100, it will follow that each current, whether it be of the Wheatstone transmitter or of the alternating-current machine, will rise from zero to maximum and fall from maximum to zero in ·0025 second. The strength of current generally used in the Wheatstone transmitter is 50 milli-amperes, while that of an alternating-current machine may amount to 100 amperes; and since most of the Wheatstone circuits in this country are long circuits, upon which the resistance and the capacity are considerable, it is quite evident that in actual practice the rate at which the current rises and falls is greater in the case of alternating-current machines than in the case of the Wheatstone transmitter. The fact that it is so is sufficiently proved in actual practice. Before the Grosvenor Gallery Company established their present metallic loop system, all the telephones around and about Bond Street were disturbed. In order, however, to settle the matter, I have carefully experimented with the currents produced by the alternate-current machines at West Brompton and at Eastbourne, and I find that it is quite impossible to detect any difference

between the effects of Wheatstone transmitters and alternating-current dynamos.

These experiments, taken alone, are sufficiently definite to render it decisive that the use of the earth in such instances cannot be allowed; but it may happen that in other systems of distribution where the currents are continuous and steady, such as the systems by which it is proposed to distribute by means of batteries, the earth under exceptional circumstances may be permitted.

Mr. W. M. MORDEY: Mr. Preece refers to the current as being at a tension of 10,000 volts. I would ask whether there is any direct or necessary connection between the tension and the leakage in this case. The only leakage is from one part of the outer conductor to another part of it through the earth, and the greatest possible tension, therefore, applied to the earth must be that due to the loss of pressure—the fall of potential in the outer lead. This may be only a few volts, although the actual pressure used in the station may be 10,000 volts. I am not concerned as an apologist for the Deptford scheme, but I do not quite see how Mr. Preece connects the very high tension which is proposed to be used there, with earth leakage. With the same loss of pressure in volts the leakage would be the same, even if the terminal potential difference were only a fraction of that stated. Mr. Mordey.

Professor W. E. AYRTON: I do not suppose—at least, I hope—that Mr. Preece does not intend by earth leakage to mean what is usually understood by earth leakage. The fact, of course, is, as indeed I happened to point out—I think it was in 1878—that if you have a varying current in a wire, and you use an *un*insulated tube as the return, the uninsulated tube does not shield outside space from inductive action; because it is only when the tube surrounding the wire takes back the *whole* of the current that goes by the interior core, that the outside space is entirely shielded from magnetic action. The actions that Mr. Preece gets are probably not due to earth leakage in the ordinary sense of the word at all, but they are due to the fact that when the return is made by the *un*insulated lead covering of his cable the whole earth is part, or, at any rate, the earth in the neighbourhood is Professor Ayrton.

Professor Ayrton.

part, of the return wire; hence a portion of the return electric light current comes outside the telephone circuit, and you have a fluctuating number of lines of force passing through the telephone circuit, due to the alternating current. If, however, you are dealing with a direct current where there are no fluctuations at all, then I doubt very much if there will be any difference or any disturbing effect worth speaking of on the telephone circuit, whether the outside covering be put to earth or not. Therefore I think that the sort of permissive sentence that occurs at the end of Mr. Preece's paper (I am sorry that he is not here to answer for himself) is somewhat misleading. He says: "But "it may happen that in other systems of distribution where the "currents are continuous and steady, such as the systems by "which it is proposed to distribute by means of batteries, the "earth, under exceptional circumstances, may be permitted." I should say that the earth might under any circumstances be permitted if the current in the circuit were quite steady; because, as the disturbance in the telephone lines is due to electro-magnetic induction, and as electro-magnetic induction cannot be produced with a perfectly steady current, it is of little consequence whether the electric light return be earthed or not.

I was under the impression that that was made quite clear eleven years ago, when this very same subject was under discussion. The question arose, I think, at the time the Jablochkoff Company lighted the Thames Embankment, when it was pointed out—if I mistake not, by Mr. Preece—that although the return conductor of their alternating-current system surrounded the going conductor, there was inductive effect on neighbouring telegraph wires buried in the ground near; and I then pointed out that this disturbance arose from the tube which brought back the current not being insulated, for, in order to get perfect screening from induction, it was necessary that the return tube should be entirely insulated.

Mr. Crompton.

Mr. R. E. Crompton: In reference to Professor Ayrton's remark that currents which are represented to be perfectly steady have no effect on telephones———

Professor Ayrton: I said currents that were steady, not unsteady.

Mr. R. E. CROMPTON: I mean currents as steady as are produced by the ordinary direct-current machines, driven by any form of steam engine or other motor that I am acquainted with. I have noticed that such currents always do produce slight sounds in the telephone. This I attribute to the irregularities or pulsations in the speed of the motor that drives the dynamo machine. In one case a telephone wire was laid in the same trench with electric light cables. These were well insulated and lead-covered. The telephone wire was spaced a few inches distant from them. The return of the telephone was by earth. In this case it was found impossible, even when several sets of dynamo machines were working in parallel, to avoid considerable disturbance in the telephone circuit. In fact, in this case the telephone acted as a very delicate and useful detector. When a single dynamo was at work the revolutions of the engine could be distinctly counted. Further than this, we noticed that a telephone wire thus laid becomes a most valuable detector of leakage between the armature winding and earth, for such leakage, being of an intermittent nature, is noticed at once by the alternations corresponding with the number of revolutions. In one case an accidental short-circuit in the armature produced a slight alternating current, which was sufficient to ring the telephone bells. It is easy to see that this application of the telephone may be of great use in central station work. Mr. Crompton.

With regard to the inductive disturbances from the outside conductor referred to in Mr. Preece's paper, I think that the area within which those disturbances would be felt must be comparatively small in the case of a straight line of conductors, and would, in fact, only be that which bounds the portion of earth carrying an appreciable quantity of the current. Outside this portion of earth there would be no magnetic disturbance, as it would be balanced by the presence of the opposite exciting current in the inner conductor.

On the other hand, if the line were laid in a curved direction, the whole space included within it would be out of balance, considered magnetically, and inductive disturbances would take place therein.

Mr. Esson. Mr. W. B. Esson: May I ask Mr. Crompton whether the machines he spoke of were Brush machines?

Mr. Crompton. Mr. R. E. Crompton: No; they were machines of our own manufacture, and were perfect machines.

Mr. Esson. Mr. W. B. Esson: I mention this because it does not follow that a current flowing always in one direction is a *continuous* current—it may flow in the same direction while very *discontinuous*. I should think Brush machines would always produce a very considerable effect on adjacent telephone circuits.

Mr. Crompton. Mr. R. E. Crompton: We have not yet got a steam engine that drives at a continuous speed, and consequently the continuous current sent is not continuous in any machine that I am acquainted with.

Professor Ayrton. Professor W. E. Ayrton: I may just remark that the last point in Mr. Preece's paper had reference to batteries, and not to machines at all; "distribution by means of batteries," is the last sentence in the paper. I am perfectly prepared to admit that there are certain machines which are very discontinuous; and, indeed, it may possibly be in the memory of some people that Professor Perry and I proposed, several years ago, a discontinuity meter, as I think it was called. The apparatus was simply an induction coil through one circuit of which the so-called continuous current was sent; in the secondary circuit of this induction coil was placed a telephone, or a dynamometer, or other suitable instrument for measuring the alternate current induced in consequence of want of perfect constancy of the primary current. Experiments were made, I think in 1879, and we showed that with the Brush machine there was a most loud sound; in fact, the Brush machine, sending a so-called steady current through the primary coil of the induction coil, gave a steady deflection with a Siemens dynamometer attached to the secondary. Of course such an induced current could only have been obtained if there were considerable variation of the current in the primary coil. Sound could also be heard with the Edison machine, but no decided deflection was obtained with it, as far as I remember, with the dynamometer of our discontinuity meter. But the point I was speaking about was the distribution

| | | | | | | | |
|---|---|---|---|---|---|---|---|
| [illegible] ... | £112 | 15 | [illegible] | | | | |
| Book-Cases presented by Sir David Salomons ... ... ... ... | 30 | 0 | 0 | | | | |
| Cost of Book-Cases and Carpet purchased, as above ... ... | 56 | 2 | 6 | | | | |
| | 198 | 17 | 6 | | | | |
| Less depreciation on £112 15s., say ... | 11 | 5 | 0 | | | | |
| | | | | 187 | 12 | 6 | |
| Overdue Subscriptions estimated to realise ... ... | | | | 205 | 0 | 0 | |
| Stock in hand of Society's Journals and Ronalds' Catalogue, estimated value ... ... ... | | | | 825 | 0 | 0 | |
| Books, Pictures, &c. (other than the Ronalds' Library), | | | | | | | |
| As per last Balance-Sheet ... | £692 | 0 | 0 | | | | |
| Add value of Books, &c., since purchased and presented ... | 40 | 0 | 0 | | | | |
| | | | | 732 | 0 | 0 | |
| Amount due for Advertisements in Society's Journal, 1888 ... ... ... ... ... ... ... | | | | 38 | 10 | 0 | |
| Cash at Bankers, as shown above ... ... ... | | | | 393 | 19 | 8 | |
| | | | | £4,385 | 12 | 5 | £4,385 12 5 |

We certify that we have examined the Books, Vouchers, and Securities of the Society, and that the above Statement of Receipts and Expenditure and Estimate of Assets and Liabilities are correct, and exhibit the true financial state and condition of the Society.—

J. WAGSTAFF BLUNDELL
(WAGSTAFF BLUNDELL, BIGGS, & Co., Chartered Accountants),
FRED. CHAS. DANVERS,
} *Auditors.*

* These amounts are now invested.

Secretary, *March 25th*, 1889.

by means of batteries. If the distribution is by means of batteries, which are not machines of this discontinuous nature, then I doubt whether you might not put the return to earth without disturbing telephone circuits. Professor Ayrton.

The CHAIRMAN: I will now ask you to accord to Mr. Preece a hearty vote of thanks for bringing this interesting subject forward; and I can only hope on my own part that we shall have an early opportunity of discussing the subject again, on an evening when he can be present, and when a little more time is at the disposal of the meeting than has been available to-night. Mr. Siemens.

The motion was unanimously carried.

A ballot took place, at which the following candidates were elected:—

*Members:*

| | |
|---|---|
| E. A. Kenyon. | Edward Manville. |

*Associates:*

| | |
|---|---|
| Richard Charles Bennett. | G. Mahon. |
| Frank Broadbent. | Allan Plucknett. |
| J. W. Howard. | Lionel Hugh Kenmore Stotherd. |

*Students:*

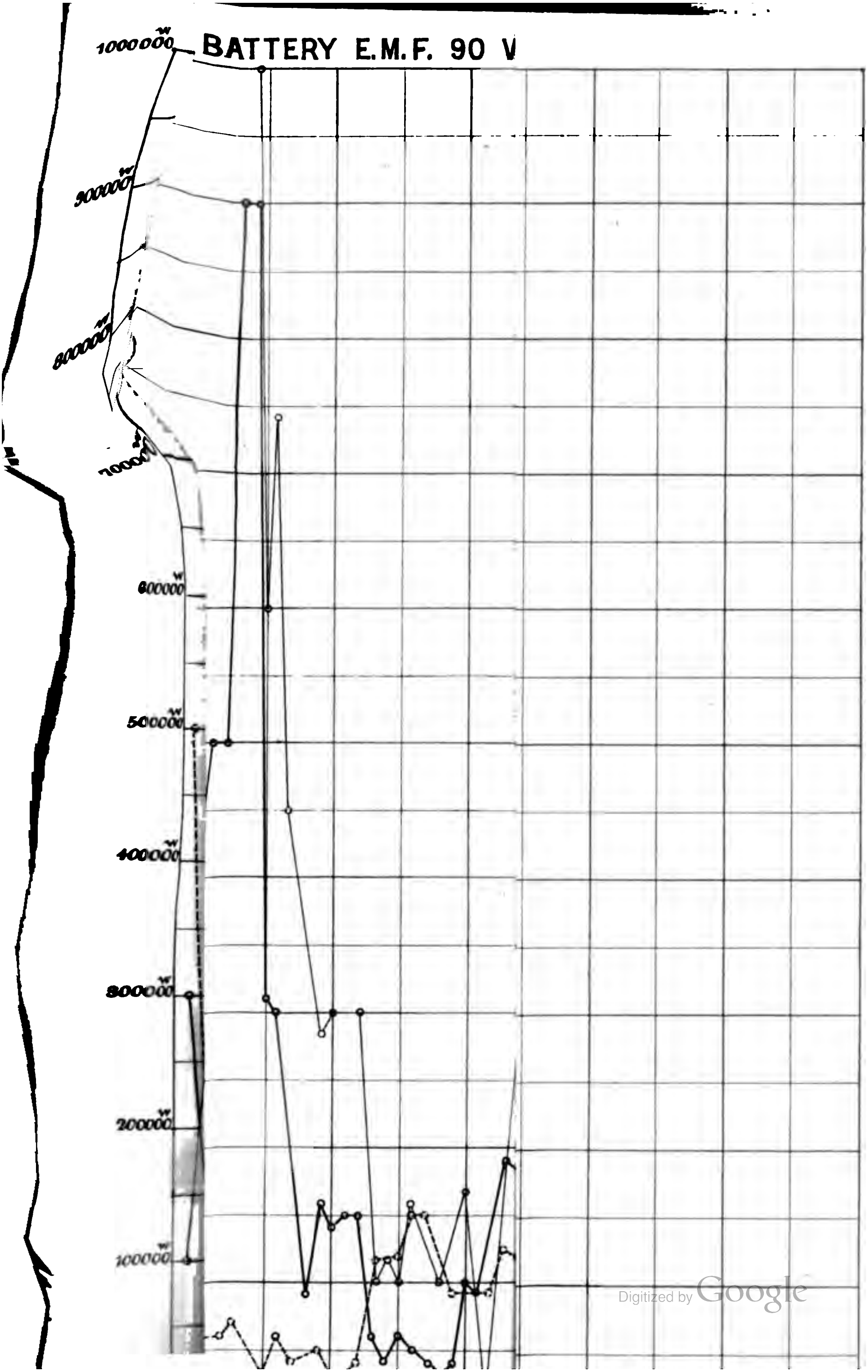
| | |
|---|---|
| Archibald John French. | Archer Turner. |

The meeting then adjourned.

# ORIGINAL COMMUNICATIONS.

7, Westminster Chambers, Victoria Street, S.W.,
London, 11th *April*, 1889.

Dear Sir,—Referring to the discussion on Professor Jamieson's paper on "Insulation Resistance," &c., and in accordance with the suggestion of Mr. Preece that the result of actual tests should be forwarded to you, I now hand you a diagram of tests taken at Hastings. The circumstances under which the results were obtained of course vary very considerably, but in the majority of cases it may be taken that all exposed portions of the circuit would have a coating of salt, which rendered the insulation peculiarly liable to be affected by slight moisture in the atmosphere. The differently drawn lines show separate circuits, and it may be taken that each circuit had approximately the same liability to leakage, as on each there were arc lamps and incandescents. Most arc lamps were outside, exposed to the weather; the incandescents being almost wholly within doors. The cables were 7/16 copper strand, covered with a double coating of gutta-percha, and braided. The total length of circuits at this time was, roughly, 8,900 yards, all laid underground in 4-in. cast-iron pipes. Test boxes every 100 yards. Pipes laid in roadway about 15 in. from surface. An attempt was made to keep all water out, but it was afterwards found that the liability to leakage was not thereby appreciably reduced. Tests were taken twice a day with a Wheatstone bridge and mirror galvanometer, the battery giving an E.M.F. of about 90 volts. The great source of breakdowns was not, as might have been supposed, the joints, but where the cable had received slight injury in pulling into the tubes. As skilled labour was at that time and place not obtainable, this is hardly to be wondered at. It was found that some few of the joints did go, but this arose from want of care principally. I do


BATTERY E.M.F. 90 V
1000000 W
900000 W
800000 W
700000 W
600000 W
500000 W
400000 W
300000 W
200000 W
100000 W

not remember, after the first few months' working, any joint proving bad, except those made at the start. The E.M.F. used was about 2,000 volts.

Yours faithfully,

F. B. NICHOLSON, Associate.

The Secretary,
Institution of Electrical Engineers.

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7, Westminster Chambers, Victoria Street, S.W.,
London, 26*th April*, 1889.

Dear Sir,—In further reference to my letter on the subject of insulation tests at Hastings, I beg to hand you a few figures relating to cost of same, for use as you think fit.

Yours faithfully,

F. B. NICHOLSON.

Secretary,
Institution of Electrical Engineers.

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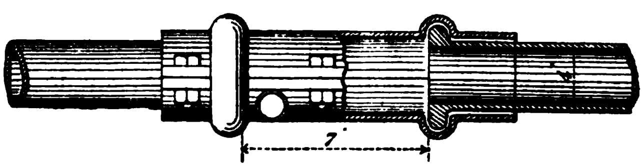
## NOTES ON THE COST OF LAYING PIPES FOR UNDERGROUND MAINS.

### Hastings, 1883 and 1884.

*Pipes.*—4 in. internal diameter, and ½ in. thick; length, 9 ft. weight of each pipe, 156 lbs. Joints made with a clip thus,




in two halves. In lower half of clip a boss cast and screwed, with a thin casting of iron filling up; in order to take off a branch to a house, all that is necessary is to break in this with a chisel and screw in 1¼-in. gas barrel. Each clip weighing 40 lbs., and giving an additional 7 in. to pipe, so that length between centres of joints = 9 ft. 7 in. Test boxes put in about 100 yards apart, nearer on curves. All pipes laid straight.



*Wires.*—7/16 cables, covered with gutta-percha in two layers

and heavily braided. Pulled in pipes by wires laid in for the purpose; wire on drum, and guided by rollers.

*Costs*—

| | Per yard. s. | d. |
|---|---|---|
| Pipes, clips, and bolts ... ... | 4 | $1\frac{3}{4}$ |
| Wages ... ... ... ... | 0 | $11\frac{1}{4}$ |
| Tool sharps, &c. ... ... ... | 0 | $1\frac{1}{2}$ |
| Putty and red lead ... ... ... | 0 | 1 |
| Carriage and cartage ... ... | 0 | $1\frac{3}{4}$ |
| Road repairs... ... ... ... | 0 | $1\frac{1}{4}$ |
| Test boxes ... ... ... ... | 0 | $3\frac{3}{4}$ |
| | 5 | $10\frac{1}{4}$ |

Total length laid, 9,400 feet.

| | Metal used. |
|---|---|
| Pipes ... ... ... ... ... | 1,025 |
| Clips ... ... ... ... ... | 1,025 |
| Bolts ... ... ... ... ... | 6,150 |

*Wire Laying.*—Total length, 8,870 yards.

| | | d. |
|---|---|---|
| Labour ... ... ... | per yard | ·38 |
| Odd material ... ... | „ | ·06 |
| | | ·44 |

Pipes laid in trench 1 ft. 6 in. deep by 1 ft. 3 in. wide. Conditions of ground very varying. About one-half total length ordinary macadamised road, one-quarter macadam made up of three layers where road had been raised, remaining quarter in soft gravelly bed of Parade. All work carried out by men quite unskilled in cable work, and trouble was experienced at the start from bad joints, but after experience remedied this fault.

*Arc Lights.*—A contract was entered into with the Corporation for the erection and lighting of 15 arc lamps on the Parade, and the following figures refer to it. Some posts were put in the ground in the ordinary manner, and little trouble was experienced; but some three or four had to be placed in an excavation in concrete, which materially increased the cost of labour.

*Posts* about 20 ft. high, cast iron.
*Lanterns* specially made in copper.
*Lamps*, Brush, 16-hour, round type.

| | | £ | s. | d. |
|---|---|---|---|---|
| Lamps ... ... ... | each | 12 | 5 | 0 |
| Lanterns ... ... ... | ... | 5 | 10 | 0 |
| Posts ... ... ... | ... | 4 | 17 | 5 |
| Switches ... ... ... | ... | 0 | 13 | 6 |
| Wages ... ... ... | ... | 2 | 0 | 5 |
| Tool repairs ... ... | ... | 0 | 0 | 7¾ |
| Materials (odd) ... ... | ... | 0 | 6 | 11 |
| Repairing roads ... ... | ... | 0 | 1 | 5 |
| Gas barrel from mains ... | ... | 0 | 3 | 6½ |
| Carriage ... ... ... | ... | 0 | 2 | 0 |
| Cable ... ... ... | ... | 0 | 13 | 6 |
| Total, per lamp | ... | £26 | 14 | 4¼ |

F. B. NICHOLSON.

## COST OF RUNNING ARC LAMPS.

### Taken from Actual Working.

*Case* 1.—One hundred arc lamps, each of 2,000 nominal candle-power, viz., 50 volts and 10 amperes. All in series. Total cost, including rent, taxes, wages, allowance for depreciation, and interest on capital, power, &c., between ½d. and ⅓d. per lamp per hour. Lighting for about 3,000 hours per annum.

*Case* 2.—Three 2,000-candle-power arcs. Time, or running, 383 hours per annum. Cost, between 10d. and 11d. per hour for the three arcs, illuminating an area of 4,674 square feet, and replacing 300 gas jets (4 feet), and costing 2s. 7d. per hour. Gas 2s. 2d. per 1,000. Cost includes power, and interest on £250, cost of installing; wages not enough to be reckoned; coal, 5s. 6d. per ton; carbons (11 mm., 12 in. long), 2d. each.



F. B. NICHOLSON.

# ABSTRACTS.

## H. NAGAOKA—COMBINED EFFECTS OF TORSION AND LONGITUDINAL STRESS ON THE MAGNETISATION OF NICKEL.

(*Phil. Mag., Vol.* 27, *February*, 1889, *p.* 117.)

The nickel wire to be experimented upon, which was 1 mm. in diameter and 40 cm. long, had two pieces of brass wire securely attached to its ends; the upper piece was firmly fixed to a support, the lower piece carried a scale-pan which could be loaded to any desired extent. The lower brass wire passed through a hole in the centre of a brass bar, the ends of which could slide in two vertical V grooves, cut diametrically opposite each other on the inside of a brass cylinder. This cylinder, which was divided on its outer surface into a circle of degrees, fitted on to a lower cylinder, so as to admit of its being rotated; a set screw in the diametral brass bar clamped the lower piece of brass attached to the nickel wire. Any degree of torsion could therefore be given to the nickel wire which passed vertically through the centre of a magnetising solenoid, while the V grooves allowed of its being stretched by the weights in the scale-pan.

The conditions of the experiment were continually varied by altering both the longitudinal stress and the strength of the magnetic field produced by the solenoid. The author states that the general results may be summarised as follows:—In all magnetic fields with moderate loading, the effect of twisting nickel wire is to increase the magnetisation. This increase depends on the strength of the field as well as on the longitudinal stress. If the field be weak, and the longitudinal stress sufficiently great, the magnetisation increases in one direction of twist, and decreases in the other. Eventually for a particular stress which is approximately proportional to the field, the wire begins to show opposite polarity, and the cyclic curve of magnetisation passes gradually from a two-looped to a single-looped form. For stronger fields similar effects exist, but in fields higher than a critical value the increase and decrease of magnetisation take place for reversed directions of twist, and at the same time the course of the curve becomes eversed.

## E. DORN—DETERMINATION OF THE TRUE OHM.

(*Annalen der Physik und Chemie, Vol.* 36, 1888, *pp.* 22 *and* 398.)

It is not possible to do more than glance at this very complete treatise on the accurate measurement of the ohm, extending as it does to one hundred pages bristling with formulæ and tabulated results of observations.

The method of measurement was that commonly known as W. Weber

third method, modified by Dorn. The damping of a single powerful magnet by the coils of a galvanometer closely surrounding it was observed, and the constant of the galvanometer determined by comparison with a tangent galvanometer. The observations, which were carried out in the latter half of 1885 and in the beginning of 1886, were very numerous. Each observation comprised the following measurements:—(1) Measurement of the distance of the scales from the two galvanometers; (2) Verification of distance of the index marks of the magnet; (3) Comparison of the resistance of the galvanometer with a standard resistance; (4) Measurement of the damping with four different values of the galvanometer resistance; (5) Determination of the constant of the galvanometer; (6) Determination of the period of oscillation; (7) Determination of the air-damping; (8) Observation of the ratio $^M/_H$ from the deflection of the magnetometer of the tangent galvanometer by the galvanometer magnet in two positions; (9) Corresponding observation at the place occupied by the galvanometer; (10) Comparison of the value of the earth's horizontal intensity at the place of the galvanometer with that at the place of the tangent instrument.

The final mean value of the ohm found by Dorn was—

1 ohm = **1·06243 metres** of mercury, 1 sq. mm. in section at 0° C.

This value is the mean of the following:—Six determinations in summer gave a mean of 1·06243 ± 0·00018; nine in autumn, 1·06242 ± 0·00025; eight in winter, 1·06244 ± 0·00052.

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## G. H. VON WYSS—RESISTANCE OF MAGNETIC IRON.

(*Annalen der Physik und Chemie, Vol.* 36, 1889, *p.* 447.)

The iron wire, placed inside a magnetising solenoid, formed one of the branches of a Thomson bridge, so arranged that the resistance of the connections and contacts could be eliminated. In order to eliminate the effect of the heat which was necessarily produced in the inside of the solenoid by the passage of the magnetising current, a second arm of the bridge contained a precisely similar iron wire in a precisely similar solenoid so far as size and construction were concerned, but with its coils so connected that the current in one half neutralised that in the other half. The two solenoids being connected in series and the same current passed through them, both were equally heated, but only one iron wire was magnetised.

The first point to which Von Wyss directed his attention was the reversal of the direction of the magnetising current; he found it to have no effect. The tabulated results of the measurements and the curves plotted from them show that the resistance was in every case increased by magnetisation; the fields of force used varied in intensity. Also, it appears that the increase in resistance becomes greater with more intense fields. The author carefully determined the magnetic moment of the iron wire; and he concludes that the change in its electric resistance is nearly proportional to the change in the magnetic; at any rate, this is true within the limits of his experiments.

## C. L. WEBER—CONDUCTIVITY OF SOLID MERCURY.

*(Annalen der Physik und Chemie, Vol. 36. 1889, p. 587.)*

Mr. L. Grunthal having published some results which are opposed to those obtained by Cailletet and Bouty, and by the author, the latter seeks to explain the differences, which are:—(1) The resistance of liquid mercury is only about one and a half times that of solid mercury, instead of four times; (2) The temperature coefficient is only 0·002 to 0·0004, instead of 0·4 per cent.; (3) This temperature coefficient is variable. The author does not claim absolute exactness; but he cannot admit that he is so far wrong as Grunthal's experiments would go to prove; besides, his figures agree closely with those obtained by the two French observers.

Both agree as to the resistance of mercury just above the freezing point; therefore, in order to explain the wide divergence in the measurement of the resistance just below this point, it must be assumed that on solidification some sudden alteration takes place which either makes Weber's resistance appear less than it really is, or makes Grunthal's appear greater. There is, however, nothing in the phenomenon of solidification which would lead us to expect a decrease, but rather the reverse, therefore Weber's value is the more probable, especially since, in solidifying, breaks in continuity of the substance of the mercury may occur. Moreover, as stated, Weber's figures agree with those of Cailletet and Bouty; from the value of the specific resistance at zero, viz., 0·2826, and the temperature coefficient, 0·00433, the specific resistance at − 39 would be 0·23488 for the solid mercury, and from $s = 1$ and $a = 0{\cdot}00901$ the specific of the liquid mercury at − 39° would be 0·96486, the ratio of these two values is 4·108; Cailletet and Bouty find the ratio to be 4·08.

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## J. KLEMENCIC—THE SUITABILITY OF PLATINUM-IRIDIUM WIRE AND SOME OTHER ALLOYS FOR THE CONSTRUCTION OF STANDARDS OF RESISTANCE.

*(Beiblätter, Vol. 13, 1889, p. 89.)*

Let $s$ be the specific gravity, $k$ the conductivity at 16°-17°, mercury being taken as unity, $e$ the thermo-electro-motive force with copper in microvolts per degree, $p$ the temperature coefficient per degree; then the following values have been found for various alloys:—

| ALLOY. | S | K | $l^o - 17^o$ | $l^o - 100^o$ | $10^6 p 16{\cdot}5 - 46$ | $10^6 p_0 - 16{\cdot}5$ |
|---|---|---|---|---|---|---|
| Platinum-Iridium... | 21·60 | 3·78 | 7·14 | 7·15 | 1,264 | 1,250 |
| Bare Nickelin ... | 8·96 | 2·90 | 28·3 | 33·2 | 180 | 185 |
| Covered Nickelin ... | 8·96 | 2·91 | 28·7 | 33·9 | 180 | 183 |
| Bare German Silver | 8·62 | 3·84 | 9·75 | 11·47 | 396 | 380 |
| Covered German Silver ... ... | 8·64 | 3·60–3·67 | 11·1 | 13·2 | 367 | 360 |
| Platinum-Silver ... | — | 3·11 | 6·62 | — | 271 | 267 |

With copper and nickelin or copper and German silver, the thermo-current flows across the warm junction to the copper; in the two platinum alloys the direction is the reverse.

On comparing the resistance coil at intervals of ten months with a mercury standard, small differences were found which were sometimes positive and sometimes negative, and varied from 0·019 to 0·064 per cent. Small deformations have but little effect on the resistance. The two platinum alloys and covered nickelin show a small secular decrease. Repeated deformations, such as bending and unbending, have considerable effect. If the resistance of the wires before being heated to glowing is equal to unity, then the resistance afterwards, when reduced to the initial temperature (16·5°), is—

| | | | |
|---|---|---|---|
| Platinum-Iridium ... ... ... | 0·97243 | Platinum-Silver ... ... ... | 1·01393 |
| Bare Nickelin ... ... ... | 1·00789 | Bare German Silver ... ... | 1·00366 |
| Covered Nickelin ... ... ... | 1·00725 | Covered German Silver ... | 1·00824 |

Stretching and torsion increase the resistance in each case; the increase may be transient or permanent according to the length of time during which the force acts.

The rate of cooling was also very carefully investigated. If T is the temperature of the wire, $t$ that of the surrounding medium, S the specific resistance at $t$, $i$ the strength of the current used to heat the wire, $r$ the radius of the wire, $a$ the coefficient for temperature, and A the constant of cooling, then

$$\frac{i^2 \, S \, M}{r^2 \, \pi} \left\{ 1 + a \, (T - t) \right\} = 2 \, \pi \, r \, A \, (T - t),$$

and if $a$ is small, for wires of equal diameter and for equal currents,

$$T - t = \text{const.} \, \frac{S}{A}.$$

The actual wires were not all of the same diameter, otherwise the values of A, which varied from 0·00091 to 0·00133, would have been equal.

The author concludes that platinum-iridium and platinum-silver are the alloys which best meet all the requirements. The high thermo-electro-motive force of nickelin when in conjunction with copper is an objection to its use for a standard resistance, though its low temperature coefficient is favourable to its use in resistance-boxes.

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## E. LANDMANN—EXPERIMENTS ON BICHROMATE BATTERIES WITHOUT DIAPHRAGMS.

(*Beiblätter, Vol.* 13, 1889, *p.* 94.)

The cells have a height greater than their diameter, and contain about 6 litres of a solution of 12 parts of bichromate of sodium in 100 parts of water, to which 25 parts of sulphuric acid is added. If about 1 square dm. of zinc is exposed to the solution, it will not need to be changed for

two hours. Owing to their very low internal resistance, these cells can be conveniently used for working glow lamps coupled in parallel. On account of the small quantity of material required, the difficulty of crystallisation, and the low price of the material, the bichromate of sodium is to be preferred to the potassium salt or to chromic acid. The depolarisation and E.M.F. is greater with porous carbon than with hard close-grained carbon. The zincs used should be free from carbon and iron, since, if the cells are coupled in parallel, local action may be set up.

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## M. BELLATI and S. LUSSANA—OCCLUSION OF HYDROGEN BY NICKEL.

(*Beiblätter*, *Vol.* 13, 1889, *p.* 95.)

Nickel will not absorb dry hydrogen; but when a nickel wire was used as the negative electrode in a water voltameter, and the quantity of gas evolved compared with that from another voltameter with platinum electrodes, and with a third with platinum anode and nickel cathode, it was found that in 200 hours the nickel had absorbed about 100 times its volume of hydrogen. The occluded hydrogen is not given up if the nickel is placed in a bell-jar containing air and sealed with mercury, but the mercury rises owing to oxidation of the metal.

When a long nickel wire, hanging vertically in a tube of acidulated water, served as cathode to a parallel platinum wire, the nickel wire increased by 0·000036 of its initial length, viz., 1·44 m. This is not an effect of temperature, since comparison was made with the platinum wire, which would have expanded about equally under the influence of heat.

The occlusion of hydrogen increases the resistance of the nickel. If the volume of hydrogen at 0° C. and 760 mm. pressure is $v$ times the volume of the nickel, then the increase of resistance at 22·7° can be calculated from the formula,

$$0{\cdot}001626 + 0{\cdot}00005789^2\, v.$$

With increasing temperature, the resistance of nickel containing occluded hydrogen seems to alter rather less than ordinary nickel; thus the temperature coefficient of the latter is 0·00371, while that of nickel with 100 times its volume of occluded hydrogen is 0·00369.

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## J. H. VAN t'HOFF and L. T. REICHER—THE DISSOCIATION THEORY OF ELECTROLYTES.

(*Beiblätter*, *Vol.* 13, 1889, *p.* 98.)

The experiments were carried out with very dilute solutions, down to $\frac{1}{10000}$ gramme molecule per litre. Their resistances were determined by means of the telephone method, using Arrhenius' resistance vessels. If $v$ is the volume in litres, in which 1 gramme molecule is dissolved; $m$ the mole-

cular conductivity; $m_\infty$ the molecular conductivity for an infinitely dilute solution; then, according to Ostwald,

$$\frac{\left(\frac{m_\infty}{m}\right)^2}{v\left(1-\frac{m_\infty}{m}\right)} = K,$$

where K is a constant.

The following table shows the results of the experiments:—

| | Temp. | $m_\infty$ | (Log. K) + 10 | Temp. | $m_\infty$ | (Log. K) + 10 |
|---|---|---|---|---|---|---|
| Acetic Acid ... ... ... | 14·1 | 316 | 5·250 | 19·1 | 335 | 5·337 |
| Butyric Acid ... ... | 14·1 | 305 | 5·214 | 19·1 | 327 | 5·274 |
| Benzoic Acid ... ... | — | — | — | 19·1 | 307 | 5·921 |
| Formic Acid... ... ... | 14·1 | 330 | 6·296 | — | — | — |
| Monochloracetic Acid | 14·1 | 311 | 7·2 | — | — | — |

# LIST OF ARTICLES

RELATING TO

# ELECTRICITY AND MAGNETISM

Appearing in some of the principal Technical Journals during the Month of MARCH, 1889.

## I.—BATTERIES AND ACCUMULATORS.

L. DONATI—Battery for Powerful Currents.—*Beiblätter*, vol, 13, p. 169, 1889.

F. EXNER and J. TUMA—Chemical Theory of the Galvanic Cell.—*Beiblätter*, vol. 13, p. 178, 1889.

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## II.—DYNAMOS AND MOTORS.

R. ARNOUX—Commercial Efficiency of Dynamos.—*Lum. El.*, vol. 31, p. 501, 1889.

G. RICHARD—Details of Dynamo Construction.—*Lum. El.*, vol 31, p. 601, 1889.

ANON.—Fritsche's Wheel-Dynamo.—*El. Zeit.*, vol. 10, p. 134, 1889.

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## III.—ELECTRO-CHEMISTRY AND ELECTRO-METALLURGY.

VIOLLE and CHASSAGNY—Electrolysis.—*C. R.*, vol. 108, p. 248, 1889.

E. BOUTY—Conductivity and Electrolysis of Concentrated Solutions of Sulphuric Acid.—*C. R.*, vol. 108, p. 393, 1889.

A. POTIER—Electrolysis of Mercurous Nitrate as a Measure of Current.—*C. R.*, vol. 108, p. 396, 1889.

W. OSTWALD—Mercury-Drop Electrodes.—*C. R.*, vol. 108, p. 401, 1889.

A. MINET—Introduction to the Study of Electro-Chemistry.—*Lum. El.*, vol. 31. pp. 426, 477, 532, 576, 619, 1889.

W. H. SCHULTZE—Electrolytical Properties of Mica at High Temperatures.—*Annalen*, vol. 36, p. 655, 1889.

K. SCHREBER—Electro-motive Force of Thin Films of Hydrated Superoxides.—*Annalen*, vol. 36, p. 662, 1889.

B. NEBEL—Disintegration of Copper by the Electric Current.—*Beiblätter*, vol. 13, p. 177, 1889.

ANON.—Diamonds made by Electricity.—*El. Zeit.*, vol. 10, p. 163, 1889.

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## IV.—ELECTRIC LIGHT.

G. RICHARD—High-Speed Engines.—*Lum. El.*, vol. 31, p. 407, 1889.

C. REIGNIER—Overall Diameters of Stranded Cables.—*Lum. El.*, vol. 31, pp. 425, 456, 1889.

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ANON.—Schuckert's Installations in Germany.—*El. Zeit.*, vol. 10, p. 135, 1889.

C. DIHLMANN—Calculation of a Distributing Network of Conductors.—*El. Zeit.*, vol. 10, p. 148, 1889.

E. LIEBENTHAL—Note on the Bunsen Photometer.—*El. Zeit.*, vol. 10, p. 161, 1889.

ANON.—Electrically Lighted Buoys.—*El. Zeit.*, vol. 10, p. 165, 1889.

## V.—ELECTRIC POWER.

ANON.—Tree-Felling by Electricity.—*El. Zeit.*, vol. 10, p. 164, 1889.

## VI.—MAGNETISM AND ELECTRO-MAGNETISM.

WM. BROWN—Steel Magnets.—*Phil. Mag.*, vol. 27, p. 270, 1889.

T. MOUREAUX—Absolute Value of the Magnetic Elements on January 1st, 1889.—*C. R.*, vol. 108, p. 56, 1889.

G. BERSON—Effect of Blows on the Permanent Magnetisation of Nickel.—*C. R.*, vol. 108, p. 94, 1889.

P. JANET—Reciprocal Effect of Two Magnetisations at Right Angles in Iron.—*C. R.*, vol. 108, p. 398, 1889.

A. POTIER—Relation between Magnetic Rotatory Power and the Influence of Ponderable Matter on Luminous Waves.—*C. R.*, vol. 108, p. 510, 1889.

H. LORBERG—Theory of Magneto-electric Induction.—*Annalen*, vol. 36, p. 671, 1889.

D. GOLDHAMMER—Effect of Magnetisation on Conductivity.—*Annalen*, vol. 36, p. 804, 1889.

C. BAUER—New Researches on Magnetism.—*El. Zeit.*, vol. 10, pp. 128, 151, 1889.

## VII.—MEASUREMENTS AND MEASURING INSTRUMENTS.

J. MOSER—Capillary Electrometer and Mercury-Drop Electrodes.—*C. R.*, vol. 108, p. 231, 1889.

E. MORELLI—Measurement of Phase Differences by means of the Semicircular Electrometer.—*Lum. El.*, vol. 31, p. 630, 1889.

L. DONATI—New Quadrant Electrometer.—*Beiblätter*, vol. 13, p. 168, 1889.

## IX.—STATIC AND ATMOSPHERIC ELECTRICITY.

K. WESENDONCK—Difference of Polarity in Electrostatic Discharges—*Beiblätter*, vol. 13, p. 194, 1889.

F. NEESEN—Lightning Conductors.—*El. Zeit.*, vol. 10, p. 145, 1889.

## X.—TELEGRAPHY AND TELEPHONY.

VASCHY—Theory of Current Propagation in a Line.—*Ann. Tel.*, vol. 15, p. 481, 1888.

J. MOOSER—Experiments on Microphonic Contacts.—*Lum. El.*, vol. 31, p. 451, 1889.

Anon.—Wehr's Lightning Discharger.—*Lum. El.*, vol. 31, p. 484, 1889.

Anon.—Binding for Siliceous Bronze Wires.—*Lum. El.*, vol. 31, p. 485, 1889

E. Zetsche—New Multiple Telephone Switch-Boards.—*Lum. El.*, vol. 31, p. 556, 1889.

J. Karreis—The Stockholm Telephone System.—*Lum. El.*, vol. 31, p. 608, 1889.

Weisner—Telephony in England.—*El. Zeit.*, vol. 10, pp. 140, 154, 1889.

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## XI.—THEORY.

E. F. Herroun—The Divergence of Electro-motive Forces from Electro-chemical Data.—*Phil. Mag.*, vol. 27, p. 209, 1889.

J. W. Gibbs—Comparison of the Electric Theory of Light and Sir W. Thomson's Theory of a Quasi-labile Æther.—*Phil. Mag.*, vol. 27, p. 2°° 1889.

L. Poincarré—Conductivity of Fused Salts.—*C. R.*, vol. 108, p. 138

E. L. Trouvelot—Application of Photography to the Study Phenomena.—*C. R.*, vol. 108, p. 346, 1889.

C. Decharme—Difference between the so-called Positive and N tricities.—*Lum. El.*, vol. 31, pp. 401, 473, 566, 1889.

L. A. Lorentz—Theory of Thermo-Electricity.—*Annalen*, vol. 36,

M. Planck—Theory of Thermo-Electricity in Metallic Conducto vol. 36, p. 624, 1889.

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H. Hertz—Rays of Electricity—*Annalen*, vol. 36, p. 769, 1889.

J. Bergmann—Effect of Heat on Conductivity.—*Annalen*, vol. 36, p.

S. Tereschin—Specific Inductive Capacity of some Organic Bodie vol. 36, p. 792, 1889.

H. Bäckström—Conductivity of Specular Iron Ore.—*Beiblätter*, vol 1889.

R. Felici—Potential of a Conductor moved in a Magnetic Field. vol. 13, p. 192, 1889.

A. Righi.—New Phenomenon produced by Radiation.—*Beiblätt* p. 198, 1889.

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## XII.—VARIOUS APPLIANCES.

L. Damion—Method of Diffusion of an Electric Current in the Huma ody.—*C. R.*, vol. 108, p. 88, 1889.

E. Zetsche—Professor Kleiszner's Electric Calendar Clock.—*Lum. El.*, vol. 31, p. 419, 1889.

Anon.—Electro-magnetic Lighter.—*Lum. El.*, vol. 31, p. 486, 1889.

JOURNAL

OF THE

# Institution of Electrical Engineers.

*Founded* 1871. *Incorporated* 1883.

| Vol. XVIII. | 1889. | No. 80. |
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The One Hundred and Ninety-first Ordinary General Meeting of the Institution was held at the Institution of Civil Engineers, 25, Great George Street, Westminster, on Thursday evening, April 11th, 1889—Professor W. E. Ayrton, F.R.S., Vice-President, in the Chair.

The minutes of the Ordinary General Meeting held on March 28th were read and approved.

The names of new candidates for election into the Institution were announced and ordered to be suspended.

The following transfers were announced as having been approved by the Council:—

From the class of Students to that of Associates—

Bernard M. Jenkin. | Leonard Newitt.

Donations to the Library were announced as having been received since the last meeting from the Director-General of Italian Telegraphs, the Institution of Civil Engineers, and A. R. Bennett, Member, to whom the thanks of the meeting were heartily accorded.

The Chairman: The paper to be read this evening is on "Underground Conduits and Electrical Conductors," by John B. Verity; but I am sorry to say that Mr. Verity has been suddenly

seized with illness, and is unfortunately unable to be present to personally give us the paper, which will be read by the Secretary.

The following paper was then read by the SECRETARY:—

# UNDERGROUND CONDUITS AND ELECTRICAL CONDUCTORS.

By JOHN B. VERITY, Member.

My object in bringing this subject before you this evening is to promote an interchange of opinion and experience concerning one of the few remaining problems connected with the general distribution of electricity in our cities. Since I began to write this paper on my return from America, much information has appeared in the electrical press, and the close attention now being paid to the matter in America is equalled by a like spirit of inquiry here. The outcry against overhead wires has been gaining in force; and although the numerous objections urged are devoid of solid grounds, there is a general feeling that any considerable extension of overhead cables for electric lighting is impossible, both on account of the difficulty, and no inconsiderable expense, in procuring way-leaves, and also the attitude taken up by the Board of Trade and others to put a stop to them. Overhead wires in this country may therefore be simply considered as a temporary measure to admit of the electric supply companies getting to work, and permitted only on the distinct pledge of the removal of such wires within a certain period.

The time allowed for the execution of underground works in Provisional Orders and Licenses, although as long a period as could be obtained, is all too short, and a thorough discussion of the subject at the present time, in its numerous aspects, is highly advisable to enable it to be afterwards promptly and adequately dealt with.

In three months we may reasonably conceive that certain electric supply companies will be under legal obligations to at once face the problem of putting high-tension electric conductors

underground. It will be no petty attempt narrowed down to a small area, but in two special instances the undertaking is proposed to be carried out on a large scale by companies of adequate financial resources, and with supply stations already erected or in process of erection.

"Is failure probable?" may seem a singular question to put here, but it is a necessary one, leading up to the various difficulties to be encountered, and, in view of the alarmist statements at the recent Electrical Convention in Chicago, one that deserves prominence. Mr. Lynch's paper giving the results of the working of underground cables, and which was practically the only part of the meeting reported in the English papers, showed but one side of the case, and from the discussion following it (and which appeared in the *Electrical World*, March 2) much was to be learned. Many of the companies operating underground cables had from some cause or other not given their opinions, and the failures alluded to were in several instances traced to defective conduits, insufficiency of insulation, poor insulation, imperfect jointing, and cheap work. Cheap work has damned electrical enterprise before now, and it behoves us all to guard against a recurrence of it. Anything also on the subject of conduits and underground electric conductors emanating from American electricians has to be carefully sifted, as not even are our Unionists and Home Rulers more defined than certain "overhead" and "underground" men in America. The overhead men accuse all the underground men of being interested in some of the dozen forms of conduits put forth; the underground men retaliate by saying, "You don't want to go underground," and "You boldly say we cannot go underground."

In England we stand on a different footing altogether: if much more electric lighting work is to be done we must go underground; and having recognised that, how can it best be done? The necessity and best form of conduits are the first points for discussion.

The successful working of a system of underground electrical cables in a city must be linked to an arrangement of ducts, as the impossibility, without such, of getting at the cables for repairs,

inspection, tapping, and alterations is evident. Even if the local authorities remained quiescent during any continual disturbance of our streets on account of electrical conductors, commercial interest must prevent such occurring. The expense of opening up a crowded thoroughfare to lay underground cables is in many cases as great as the cost of whatever is being laid, while by using a duct this expense, once incurred, should not be again necessary. Again, if the cable be laid in the ground without a duct, it is necessary to armour it, and lay it down at first equal to all possible requirements, otherwise any extension of the lighting beyond the capacity of the cable would necessitate a repetition of the whole original work. In this case the first cost of such a large cable would be nearly as great as the smaller cable and duct together. Finally, the life of the best cable for high-tension currents cannot at the present be predicted; and this, together with the other reasons, undoubtedly proves that a conduit with a drawing in and out system is eminently desirable.

An ideal system for underground work is a brick subway, well drained and ventilated, and of sufficient size to permit of a man working in it. Failing this Utopian arrangement, what comes next best? In New York, where the matter has been placed in the hands of a Board of Electrical Control, they start out by saying that a conduit or subway for electrical conductors is nothing more than a mechanical protection for the wires within it, and a convenience for placing or putting them underground. In their experience of three years the Board of Electrical Control have naturally tried and abandoned many conduits. In a room of the Telephone Building, New York, occupied by the Subway Construction Corporation, there is a heap of experience—experience gained at no little cost—that it would be of advantage for engineers who lightly propose undertaking such work to see and meditate on. Wooden conduits, bitumen conduits, asphalte conduits, earthenware conduits, samples of underground conductors—all are here in what is termed the museum, and afford a collection of failures most instructive as to *what not to do.*

In many instances, however, the conditions there would not

apply to England. At present we have no system of steam-heating with leaky pipes, causing trouble wherever they go. Again, here we have not those great extremes of heat and cold, the frost compelling conduits to be laid at an average depth of four feet, and the heat causing various troubles with asphalte and bitumen conduits.

At the same time, from the number of miles of conduits laid down there should be something to learn, and I therefore propose instancing some of the more important conduits known in America and elsewhere.

Conduits are divided into two classes—the so-called solid conduit, and the hollow conduit. Solid conduits are useless considering here, as they practically consist of cables bedded in asphalte or bitumen, which means ripping up the ground and breaking the enclosing mass if alterations are required. With hollow conduits, the difficulties to be encountered seem to be to provide a smooth channel with convenience for drawing in and out to prevent explosions through accumulation of gas, and to keep them reasonably dry and water-tight.

In the discussion at Chicago it was stated that in one instance it was necessary to keep a blower going to ventilate an iron conduit, while several authorities appear to consider it advisable on dry days to take off the manhole covers. Accidents have, of course, occurred from conduit explosions, and the engineer of the United States Electric Lighting Company instances one where the manhole covers of the conduit were blown over the roof of a four-storied building. But in England, again, the work done by the gas companies is far better, and the leakage in our streets is not in any way to be compared with the quantity of illuminating gas which wanders, fancy free, through the soil of many of the cities in the States.

It appears desirable that conduits should be ventilated; and the form adopted, where possible, is to ventilate them by means of a pipe running into the base or up the interior of the street lamp-posts. With regard to keeping them water-tight, this is another constructional question, surely capable of being properly dealt with.

Many of the complaints of leaky conduits and ultimate failure of cables I trace back to the Dorsett conduit.

This, one of the earliest forms of conduits, is built of blocks formed of a combination of coal-tar pitch and fine gravel, cast with tubular openings $2\frac{1}{2}$ in. in diameter running through them from end to end. They are jointed by pouring soft mastic into the cracks between them, and the blocks made to adhere to this by warming their ends with hot irons and allowing them to cool, after being well set. To prevent the melted mastic from closing the ends of the passages, tubular pieces of paper are inserted, making a sort of internal sleeve coupling. The conduit so formed terminates in brick manholes at the street crossings. The objections to this conduit are that it is brittle, porous, inelastic, and cracks with changes of temperature, so that it is not likely to be made water-tight with average workmanship.

Creosoted wood conduits have been extensively used in the States, and have been a leading cause of trouble and failure with lead-covered cables. A chemical action takes place between the crude creosote and the wood, setting free acetic acid and carbonic acid gas, which, re-acting on the lead pipe, converts its surface into a film of acetate of lead; the action continuing until the entire sheathing is converted into carbonate of lead, or white lead. Creosote is also said to rot rubber. There are, of course, ways of overcoming such troubles; but as I do not think we shall be disposed to use creosoted wood conduits in this country, the matter need not be given much prominence here.

The difficulty of obtaining a conduit that shall be anything more than a mechanical protection for the cables is certain to be great, as, however good the insulating material forming the conduit may be, and however water-tight the joints, there is a difficulty in excluding dampness unless it is practically air-tight, as damp air enters the conduit and condenses on it.

Several conduits have been made, however, with a view to afford electrical as well as mechanical protection to the cables. The Callender bitumen conduit, for instance, is well known here. It is being extensively laid down by the Chelsea Electric Lighting Company, and five hundred yards of it is being tried by the

Metropolitan Electric Supply Company. For high-tension currents its use is doubtful, but perhaps in the discussion something may be said on this point.

Vitrified 3-in. sewer pipes laid in concrete, with cement joints, have been used in some instances for underground work.

The Lake conduit, shown in the illustration (Fig. 1), is made from the best stoneware clay vitrified and well glazed. This conduit, said to be used with success by the United States Electric Lighting Company at Washington, is constructed with six compartments, each being 2½ in. by 4½ in. It is delivered in short lengths, and the joints are protected by stoneware covers set in cement.

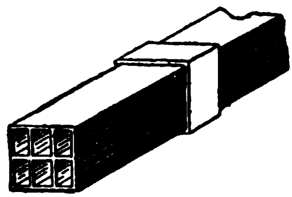


Fig. 1.

The Doulton conduit (Fig. 2) consists of a glazed earthenware pipe in which the cables are laid, separated by toothed insulating

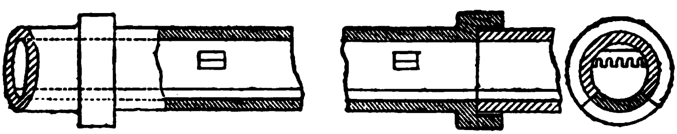


Fig. 2.

partitions. A space at the bottom drains off the condensed moisture, which is got rid of by suitable traps. The joints are made like ordinary drain pipes.

A serious objection to such conduits seems to be the joints, which are made with cement. It is very difficult to break a cement joint without also breaking the pipe; and as it might be necessary at times to put in a new length, this could not well be done. Again, the rigidity of a cement joint is a disadvantage, as in laying earthenware pipes it becomes necessary at times to alter the alignment.

The Hurlbut conduit system (Fig. 3) was designed to overcome this difficulty by means of a flexible joint. The joint is shown in the diagram. C represents asbestos gaskets recessed into the coupling and resting against the pipe; B is a cavity formed

between the asbestos gaskets, and is filled with a permanently plastic sealing material; A represents openings through which

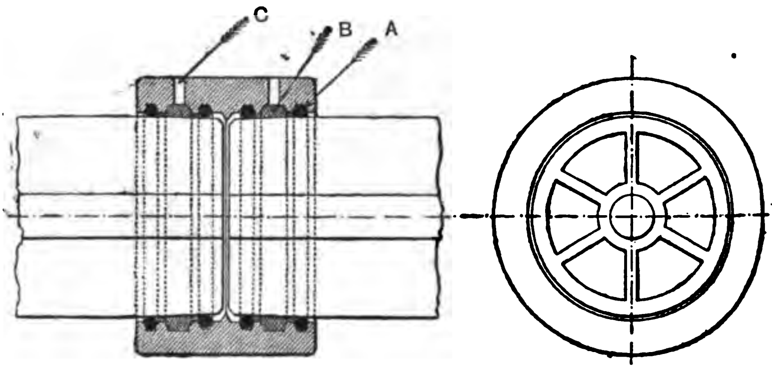



FIG. 3.—HURLBUT FLEXIBLE CONDUIT.

*A* Asbestos Gaskets. *B* Plastic Sealing. *C* Holes for filling in plastic material.

the sealing material is poured. It is claimed that such joints will always remain both flexible and hermetically sealed.

Mr. B. Verity's conduit (Fig. 4). This is a system of glazed stoneware clay conduit, with separate channel for each conductor. The joints are made as shown on diagram, the double cone-piece being inserted before the jointing material is run in. The special object of this conduit is to enable bare conductors to be used,

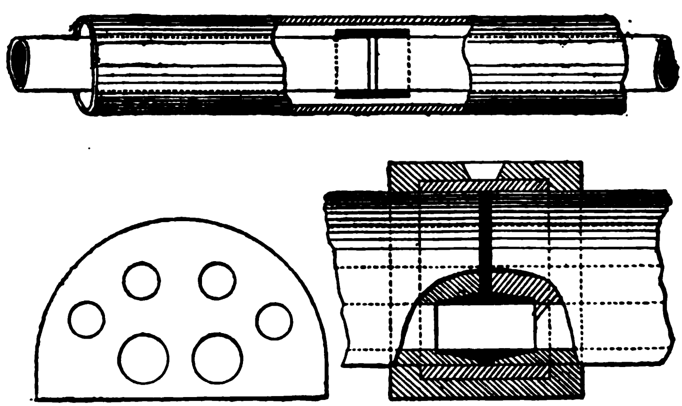


FIG. 4.

insulated only at the manholes, or where they are likely to be handled. Experiments have proved this form to be capable of

standing almost anything mechanically when laid on a fairly good bottom. Mr. Verity has also patented a system of glass tubes in iron pipes for the same purpose.

Pipes of glazed earthenware with several ducts form a good mechanical protection, being capable of resisting considerable crushing pressure; while the glazing both inside and outside provides also, under certain conditions, a first-class electrical protection. If it be desirable that the conduit should be formed of a non-conductor, then nothing could be better than such glazed earthenware, which also is of no great cost. The difficulty of maintaining good joints with earthenware pipes has been referred to some time ago in connection with conduits for telegraph purposes, several instances having been given by Mr. Preece and others, of roots of trees, branches, and other vegetation having made their way through the joints. Is this capable of being overcome by a flexible joint, which should at any rate prevent joints giving through expansion and contraction?

The great disadvantage, however, to the use of earthenware pipes in many places in a city like London is the amount of space required for them; in fact, the difficulty of laying any conduit at all in some of our streets will be found simply enormous.

As a mechanical protection only, it is immaterial whether the conduit be a non-conductor or not. The simplest form of such a duct is, of course, an iron pipe; and the Western Union Telegraph Company, among others, have laid cast-iron pipes of 4 in. and 5 in. diameter, with manholes at a distance of about 450 ft. apart. In the majority of cases, however, the plan has been to lay a number of separate iron pipes in a concrete bed, by which arrangement the pipes can be crowded, or curved, or kept apart, as may best overcome any difficulties or obstacles met with underground. For instance, four pipes may be laid on the same level in two layers; or where only a narrow excavation can be made, the pipes can be curved round so that two pipes are abreast in four layers. In this way the trenching is often materially diminished. Screw-jointed asphalted wrought-iron pipes of 2 or 3 in. diameter laid in hydraulic cement concrete are said to present the greatest tightness of duct against gas and water,

together with the greatest strength. (Illustration given—Figs. 5 and 6.)

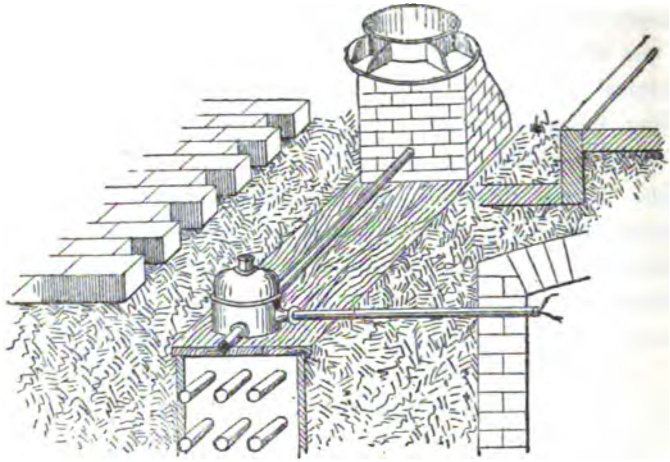


FIG. 5.

Cement - lined sheet-iron pipes, however, possess some advantages, such, for instance, as smooth interiors, and the advantage of an iron enclosure, without the cable being brought into contact with it; and on the sheet iron being eaten away, the cement still retains the pipe form. Zinc tubes, again, have been used, but are liable to be dented in; and asphalted concrete is often preferred to hydraulic cement concrete.

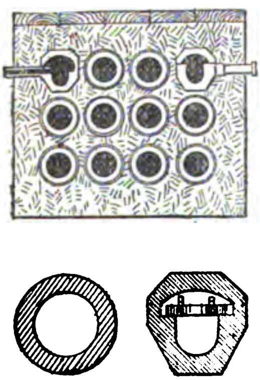


FIG. 6.

Whatever system of pipes be used, it is important that they should have smooth interiors and close joints; and every conduit, of whatever kind, must be thoroughly well bedded, so as not to be liable to displacement from above.

An ingenious conduit core (Fig. 7) has been devised by Mr. Chenoweth: a wooden cylindrical rod 14 or 20 ft. in length is cut in two, forming two half-cylinders, and the space removed is occupied by an iron rod having a thickness equal to the portion sawed out. This forms the mandrill or core shown in the illus-

tration. A ribbon of galvanised iron 1 in. wide and a thickness of No. 27 gauge is wound spirally round this from end to end, securing the ends to the wood to prevent unwinding.

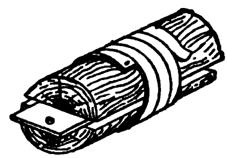


Fig. 7.

After painting the outside of the core with a mixture of clay, soapstone, and water, the core is placed in the ditch on crossed pieces of wood. If more than one duct is to be constructed, other cores are placed side by side; concrete is then well tamped round, bringing the top to an even level. The iron rod is then removed, as well as the two pieces of wood, and the fastenings of the ribbon ends. When the cement hardens, the iron spiral can be drawn out at any manhole or unfinished portion of the work. It is claimed that the surface coating of clay and soapstone adheres to the interior of the duct and produces a smooth surface, while a monolithic structure is ensured, water-tight by reason of its construction. I think this is another instance of American ingenuity.

The Johnstone iron conduit, shown in the illustration (Fig. 8), is said to be very successful, and I saw portions of it as designed to be laid in New York City. It is made in sections about 6 ft. long, and has six ducts. The three lower ones and the central duct on the top could be used for mains, and the two outer top ducts for house-to-house and street lighting. Where any house circuit is required the top half of a single section is taken, and a new half-section with a hole in it, to which an elbow can be bolted, is fixed in its place.

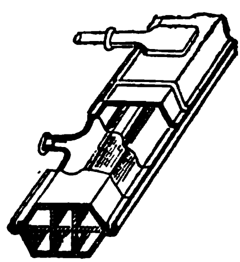


Fig. 8.

A form of conduit made by Mr. W. E. Irish, of Cleveland (Fig. 9), is also worthy of notice. In the illustration the conduit consists of a tubular pipe cast in sections, with flanges which are recessed on the inside to receive a rubber ring, forming a gasket between the two sections when bolted together. The conduit is provided with a longitudinal slot or opening along the top, with inwardly inclined sides and flat surfaces on either side of the slot, to which the cover is secured by screws. To prevent the entrance

of moisture through this slot a strip of rubber or leather is first laid on the top of the flanges, extended from one outer edge to the other. On this a solid wedge-shaped piece which fits into the

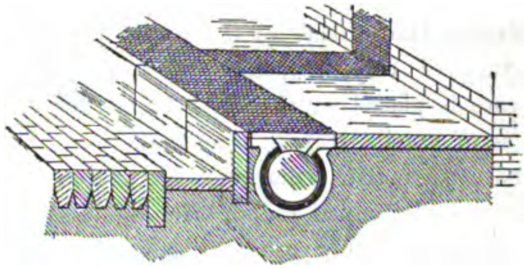


Fig. 9.

slot snugly is placed so that on the cover being screwed down the wedge, with the packing surrounding it, is forced into the slot so as to seal it perfectly. It is claimed that there is great convenience in laying wires with this conduit, and that any wire can be tapped at any point by simply removing the cover and making the necessary connections. The illustration represents the conduit with a branch, also a junction box, &c.

Diagram Fig. 10 represents Mr. J. E. H. Gordon's system, as used by the Metropolitan Electric Supply Company. The cables are drawn into an iron pipe, and smaller iron pipes are led from

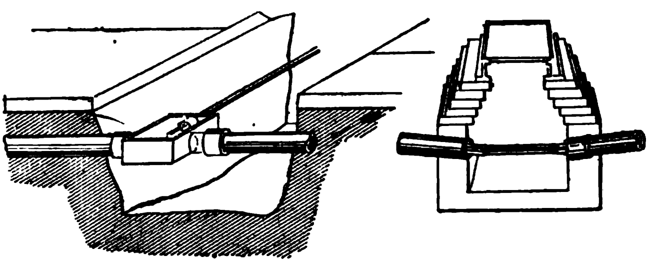


Fig. 10.

the house junction boxes to carry the wires for house lighting. Testing boxes of a larger size, and built of brick, are provided at intervals.

It is generally admitted that an important point in underground work is good manholes. Mr. W. D. Sargent, in a paper read before the National Telephone Association, 1888, said that

within reasonable limits it is almost impossible to get these manholes or working chambers too large, and that they have been compelled in many instances to rebuild and enlarge them after some time.

There is no doubt they must be of good size for convenience of drawing in and out, which is often a difficult matter, especially with lead-covered cables. In one instance, in Chicago, the manholes are built in octagon shape, in brick and cement, about 4 ft. square, being made air-tight with screw-head and rubber gasket; and testing boxes placed in them, so that troubles can be readily located, faulty wires drawn out, and good ones put in. In other cases the manholes vary from 3 ft. 6 in. to 6 ft. deep, with iron curbs and tops, and cement bottoms, and the covers made practically tight against water; many of the lids draining the water from the edge and collecting it in the centre, thus diminishing the liability to leakage at the gasket. With the Johnstone and the Irish conduits it is not necessary to provide for house connections, as the alteration can be made at any time afterwards when an application for light is received. But, as a rule, hand distributing boxes, made of cast iron, with screw-head and rubber gasket, are fixed, one between every two houses, and after jointing, the boxes are often filled with bituminous or insulating compound. In the cement-lined pipe conduit system one pipe is occasionally placed at the top for the house distribution, and connections made with the mains at the manholes. As to the frequency of these latter, much depends on the alignment of the conduit. At angles or changes of grade small manholes are necessary.

The combination curb and gutter conduit (Fig. 11) shown in the illustration is worth notice. The curb and gutter is made of fine concrete, one part of Portland cement, ten parts clean sharp sand, and three parts of broken stone, and the exposed surfaces coated with a granolithic mixture $1\frac{1}{2}$ in. thick, such as is now used for some of our London pavements. A conduit for electrical con-

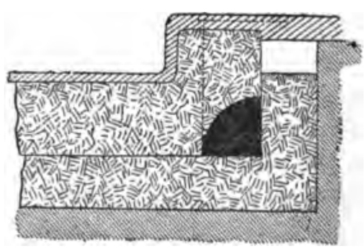


Fig. 11.

ductors of any reasonable size can be formed as shown in the inner angle of the curb, with hand-holes from above closed with iron covers at intervals. The illustration is taken from the admirable report on underground wires by Major Raymond, the engineer commissioner for the District of Columbia, and which contains valuable information on many points.

It was suggested some time ago that the curb-stones should be removed from busy thoroughfares and replaced with a hollow curb edged with cast steel, and the suggestion may bear fruit in the future.

As I still have to deal with electrical conductors, I cannot here refer to the general cost of laying conduits, as at one time I proposed doing. The principal cost, however, of laying them is in making good, and this depends on the material of the road, the cheapest being macadam (varying from 5s. per square yard, whereas asphalte would be nearer 20s. per square yard).

The difficulty of laying any form of conduit in some of our London streets must, as I have already said, be very great. The last comer will of course feel this the most; and although the Hydraulic Engineering Company may have recently had much trouble to find room for their pipes, the electric supply companies, again, will have an easier task than the Telephone Company, who are putting off the evil day.

As to what form of conduit is most suitable for London and our provincial cities there will be differences of opinion, but I trust the discussion following will narrow it down to a small issue upon which such work can be undertaken with a tolerable surety of its lasting success.

There is really sufficient subject-matter for a paper on conduits alone, and in taking up the question of underground conductors I propose to chiefly restrict myself to a consideration of conductors for high-tension electric lighting circuits.

With regard also to underground conductors for low-tension work, I feel that the Edison system, as at present laid down, is thoroughly capable of filling all the requirements of house-to-house distribution. Many companies in the States and elsewhere are using it, and at the down-town station in New York

it has been at work for the past six years successfully. In the recent discussion on Professor Forbes's paper several points of interest concerning the Edison underground conductors were explained, which it would be unnecessary for me here to repeat. A full description of the latest developments of this system, with samples of the improved form of conductors, &c., are now on the ocean, and in the event of it being possible to provide another evening for a continued discussion of this paper, I shall be most happy to show and explain them.

Everyone must acknowledge the foresight shown in devising such a complete system of underground mains so long ago, and which, in spite of the all-round developments of electrical engineering, not only holds its own, but is still far ahead in completeness of any system for low-tension underground work. The Westinghouse Company have, I understand, acquired the rights for Great Britain, and presumably will use it in connection with large transformers for sub-centres. It may be also interesting to note that Messrs. Siemens have made *concentric* three-wire cables, which are being successfully used at Elberfeld, Geneva, and Mulhausen, and they state that the troubles from what would appear very complicated jointing have been satisfactorily overcome.

It is underground conductors for high-tension currents that I propose now to deal with. A principal point for consideration is the necessity or not for lead sheathing where conduits or ducts are employed.

As to the durability of lead much absurdity has been talked. It is notorious that lead pipes have been in the ground under all manner of conditions, and at the end of long periods are still intact. There are no doubt certain clayey soils where lead has not much lasting power, but the lead may be materially preserved by coating it with coal tar, well tapeing it, and again soaking it in the coal tar or some such protective composition. Again, as Dr. W. von Siemens recently said in reply to Professor Forbes's paper, the manner in which the lead covering is generally put on such cables, viz., by heating the lead to the point of melting, and forming it in this condition round the cable, while

passing through the press. There is no certainty of a uniform thickness, and it is very difficult to obtain a tube through which damp, in course of time, will not find an entrance, as air bubbles and impurities in the lead are liable to make the covering porous. A far better result is obtained by covering the insulated cable with a cold-drawn lead tube, as the air which might still be contained in the lead block is compressed to a minimum.

A theory as to partial wasting of lead covering on cables is that it may occur from acid being present in the jute or fibrous insulation, or, again, that the petroleum residuums may set up a chemical action on the lead; but there has not been much evidence on these points. Manufacturers of lead-covered cables have admittedly put on too thin a coating of lead in the earlier days; this has been gradually increased, and with proper conditions and precautions there should not be much fear of its durability.

Iron-armoured lead-covered cables are not necessary if an effective system of conduits is used for mechanical protection. In Berlin, where such cables are employed, they are simply laid in a trench in the ground, and consequently require to be thoroughly protected from mechanical injury. Lead alone is not sufficient.

Dr. Werner v. Siemens said in his recent paper that long years of experience had taught him that although covered with hemp or jute it requires further protection to render it secure against mechanical injury, whether by men or animals, as even rats eat their way through. Such protection is afforded by a double spiral of sheet iron, which, again, is made secure against oxidation by asphalting or galvanising, or by another cover of tarred hemp or jute. Such armouring as this is quite needless with a well-constructed conduit; but as to whether galvanic action takes place, Dr. Siemens has undoubtedly shown by his firm's experience that with a substantial layer of tarred or asphalted jute between the metals it does not occur. This form of armoured cable was laid at Munich for lighting the theatre, and after nearly five years in the ground is as perfect as at first. At other central stations at Elberfeld, Darmstadt, Geneva, Salzburg, Lyons, The Hague, St. Petersburg, their iron-armoured lead-

covered cables prove that, constructed in this way, there need be no fear of galvanic action at any rate.

Lead sheathing may be made durable, but the actual necessity of it where a conduit is used is quite another matter, and a consideration of the two principal divisions of insulating material has considerable bearing on this point.

Of the fibrous and the homogeneous the latter seems to present some important advantages for insulating high-tension cables. For instance, vulcanised rubber is flexible and tough, and affords a continuous and homogeneous covering which should be superior to a fibrous covering. Vulcanised rubber cables are at times sheathed with lead for mechanical protection, but such is not necessary if laid under good mechanical conditions, and Dr. Lant Carpenter recently said that the sulphur in the vulcanised rubber would attack the lead in the same way it would copper. If, as is stated by many English cable manufacturers, compound vulcanised rubber will well withstand the deteriorating effects of damp and gases, then, the mechanical protection being afforded by a conduit, the necessity of lead as a protector is no longer of importance. It is true unprotected vulcanised rubber cables in conduits have failed in the States, but the competition among electric light companies in America is notorious, and the question as to the maintenance of cables is not considered so much as their relative first cost. The object of the American cable manufacturers, naturally suiting their market, has been to make cables with a cheap form of vulcanised rubber insulation to compete with the jute-insulated cables there, and which are cheaper. The vulcanised rubber evidently was not properly prepared as it should be to permanently counteract the deteriorating effects of damp and gases by reason of the expense.

All those acquainted with the rudiments of rubber manufacture must be aware of the enormous difference in quality of different vulcanised rubbers. With Para rubber at 3s. per lb., and the desire to sell it at a shilling, all manner of substitutes and compounds have been introduced. While thoroughly appreciating the energy and genius of American electricians, we can confidently say that cables manufactured in this country are

greatly superior to those made in the United States; and as a basis for this belief we have the fact that most of the cables for high-tension distribution in the States have proved faulty, according to general admittance, while there is undoubted evidence of the successful working of many vulcanised rubber and jute insulated cables in this country and in Europe, made by English manufacturers. The statement is made by the Silvertown Co., Messrs. Henley & Co., and others, that they are absolutely prepared to guarantee for a term of years their vulcanised rubber cables unprotected by lead, even if laid in damp and leaky conduits, provided there is suitable mechanical protection against injury, and that the cables are laid under competent supervision.

With regard to *fibrous* insulating material, it is alleged that it has the defect of opening cracks when bent. If this be the case, it would become more pronounced when the material is soaked in resinous compound, and perhaps less so when petroleum residuums are used, such as, for instance, in ozite, kerite, bitite, &c. If cracks are likely to be formed when the cable is bent or twisted about, air paths would be opened up for disruptive discharges.

In the working directions given for the laying of one of the principal lead-covered cables in the States, it says: "The fibrous "material, being superdried, will readily absorb and retain moisture "from the air, and cables showing an insulation resistance of "thousands of megohms when first made may after exposure of a "small part of the core to moist air for a few hours show a great "falling off of insulation resistance." Instructions are therefore given as to carefully sealing the ends of the cable when laying. Again, the splicers' hands are cautioned to be kept "perfectly dry, "and free from perspiration, as a little moisture communicated "to the insulation may result in vastly lowering the insulation "resistance." These remarks written by manufacturers of such cables show the readiness with which such fibrous insulation may deteriorate under conditions likely to occur even if there be the most careful supervision of the men laying the cables. It is acknowledged to be absolutely necessary to cover jute or cotton insulated cables with lead, and it is in this fact that the weakness

of such cables seems to lie, as the moisture- and gas-resisting portion is the lead absolutely, the jute insulation being of little value in this respect. Inasmuch as the majority of underground cables in the States are insulated with such jute or cotton soaked in some compound, it will be at once understood why lead-covered cables are considered in America as the only means for successfully carrying out such underground cable work, especially when the cables are liable to be laid in leaky conduits. It is right to say that apparently few troubles have been experienced in Europe with underground conductors insulated with fibrous material. Messrs. Siemens & Halske, who use a jute-covered cable, but manufactured in a different way both as regards the impregnating of the jute and the method of cold-drawn lead sheathing, have a successful record, not only for low-tension cables, but for the various concentric cables supplied to Messrs. Ganz & Co., of Buda-Pesth, for 2,000-volt alternating currents. They assert that the troubles experienced with American underground cables, whether jute- or rubber-insulated, are comparatively unknown.

The question between what I broadly term lead-sheathed jute or well-compounded vulcanised rubber seems to me the momentous one, and, as usual, each one has certain claims.

It is well known that the cost of jute-insulated cables is less than that of high-class vulcanised rubber insulation, but this would have little weight unless they can be proved in every respect equal to vulcanised rubber. In a comparison of the different costs of such insulation, vulcanised rubber appears to be about 25 per cent. more for approximately the same insulation resistance. In making this comparison I took the cost of a vulcanised rubber cable, insulated with one layer of so-called pure, and two coatings of vulcanised rubber, taped, braided, and coated with preservative compound, but not covered with lead, as this is a superfluity, the cable being water-tight without this addition. The jute-insulated cable I took as cased with lead, and protected again by a covering of tarred jute yarn compounded.

After the relative cost comes the question of jointing; and as this is one of the most vulnerable points in any cable, it is of

vital importance that the joints should be made as good as any other part. With vulcanised rubber cables the joints must be vulcanised, and first-class rubber men who would make such joints speedily should be on the working staff. With lead-covered cables a plumber would have to be employed, and I am informed by those who have had experience with both forms of cables that the vulcanising is preferable to the plumbing.

Vulcanised rubber cables are certainly easier to handle, being more flexible, ductile, and lighter, and this is of importance in view of the difficulties occurring in drawing cables in and out of a conduit.

Again, when lead-covered cables are coiled round a drum the part nearest the centre becomes compressed, while the outer portion is extended; on uncoiling the cable the opposite effect is produced, the parts before compressed being now pulled out, and the extended portion compressed. This is trying to an inelastic substance like lead, the more so when, unlike an empty lead pipe for water, it is filled with an insulated cable. It is quite conceivable that the surface of the lead may be slightly broken up, particularly when the method employed is to cover the lead hot round the cable; and although no cracks may be visible to the eye, gas and moisture will find their way in very quickly, with corresponding deterioration of the insulation.

Next, as to whether jute or vulcanised rubber is better suited for insulating high-tension alternating cables no opinion can yet be given. It is well known that on submarine cables working with reversed currents, it is usual at times to give the cable a rest. This necessity may be due to the action of the reversed current on the insulation, and it would be interesting to know how the much more powerful currents and rapid reversals of an electric light circuit will affect the insulation of a cable after carrying such currents for a considerable period. In some interesting papers published last year by Mr. Addenbrooke, he came to the conclusion that a high-tension alternating current should have less effect on any dielectric than a continuous current of the same potential. He argued that, owing to the rapid alternations, there would not be sufficient time to fully charge the

dielectric, as, before this could occur, the next charge would sweep out the former charge, partially recharging it in the opposite direction. But it would certainly appear that such an action as this on a dielectric would be far more injurious than for it to be kept in a fully charged condition as with a continuous current.

From a careful review of what I have seen in the States and elsewhere, I must confess that my own feeling is in favour of a cable insulated with thoroughly good vulcanised rubber for high-tension work; and I was interested to hear Sir William Thomson say on Monday, at the Board of Trade inquiry, that if high-tension conductors were protected in an iron pipe a lead sheathing to them was not essential.

Those acquainted with the present manufacture of rubber cables know that pure rubber proper is not used for their insulation. However good the rubber may be, there is always a certain amount of impurity and oils, which will set up decomposition sooner or later. This is overcome by slightly vulcanising the rubber, but so that there shall be no surplus sulphur to attack the copper. In the high-class cables now made by the Silvertown Company and others the practice is to put next the conductor after cotton twist a substantial coating of pure india-rubber, slightly vulcanised, upon this a coating of zinc rubber, with vulcanising materials, and covering this a compound vulcanised rubber of great resisting qualities to damp and gases. The zinc rubber takes up the surplus vulcanising material of its two outer coverings, so that the copper cable is not injured by the sulphur used in the process. Such an insulation as this is as good as could possibly be desired for any high-tension currents, and is absolutely perfect so long as the covering is continuous; and as the whole of the materials are treated at a temperature of 400°, any rise in temperature of the conductor due to accidental overloading will not injure the insulation. With regard to the outer protection of the compound vulcanised rubber, there may be still scope for further improvement; and, for instance, in the cables recently made by the Silvertown Company for the London Electric Supply Corporation, two braidings of asphalted tape have been

used, and this is found to be satisfactory. Among the instances where vulcanised rubber cables have stood stringent tests has been at Eastbourne, where Mr. Lowrie has used Silvertown cables, laid in iron pipes, for two years, and states they are still in perfect working order. He has also put down there a length of Fowler's Tatham lead-covered cable for continuous-current high-tension work, the fibrous insulation being well wrapped round several times with tape. This has also been working successfully; and in the House-to-House Company's station he proposes to try each method of insulation on different circuits, although the advantages of handling the india-rubber cables are in their favour.

The Silvertown Company have five miles of vulcanised india-rubber insulated cable sheathed with iron wires at Brussels, with 1,400 volts constant current. This was laid in the sewers two years ago, and is now quite satisfactory.

This company have also supplied two miles of such cable, but with asphalted tape on the outside, to the London Electric Supply Corporation, with 2,400 volts alternating current; the conductors being laid in an iron pipe.

There are also samples on the table of a piece of Messrs. Henley's vulcanised rubber cable that has been in use overhead for ten years.

A difficulty with underground conductors in the States seems to be disruptive discharges, and I trust the discussion may bring forth information on this subject. Owing to the frequent puncturing of the insulation, attended with pinholes in the lead covering, Mr. Acheson, the Waring Company's able electrician, and whom I desire heartily to thank, turned his attention to the matter, as the pinholes, by admitting moisture, defeated the object of the lead sheathing, and proved fatal to the life of the cable.

Mr. Acheson's theory is that the static electricity generated in the cable after a time punctures the dielectric by discharging itself between the copper conductor and the lead sheathing; and the protector shown on the diagram (Fig. 12) is designed to obviate this.

This consists of two metallic points separated by a distance

which is less than the thickness of the cable insulation. The one point is connected to the lead sheathing, and the other through a fuse wire to the copper conductor; the idea being that the static discharge would take the shorter path between the points

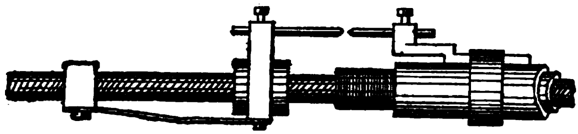


FIG. 12.

instead of rupturing the insulation. Mr. Acheson says these dischargers should be placed at every section of a cable, and if the sections are more than 200 feet long there should be one at each end.

To show the exceedingly minute path necessary for a discharge, it may not be out of place to quote Mr. Acheson's experiment. A plate of glass about one-tenth of an inch thick was broken in two pieces, and the two parts immediately fitted to their former position and clamped. The restored plate was placed between two discharging points, and so long as the points were over the solid glass no discharge was obtained; but upon removing it so that the line joining the points would lie in the plane of the fracture, a discharge immediately occurred.

A possible cause of a disruptive discharge might be that on high-tension circuits of great self-induction, as, for instance, on arc lighting circuits where lamp coils form part of the line, the induced current on breaking the circuit is very violent, and in preference to discharging through the line, and consequently the lamp coils, it gets away by rupturing the weak insulation, and passing through to the outer earthed casing.

The exemption from such trouble is credited by many entirely to a thicker insulation being used, and in calculating the insulation for a high-tension cable it should be made sufficient to withstand a disruptive discharge of this nature, and tested accordingly. For instance, owing to the almost daily occurrence of burn-outs with some cables in Chicago with $\frac{3}{32}$ insulation, laid in November, 1887, the four miles laid in December, 1887, had $\frac{4}{32}$

insulation, and the two miles laid early in March, 1888, have $\frac{6}{32}$ insulation. Careful experimental work is essential, before the sufficiency of insulation for such underground cables can be accurately determined, and our leading cable manufacturers naturally keep the results of such experiments to themselves.

Concentric cables, although so extensively used by Messrs. Ganz, of Buda-Pesth, in their stations, do not appear as yet to have met with favour in the States. It is, of course, undesirable to use ordinary lead-covered cables for alternating currents, both on account of the loss from induced currents in the lead sheathing and the annoyance to workmen handling them. If, for instance, while handling a conductor conveying such a current, the lead covering be insulated to any extent on each side of them, the workmen would receive the induced currents through their bodies. Again, when the two legs of an alternating current are in separate cables, and the cables laid in insulating materials, any chance contact or connection between the leads might cause the induced currents to pass backwards and forwards at these points, thus tending to cut away the lead covering as the result of any sparking at poor contacts between the two leads. The Waring anti-induction cable, with both wires enclosed in one sheathing, is well known, and in this the currents induced by both wires, being equal in power and opposite in direction, tend to neutralise one another.

Mr. Ferranti, by employing concentric cables for conveying the high-tension currents from Deptford Station, has brought such cables into prominent notice in England. A concentric cable is obviously the best on theoretical grounds for the elimination of induced currents, as their reaction upon one another is necessarily strongest in the axis of the current. The question as to the necessity of insulating the outer conducter of the concentric cable has recently had considerable attention given to it by Mr. Preece on behalf of the Post Office authorities, and, as a result of his experiments, they evidently must be so insulated. All the concentric cables made by Messrs. Siemens & Halske for Messrs. Ganz & Co. are of the usual stranded-wire form, and the outside conductor, after being well insulated with jute, is covered with a

cold-drawn lead tube, having an outer iron armouring as previously described. These cables have been successfully used in Rome, Tivoli, Turin, and Milan, in each case carrying a potential of 2,000 volts.

The difficulty of making satisfactory joints with concentric cables must be to a certain extent against them, as it is impossible to see what has been done inside. Messrs. Siemens say that it can be readily accomplished, and the illustration shows the manner in which the joints are made in their cables. The Silvertown Company have made several forms of concentric cables, highly insulated with vulcanised rubber, with considerable success, for ship work, and they also have an arrangement for constructing the joints.

As to whether concentric cables will be largely used in this country, much depends upon the system of distribution employed. For long lengths of cable used as feeders to sub-centres, or to systems of distributing mains, they would be of considerable advantage, as no joints would be required *en route;* but in house-to-house distribution, with frequent joints, a pair of separate cables near to each other would appear the more easy to handle and the more practicable to operate. In summing up this matter it would appear that the relative advantages are as follows:—1st. The greater convenience of making joints on separate leads, as against the trouble involved with concentric cables. 2nd. As regards the relative cost between two separate cables and concentric cables with the same insulation resistance, vulcanised rubber unsheathed concentric cables with outside insulation are dearer, but with jute insulation for concentric cables and lead sheathing they are cheaper. 3rd. There remains the increased efficiency on an extensive system of alternating currents by the use of the concentric form.

With regard to the second point, the figures showing these are:—

For vulcanised rubber, two Silvertown $\frac{19}{16}$ conductors, insulated to 5,000 megohms at 60° Fah., taped and braided over all, specially prepared for underground work, £255 per mile each = £510.

A concentric conductor of equivalent capacity, the internal conductor insulated to 5,000 megohms, as above, but the outer to 3,000 only = £900, being a difference against the concentric conductor of £390 per mile. If the outer insulation is reduced to 1,500 megohms, the price falls to £630 per mile, which is £120 above the cost of separate leads.

On the other hand, for Messrs. Siemens's specially prepared jute insulation two single conductors of same insulation, at £220 per mile each = £440.

A concentric conductor of equivalent capacity, the outside insulated to 900 megohms, lead-sheathed, taped, and asphalted, £363 per mile.

All cables for high-tension currents should be well tested before leaving the factory. In other branches of engineering the engineer would not think of receiving his steam or hydraulic pipes without first seeing them tested to, say, over twice their working pressure; it is not sufficient for him to see simply that they will hold water or steam, but he must satisfy himself that they are strong enough for the pressure they have to carry. In the same way the electrical conductors, which have to transmit energy much as the steam and hydraulic pipes do, should be tested to twice their working pressure. With the outside well earthed the current should be allowed to traverse the conductor a considerable time, and an electrical test taken afterwards for insulation. With electrical cables for underground work passed in this way, and satisfactory in other respects, there should be no fear of breakdowns under ordinary conditions.

In dealing with a subject of such wide extent and great importance as this it is impossible to do justice to all the workers in the field. I have left out much that may be considered interesting and important, but the difficulty has been, with the mass of information on the subject, what to put in and what to leave out. Doubtless there are many instances of cables and conduits having been successfully at work which have not been mentioned in this paper, and I hope such information will be forthcoming. If the discussion following be the means of clearing up some of the doubtful points surrounding the subject of under-

ground electrical work, and of demonstrating the direction in which successful work should be undertaken, the object of this paper will be fully accomplished.

Professor Ayrton.

The CHAIRMAN: Although Mr. Verity is unfortunately absent, his assistant, Mr. Girdlestone, is here, and will answer any question that he can; though, of course, the main body of questions that arise in the discussion will be dealt with by Mr. Verity in his reply, which will be published in the Journal.

I now invite discussion or comment upon the paper. There are representatives of the Silvertown Company, and probably of other cable manufacturing firms, present, and no doubt they are desirous of saying something on the subjects concerning them which have been referred to in the paper.

Mr. W. E. Gray.

Mr. W. E. GRAY: I have listened with pleasure to the interesting paper we have had this evening, and agree, with regard to the cables, with Mr. Verity on many of the points he has raised; but as this question of underground cables has so recently been discussed, I do not think I have anything to add at present which might be of interest.

Mr. Verity appears to consider it to be a question as to whether lead-covered or vulcanised india-rubber cables are the best. Naturally I believe in vulcanised rubber, but would prefer not touching on this matter now, but leaving it to be discussed by others.

As to conduits, Mr. Hurlbert's idea, judging from his flexible joint, appears to be to prevent moisture getting in. It seems to me that any system laid underground which is dependent upon the conduit itself excluding moisture would be a faulty one. On such a matter we might appeal to the Post Office for their experience in laying wires underground, or to the gas companies. I believe that hitherto, in spite of the ingenuity of many inventors and improvers, it has been found impossible to keep moisture out, or, at all events, moisture to such an extent as would materially affect any question of electrical insulation. It should be borne in mind that very little moisture in a fibrous material might easily constitute a serious fault, owing to failure in insulation; and however good, mechanically, the idea may be, still I

Mr. W. E. Gray. think that, if the insulation depends on the flexible coupling or joint remaining water-tight, it is rather leaning on a broken reed.

Mr. Verity refers to the work done by the Western Union Company. I have only a slight knowledge of what has been done by them, but understand that the conclusion they have arrived at is that New York is built upon an exceptional soil, in which there are many free gases and oils, and that the trouble they have had is in a great measure to be attributed to this cause. For this reason it has been proposed that the insulated cable should be covered with lead, the insulation to be used being vulcanised rubber. In speaking on this point it should be borne in mind that when in America the word "rubber" is used, in many cases "pure rubber" is meant, and not always vulcanised rubber, as Mr. Verity appears to have understood it.

Reference has been made to the "Johnstone conduit." In the very interesting discussion at the Chicago Convention, Mr. Johnstone spoke of this conduit as a success. Mr. De Camp, I think it was, did not seem to think that this success (at least in Philadelphia) was very marked, as, when the conduit was taken over, it was found to have been broken up in many places, and there appeared to be a difference of opinion between the contractor and the working company. I do not know if Mr. Johnstone has made any recent improvements since then, but the results obtained were reported as hardly satisfactory.

The curb and gutter conduit appears to be substantial and good, if the expense of relaying the pavements could be incurred. In the City of London and other large cities this expense would be enormous; and it is improbable that to have the cables put underground, the authorities would consent to have the streets pulled up to any great extent, otherwise the idea would appear to be good and workmanlike.

With regard to Mr. Irish's conduit, is the idea to keep the conduit water-tight? If so, I think that it is a mistaken one.

The Doulton pipe conduit has a nice appearance; but conduits cannot always be laid at any great depth in cities, and there would be a great risk of their being crushed by heavy traffic. In addition to this, as Mr. Verity says, they have the serious

objection that the pipes require to be broken to put in a branch joint; the system therefore appears to be hardly practical. Mr. W. E. Gray.

I am very much interested in hearing of the success that Messrs. Siemens & Halske claim for their cold-drawn lead-covered cables, and think that the electric lighting community is much indebted to Messrs. Siemens for the way in which they have tried to make electric light cables cheap, and, as they claim, good. The vulcanised india-rubber cables are, in some cases, not so cheap, but I believe them to be better; this is, however, a matter of opinion, which must be left to be decided by time.

I have to thank you, Mr. Chairman, for the opportunity you have given me of making these few remarks.

The CHAIRMAN: Of course you have to remember that Mr. Verity commenced his paper by making the statement that overhead wires would certainly soon have seen their last day. I do not think he is quite right, and it is open to any members to consider whether in London we ought to go in for underground or overhead wires. The paper is not merely on conduits, but also on electrical conductors, which is a very important question in the lighting of new towns. Professor Ayrton.

Mr. A. P. TROTTER: It has been objected that lead-covered cables may, and sometimes do, deteriorate when placed underground, becoming carbonated, or otherwise decomposed; but it is well known that a considerable number of specimens of lead pipes have been found which have been put down by the Romans. These pipes have suffered little or no deterioration, and the reason undoubtedly is that they were not composed of pure lead, but of an alloy. Unrefined lead contains antimony, tin, and silver, all of which tend to harden the crude metal, and such an alloy would doubtless resist the corroding action better than the lead of commerce, which is often nearly pure. The nature of the soil, and of the water in it, exerts powerful influences on the character and the extent of the chemical changes. The extreme pliability of pure lead is not required; and if a suitable alloy will better resist corrosion, a slight increase of hardness would be no disadvantage, provided that it allows the tube to be drawn through dies, for it is obvious that the old method of coating the insulated conductor with molten lead was certain to produce pinholes and other flaws. Mr. Trotter.

Mr. Trotter. The use of the lead pipe should be to keep the insulator thoroughly dry, and to protect it from mechanical injury. The insulator need not, therefore, be expensive, as no rubber need be used. Lead-covered cables received very severe tests before the Admiralty allowed them to be used in ship work, for which they are very well suited. If a lead-covered conductor could be laid simply in the earth, protected, perhaps, by a board, the expense of the complicated and costly subways and conduits might be avoided, and this would allow a good deal more to be expended in making a thoroughly sound conductor. It would be a great convenience if such leads could be plastered into the walls of newly built houses, but pure lead is rapidly corroded by calcic sulphate in the presence of moisture.

Mr. Shoolbred. Mr. J. N. SHOOLBRED: In the following remarks on this very interesting paper, I wish to touch, not so much on the electrical merits of the various conductors, as on the mechanical contrivances and the conduits which Mr. Verity has presented to us.

The first question that presents itself to the minds of many —and it certainly does to myself—is whether, in the central portion of the average of towns, such as we meet with in this country, a continuous conduit is possible. My own experience would lead me certainly to answer the question in the negative, considering the number of main pipes that are running longitudinally under the causeways or pavements—gas, water, telegraph, and telephone pipes—and then, again, the transverse services that go into the houses, with their valves often at different levels. There are also the lamp-posts that project inside the kerbstone, often requiring the cables to go outside under the roadway. Taking all these things into consideration, one may be very fortunate in some cases if a length, on an average, of 20 ft. of anything like a direct line on the horizontal can be got for the cables. The differences of level are very considerable, and often succeed each other very abruptly; the cables coming across large pipes and small pipes, and having to thread in between pipes where only 2 or 3 in. clearance, in level, occur between them. A case was brought to my notice, some time ago, where, in crossing a brook which was covered in, there were eighteen cables,

many of them as much as 2 to 3 in. in diameter, and these were passed eventually in a trough; the whole space available being about 2 ft. 6 in. wide, and only 5 in. deep. There are perhaps a few towns where it may be possible in the suburbs; but, judging from the difficulties that one sees and hears of, I fancy the conduit has yet to be devised that will afford a good and regular protection for the conductors. Mr. Shoolbred.

With regard to some of the conduits that Mr. Verity has brought before us. In the first place, there is his own conduit, which, I understand, is a solid block of earthenware with longitudinal circular perforations or tubes running through it for naked copper conductors to pass along. The surface of these tubes being of glazed earthenware would tend to condense moisture and become practically small sewers for the passage of water: the effect of this moisture upon the naked conductors would certainly not conduce to improve the state of the insulation.

Then, again, with regard to Mr. Irish's conduit. It may answer its purpose, possibly, in some places; but there is, I think, one nearly insuperable objection on the part of the local authorities that Mr. Irish would meet with, viz., the use of a continuous iron cover-plate along the pavement. Municipal bodies object very strongly to iron plates being multiplied in their pavements; they say, and I think very fairly, that iron plates become slippery, and cause many accidents to foot-passengers.

With regard to the Doulton conduit, I am afraid, as has been pointed out by Mr. Verity himself, and also by Mr. W. E. Gray, that it consists of perishable materials, liable to cause fractures in the pipe itself, or else at the joints. Besides, if the joints are good, and are made with good cement, the pipes will have to be broken whenever new connections are required; or, if the joints are not good, then leakage will occur at them. So that I doubt very much if such a conduit would prove by any means an efficient or economical mechanical protection.

To my mind the main and most important question to consider—and it is one that must be the outcome of experience, from which we have yet a great deal to gather—is whether a conduit of

Mr. Shoolbred. any length at all can really be relied upon. Even, for instance, at Kensington, where the streets are comparatively open, I do not suppose that anything like a 100 yards of straight run can be obtained for the brick conduits that Mr. Crompton advocates so strongly, with naked copper conductors. Mr. Crompton himself has stated that he has had to have recourse, at intervals, to ordinary cables. Side streets, which cause these interruptions, occur much more frequently, as a rule, than at every 100 yards.

Another very important point has attracted the attention of many persons, and was pointed out very strongly in this room by General Webber some time back, but which is often overlooked, viz., that the cable should be accessible throughout its entire length with great facility. If it is put into a protecting conduit which is difficult to get into, that conduit will have to be broken into, at considerable expense, in order to get at the conductor to make side connections, or to remedy defects.

These points lead me to think that under most circumstances, and in the busy parts of towns, a cable that is perfectly sheathed, and able to take care of itself, placed underground, best meets with the requirements and difficulties of laying, and at the same time complies with the other question of accessibility for jointing, or for whatever else you may want, throughout its entire length.

Mr. Erskine. Mr. R. S. Erskine: With reference to Kensington Court, it may be useful if some information is given with regard to our conduits there. It has been remarked that we have difficulty in getting a considerable straight length. The longest run we have actually at work, without a break, at the present time is 800 yards; we go perfectly straight along. We have recently laid down another length of 400 yards, and in that case we certainly came across one pipe which was rather awkward, but we had that removed to one side. The great advantage of working under the Electric Lighting Act is that one gets power to ask the water company—at our expense, of course—to move their pipes, as also the gas and other companies, if they come in the way so as to interfere with the working of the system; so that if it does not interfere with the water company at all to move their pipe 18 in. on one side or the other, we can get it done. If their

pipes cross ours at an awkward level, we can get them lowered or raised, as the case may be; and so almost always, provided there are no cellars coming up to the pavement, we can get a straight line down the street. The only cases in which we get interfered with in our culvert are where the cellars have been built very nearly up to the pavement, and that is very seldom. In such cases we put in 2-in. iron pipes and take cables through until we get more space, and then we start the culvert again. We have tried two or three different sorts of iron pipe, and have come down to the 2-in. owing to the ease with which we can put them all on one level, or we can put them one above another, according to the arrangement of the pavement. We do not trouble very much about the joints being water-tight, because we find that condensation takes place in the iron pipes to such an extent that it really does not matter whether the water gets in from the outside or not. In fact, under a pavement you very seldom get any leakage through joints, because the pavement itself is made water-tight, and so you are protected. In the case of big pipes under the roadways, if they have to be used, they very likely have to dip down, and we find that water, no matter how careful we are, will always collect at the lowest point; and in some cases we have left an open joint to let the water drain away into the ground. Mr. Erskine.

Mr. J. N. Shoolbred: May I ask a question before Mr. Erskine sits down? I think he has misunderstood my allusion to Kensington Court, unless I have misunderstood Mr. Crompton himself. Does Mr. Erskine mean to say that the length of 800 yards he spoke of just now is continuous bare copper conductor from end to end?

Mr. R. S. Erskine: Yes.

Mr. J. N. Shoolbred: Not broken up by lengths of Callender's cable?

Mr. R. J. Erskine: No. We have cables in places, and we have considerable lengths of Callender's cable through sewers, but most of our cables are short lengths. We hardly ever put a cable down now, but nearly all bare copper. Most of our cables were fixed before we had given Mr. Crompton's system a fair trial;

Mr. Erskine. but since we have been able to see exactly how it works,—and Mr. Crompton has made great improvements, which get over almost all difficulties,—the arrangement has been adopted as the standard system of mains to be laid down by the company.

Professor Ayrton. The CHAIRMAN: We have a little more time for questions or discussion. We cannot take the reply this evening, but it must be given afterwards, and I invite any further questions.

Perhaps I might ask one or two questions myself. I should like to know, if Mr. Gray could tell us, why the india-rubber cables of the earlier days occasionally failed. I am not speaking, of course, of the Silvertown make specially; I have rather in my mind the cables which were made by Messrs. Hooper, of vulcanised rubber. They were constructed very much in the same way that Mr. Verity has described, of nearly pure rubber, put round copper. The copper was tinned, as a matter of fact; then came a separator, as we called it in those days, of india-rubber with zinc oxide; and outside was a coating of vulcanised india-rubber. The insulation obtained at first was extremely high—far higher than was the specified insulation—but, for some reason or other, I believe, I am right in saying that occasionally, at any rate, the insulation used to go down. A good many years ago I tested many thousands of miles of cables made in that way at Messrs. Hooper's works at Mitcham, and the insulation, when the cable was tested at the works, and also after it had been sheathed at Millwall, was far higher than had been specified for. I do not know exactly what happened, as I went to the Far East just then; but the cable was subsequently laid on the East coast of South America, and I believe some difficulty was experienced on account of the insulation going down. Possibly Mr. Gray can give us some information on that point.

Another curious experience I had on a previous occasion with a cable made in the same way by Messrs. Hooper. There were some cables lying in iron tanks, with water in the tanks, in Calcutta, for a long period. Those cables were tested from time to time, and found to be extremely good. Then they had to be moved and taken to be laid under the Hooghly; and after being laid, although there was no evidence of their being

damaged in any way, the insulation of all of them had gone down very much indeed. I am speaking of about 1870, or possibly a little earlier. I tested those cables before they left Calcutta, and I tested them while they were being laid, and directly they were laid, and the insulation of all—I think there were six—went down, so that it is certain it was not due to a single fault in any one specimen. From some reason the material had deteriorated. Possibly uncoiling the cable out of the tanks at Calcutta, removing it and laying it, developed minute cracks in the insulating material. Probably some change has been made at the present day in the method of construction to prevent the material deteriorating in this way. Professor Ayrton.

The question of the puncturing of insulated cables used for electric lighting is an extremely important one, and one which I think has not had sufficient attention given to it. It has been assumed that the electro-motive force that the electric light cable will work at, is the electro-motive force produced by the dynamo, and no more; but, as Mr. Verity points out quite rightly, in his paper, there is the probability of a very much higher electro-motive force being brought to bear on cables used for electric lighting, in consequence of self-induction—a subject which will come before us in another way at our next meeting, when Professor Oliver Lodge is to give us a paper on lightning conductors. The surging backwards and forwards that he has so ably drawn attention to, that you have in certain cases in electrical conductors, produces an electro-motive force infinitely greater than you would expect. A case was brought to my notice the other day of a somewhat extraordinary character. Two people were walking together in the Inventions Exhibition, along a court where there were a good many electric wires; they were several feet away from the wires, which were overhead; they were walking on wood; and they both say that they simultaneously got a sharp shock. Of course, from our old point of view, we should have said it was quite impossible, because the insulation was only working with—I do not know what it was, whether it was 100 volts or 200 volts; but they could not have received a spark of several feet with a potential difference of even a

Professor Ayrton. few thousand volts. At the same time, you have their evidence that they independently felt a shock at a certain moment, and said immediately to one another, "I felt a shock." They applied to the people in authority, but of course they could get no information.

Now, with the illustrations that we have had recently shown us by Dr. Lodge at the Royal Institution the other evening, it does not seem improbable that you may have potential differences set up in wires far greater than the potential difference that the dynamo can produce, due to the sudden stopping of the current: for example, whether you may be working at 100 or 1,000 volts, you may have 10,000 volts, or even more, produced. I think that that may explain the breakdown of some of these cables. Cables carrying telegraph currents may last for a long time where there are not such high voltages used, because the amount of energy available is not nearly as great. I do not wish to detain you any longer; but if Mr. Gray, or any other representative of the india-rubber and cable industry, could give us some information as to why the cables are more likely to last now with vulcanised india-rubber cables than they lasted in the past, I for one should be extremely grateful.

Mr. Gray. Mr. W. E. GRAY: I do not know whether there are any other cable makers present; but you have, Mr. Chairman, touched on rather a delicate point, I think, by asking the Silvertown people to discuss and criticise the failure of Hooper's cable. I think we could hardly be expected to do this.

Professor Ayrton. The CHAIRMAN: I do not, of course, want to ask you to go into any sort of commercial difficulty; but is it not possible, without touching on commercial questions, to suggest some explanation which would give confidence to those who have used india-rubber cables and found them fail? That is what I mean.

Mr. Gray. Mr. W. E. GRAY: There is one thing you have mentioned, Sir—that is, the fall in the insulation of these cables. I do not propose to go very fully into the matter, but I may say that in ordinary cables, as probably you are well aware, this depends a good deal on the quality of the rubber used. As Mr. Verity has explained, rubber can be got at 1s. per lb., and also at 3s.

per lb.; the reason for the higher price is the greater durability of the higher class, as, if the lower grade were used, although you might even get as high an initial insulation as with rubber at the higher price, this insulation would not last. I think that is well known to rubber cable manufacturers. As regards the question of the Hooghly river cable, I do not know what the temperature may be there, but presume it is greater than the ordinary temperature in England, in which case the cause of the fall might be traced to the fact that, if the material were imperfectly treated, any decomposition would probably be increased and accelerated by the increase in temperature. Of course I cannot tell you why Hooper's cable did not last, but can only say that the explanation would apply to rubber cables in general. The failure may be due to altogether different causes, and I can only regret that one of Messrs. Hooper's staff is not here to explain it to you. Generally speaking, there is as much difference between rubber and rubber as between leather and leather, or, indeed, any other material; and this should be taken into account in the specification. If the consulting engineer understands the material he should be able to get a good thing; but if he has not this knowledge, and throws the matter open to competition, the manufacturers would probably only give what was asked for in the specification. As an illustration, I can quite suppose that when buying telegraph wire, the Post Office, knowing that there are various qualities of this, would hardly expect to get for £8 per ton the very best telegraph wire; and their specification would be mechanical as well as electrical. The same thing applies to rubber as to anything else—it is merely a question as to what people will pay for, and the durability is to a great extent a question of price; but there is no doubt whatever that a good cable, manufactured of good material, properly and uniformly treated, and well vulcanised, will last. There are, I believe, in existence now, rubber cables that have been working for twenty to thirty years, that have lasted, and will last for some time yet. Mr. Gray.

Mr. GISBERT KAPP: The author said that he had not dealt with several other systems, and I think we should all be glad if Mr. Kapp.

Mr. Kapp. in his reply to the discussion he would amplify his paper in this respect. In particular, I should be glad if he would add some facts upon a system of which a good deal has been said in America, viz., that of oil insulation. No doubt you are all familiar with the experiment that Mr. Brooks performed some years ago. He twisted two ordinary cotton-covered wires together except at the ends, which were kept 1 in. apart; the wires were then put into a jar filled with paraffin oil, so that only the separate ends projected above the surface of the oil. The wires were then connected with an induction machine, and sparks passed in air from one wire end to the other, but no sparks passed through the cotton covering and oil where the wires were twisted together. Mr. Brooks has employed this discovery of the enormous insulating property of oil, in the construction of an underground system. An iron pipe filled with some cheap mineral oil forms the duct, and into this are placed the conductors, which, I believe, need no other covering than cotton. If this system should prove really so successful as its inventor believes it to be, it would be of the greatest importance to central station lighting in this country, and I am rather disappointed to find no mention of it whatever in Mr. Verity's paper. If he has seen the Brooks system in actual operation in the United States, I would ask him to give us in his reply some facts about it, obtained from his own personal observation.

Mr. Phillips. Mr. C. J. Phillips: As I was in Messrs. Hooper's service at the time they were making the Western Brazilian cable, which is the one referred to by the Chairman, perhaps I may offer one explanation. With regard to that cable, I think you are aware, Sir, that the greater part of it was originally designed for use as a deep-sea cable, and that it was afterwards laid in shallow water as a coast cable. This fact, I think, to a great extent accounts for its comparative failure, the type being unsuited to the locality.

With regard to the quality of the material which was used in the insulation of the cable, I think at that time, Sir, you were with Sir William Thomson, who was the consulting engineer of the Western Brazilian Company, and I think his duty was to see that the materials used were according to specification. I have

no connection with Messrs. Hooper now, but I certainly know from my own experience that they used the best rubber that could be procured, and that any failure in the insulation is not to be accounted for in that way. I believe now that a rubber cable, as Mr. Gray says, "properly made," is perfectly reliable. Mr. Phillips.

The CHAIRMAN: I was going to say that I do not think Mr. Gray has hit the right nail on the head by implying that the failure of the cable was due to cheap material being used. I also have nothing to do with Messrs. Hooper now, or then, and I have no interest therefore in supporting any particular way of making cables, but I think that Messrs. Hooper were quite as much astonished as anyone else at the subsequent falling off in the insulation of their cables. I would like to add one other point, and that is, would Mr. Gray point out what should be the test of the insulating quality of a material if a great number of megohms resistance means nothing? What would he propose should be the test? Should it be a chemical test of the rubber? or what should be the test in passing a cable, if the fact that it had a far higher insulation resistance than could be expected in a cable, did not show that it was made of good india-rubber? I think, in the case of iron, that if the iron fulfil all the mechanical tests of tension, torsion, elastic limit, &c., required of good iron, it is really all the Post Office, or anybody else, would want, whatever the iron be called, however many "B.B.B.'s" or "Best Bests" may be put before the name. Professor Ayrton.

Mr. W. E. GRAY: I am sorry to occupy the time of the meeting with what appears to be rather a conversation than a discussion. A gentleman has just spoken who says he was with Messrs Hooper at the time the Brazilian cable was made. I hope he did not think I was implying anything derogatory to that firm. Mr. Gray.

Mr. C. J. PHILLIPS: Not at all.

Mr. W. E. GRAY: I certainly did not mean to do so in the slightest degree. I simply tried to give an explanation that might account for failure in rubber cables; but it would be necessary to know all the conditions of the specification before one could judge as to why the cable failed. I do not know the

Mr. Gray. specification, and I certainly could not undertake to give the reason to you, Mr. Chairman, who tested the cable, and had a full knowledge of the specification.

You, Sir, asked me how to make rubber cables. This is rather a long process to explain to you, and although I would be glad to have a private opportunity of doing so, I do not think that the present occasion is suited for going into details of manufacture. There is no doubt high electrical insulation can be easily obtained, but the practical question is the *durability* of the cable. It is hardly sufficient to make an electrical test, and, because the instruments give a good reading, be satisfied with the cable. The question of the time and temperature would have to come in. I do not know whether the cable you refer to was tested at a high temperature, or what the duration of the test was; but it seems to me that the whole conditions of use of any cable should be taken into account in the specification, and suitable tests applied. If permanency of insulation is required, then the manufactured material used should be such as to give that permanency, and precaution should be taken to get this; but it appears to me to be a matter of judgment and knowledge of the material used.

It is impossible to go into all these points at present, but I should be glad to give you any detailed information you might require, outside of this meeting.

Professor Ayrton. The Chairman: I will accept your kind invitation after the meeting. I venture to say that neither a month's test nor a several months' test shows the going down in the insulation. During the whole manufacture of the Hooper's cable I have been referring to—it had all to be sheathed, of course, and it took many months to make this sheathing—there was no evidence whatever of the insulation going down. Not only did the many tests of each section of the core, but the constant tests of the sheathed cable before laying, failed to show any suspicion of faults. In the case of the Hooghly india-rubber cables, certainly a year's testing did not show what the coiling and uncoiling of the cable at once brought into evidence.

Mr. Farquharson. Mr. J. Farquharson: Gentlemen,—I think our Chairman raised a very important question, that requires a very much

fuller answer than it has had, viz., how we shall know india-rubber that is worth 1s. a pound from that which is worth 3s. a pound. Now I have had something to do with india-rubber in my time, and I will tell you the test that I adopted. I do not know that Mr. Gray will thank me for it, but it is a test that has been continued by the Admiralty for the last twenty-five years, and there should be no harm in my telling you here what it is. The test that the Admiralty adopt as a test of the quality of india-rubber is this: They take a portion of it and put it in a steam chest under a pressure of 60 lbs. of steam (about 300° Fah.), and keep it there for three hours. It is then removed and put in dry heat for two hours at 270° Fah., and then allowed to cool. If it is then soft, and bends without cracking, the quality is good. This is a test for the quality of the rubber. The proportions of the constituents should be specified and tested by analysis. I may add that the sulphur should never exceed 3 per cent. The oxide of zinc need not be less than 40 per cent.; and if to be used in a soil or for a purpose where it may be subject to mineral oil, the oxide should be at least 60 per cent. Mr. Farquharson.

Mr. GEORGE SUTTON: I do not think there was anything with regard to the cables laid on the east coast of America that would show that the material was of poor quality. I have met with many who were connected with that expedition, and I always understood that the failure of the cable was owing to its being destroyed by marine animals, especially the saw-fish. A tooth of a saw-fish was found embedded in the cable, having got right through the sheathing to the core. Mr. Sutton.

My Company have made rubber cables which have been laid in many parts of the world, and have given no trouble. No doubt gutta-percha has practically superseded the rubber cable. We have to go into the question of expense, and hitherto nothing has been found to serve so well and so cheaply as gutta-percha for submarine work generally. Rubber cables have been for years more expensive than gutta-percha, and the question of cost is a very serious one when it is a case of laying, say, a thousand miles of cable.

Major-General WEBBER [*communicated*]: I have to thank Major-Gen. Webber.

Major-Gen. Webber. Mr. B. Verity for his interesting paper, as it reminds me that I received from Mr. Leonard F. Beckwith, chief engineer of the Consolidated Telegraph and Electrical Subway Company, of New York, so long ago as early in February last, a very detailed account of the electrical subways built within the last three years in New York City.

Mr. Beckwith gave me a description of what is known in America as the Dorsett system, and its application; of the creosoted wood conduits; of the iron and cement pipe conduits; of the conduits of iron pipe laid in asphaltic concrete; of conduits of iron pipe laid in hydraulic cement concrete; and of the Johnstone cast-iron conduits.

Our correspondence arose out of the fact of Mr. Beckwith becoming aware that I had for several years had my attention turned to this subject, and that the result of practical experience, dating since 1870, in laying underground electrical conductors, and many experiments since 1882 (when Edison's system was exhibited at the Crystal Palace), appeared in the arrangement of conduits known as the Callender-Webber system, exhibited two and a half years ago at the Borough Road Works of the Anglo-American Brush Electric Light Corporation.

I was glad to hear from Mr. Beckwith that nothing precisely in the same form had been tried in the United States, because, with the rapid extension of underground work owing to the demand for electrical distribution in that country, it might have been thought that it had been used and proved a failure. When I say "precisely," I mean that no cases or conduits had been made or laid down consisting of the same material or made in the same way.

As to priority, or as to our having much to learn, I would once more beg to be allowed to remark that the history of this subject in connection with telegraph conductors, in spite of the many warnings of telegraph engineers, almost exactly repeated itself in connection with the laying down of conductors suitable for electric lighting, and, I am sorry to say, in spite of those warnings, is likely to repeat itself again.

Early in telegraph days the conductors were all sealed into

the pipes. Bitter experience led to the *drawing-in-and-out* system, in which even insulation is secondary to the perfect facility for removing and replacing a defective cable. Major-Gen. Webber.

All the troubles from steam, heat, temperature of the soil, gas and water, danger of damage from other works, difficulty of finding room under the road, were encountered and conquered long before 1880. The only vital change in the *principles* and *practice* then learnt, that I know of, has been *that with large cables each conductor must be provided with a separate "way."*

The chief feature of the Callender-Webber casing is that this is provided for in a more simple and less expensive manner, I believe, than with any other material; at any rate, such was the case three years ago, when the plan was first thought out.

The arrangement most similar to it, amongst those referred to, I will describe in Mr. Beckwith's own words:—

"This construction consists of lap-welded wrought-iron pipes, "the different sizes varying from one and a half to three inches, "according to requirements, laid in tiers in hydraulic cement "concrete. The concrete is from an inch to an inch and a half "thick between the pipes, and three inches thick around the "mass of pipes. The pipes are from eighteen to twenty feet long, "with screw coupling joints. The concrete has due proportions "of cement and sand."

The work of laying down this conduit can easily be specified by any engineer, and I will not give its details. Its advantages are manifest in its strength, in being impervious, in the easy multiplication of cable-ways, and in its great durability. But it cannot compete for one moment with the following advantages in the use of the bitumen concrete casing I have alluded to, namely:—

The occupying of small space, and space of different sections.

The being able to bring the casing to the work ready made, saving the time during which streets have to be opened.

The lightness and cost of the material, which, in the case of the bitumen concrete casing, is about one-half per lineal measurement of tube.

Major-Gen. Webber.

The power of being able to bend the casing on the spot to suit the obstructions previously unforeseen.

The ease with which the material can be cut, and a house service-box put in.

The smoothness that can be obtained in the interior surface of the ways.

The other day two 1,800-feet cables were drawn in at Chelsea with great ease.

Each plan, no doubt, has some advantages over the other; but for the same cost the former cannot compete with the latter, if the great end of being able to draw in and out is essential. In view of the opinion expressed in favour of this condition by the Board of Electrical Control of New York, and by the Municipality of Paris, and elsewhere, this can hardly now be doubted.

That each work may be carried out so as to give bad results I have no doubt; and as regards the laying down of the Callender-Webber casing, I have none whatever that failure will infallibly follow, and facilities for maintenance in the future be jeopardised, unless the most evident precautions, and others less apparent, are followed.

In connection with conductors destined to be used with high pressure, I am all with Mr. Verity that its maintenance will depend on the quality of the insulation, and not on the comparative insulating properties of the material of which its surrounding conduit is composed. With perfect facilities of renewal, the latter is a condition to cause no anxiety.

One word as to leakage. To jointing I will not refer more than to remark that if electrical engineers studied the subject, and taught their men as well as we were instructed and taught at the Postal Telegraph Works at Gloucester Road, under my old friend Mr. Andrew Bell, we should have less to fear in that direction. In simple jointing and connecting safety will come—come in time.

But the really serious causes for leakage lie in the accidental, local, and varying conditions which come into existence at unforeseen points along the line, arising out of the insulation being attacked by gases and liquids present or percolating in the surrounding soil. These will be the bane of the electrical

engineer of a supply system, and their frequency will increase with the life of the plant. To the practical prevention of their results he must direct untiring attention. Major-Gen. Webber.

The CHAIRMAN: I am sure we are all extremely obliged to Mr. Verity for having brought this paper before us, and that we cannot do better than give him a hearty vote of thanks for the paper, and express our great regret that illness should have deprived us of his presence. Professor Ayrton.

The motion was unanimously carried.

Mr. VERITY [*communicated*]: Since reading my paper I have received from America some samples of Edison three-wire conductors and junction boxes. I should have been happy to show these to the members had it been possible to devote another evening to the discussion; but since it is not possible, I am only able to show you illustrations in place of the actual things. Mr. Verity.

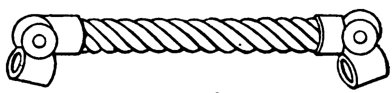


FIG. A.

FIG. B.

Fig. A shows the flexible copper connections used throughout the system at all junction boxes. Fig. B shows a section of the three conductors in the iron tube, made in lengths of 20 feet, each length joined with the flexible connectors (A) in the joint box, which is then filled in with insulating compound. The flexibility of the system is maintained by the joints being all of the ball-and-socket form, giving a limited movement, but quite sufficient for all requirements. Fig. C shows a T junction box, and the method of making connection with the flexible connectors (A) to a branch for house service. The whole service is very complete, and has been invariably successful in America, although, so far as I know, it has only been used for currents of less than 300 volts potential.

With regard to earthenware pipes, I have been informed that the troubles arising from branches and roots of trees getting into

Mr. Verity. the joints is thoroughly overcome by using a good cement joint, and that where these troubles have arisen only clay has been used for filling up the joint space.

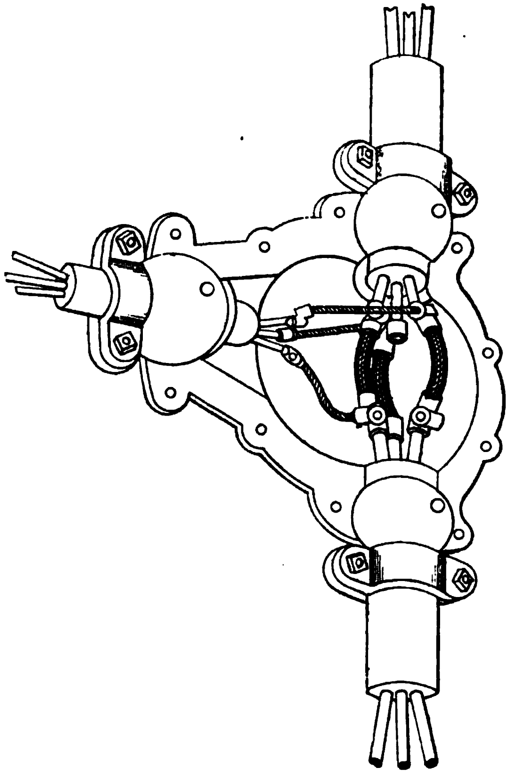


Fig. C.

Mr. Shoolbred has spoken of Mr. B. Verity's conduit as one in which a large amount of condensation would take place, and has assumed that its efficiency would be impaired thereby, but I do not think this is so, for even if all of the channels were wet it does not affect the insulation so long as they are kept separate, which is distinctly a part of the system.

The diagram, perhaps, did not show the details very plainly, as proper means are provided for draining the ducts into a trap and getting rid of the accumulated water. Provided the joints are properly made and the conduit laid on a fair bottom, it would practically last for ever, as I do not know anything that destroys

glazed stoneware (except main force, which would be required in rather a large quantity for this purpose). The real objection to such conduits are their want of flexibility. Mr. Verity.

In reply to Mr. Kapp, I had the pleasure of twice meeting Mr. Brooks in America and hearing his views on this subject, and a description of his particular system; but I regret to say my opinion was, at any rate so far as electric lighting is concerned, that the system was not a practical one; and that seemed the general impression there. However, as an English firm have taken up the patents for this country doubtless they have satisfied themselves of its practicability, and we shall hear more of it. The conduit consists of wrought-iron pipes supplied with suitable splice boxes, hand holes, and outlets; these boxes are protected from oxidation by being laid in a wooden trough into which hot pitch is poured. The wires used in this system are cotton covered, and stranded into a rope or bundle, which is then covered with a textile weaving; the bundle of wires is then soaked in hot mineral oil drawn into the pipe, and a heavy mineral oil is forced in for the purpose of excluding moisture and maintaining the insulation. In towns where there is much difference of level, the head of oil in the pipes would be a source of trouble, as may be easily imagined. The experiment mentioned by Mr. Kapp, as showing the efficiency of the oil insulation, is thus described :—Two wires were attached to a Holtz machine, the extremities being carried into a narrow jar of the insulating oil in such a way that when the distance between them was one quarter of an inch in the oil they were one and a-half inch apart at the surface of the oil; on turning the machine, the spark passed above the surface of the oil and not at all through the oil, even when the distance between the extremities was reduced to one-sixteenth of an inch.

My own opinion is, after carefully considering the subject, that a 5- or 6-inch iron pipe, with a smooth interior, good joints, and suitable distributing boxes, forms the simplest and cheapest form of conduit possible; that is looking at it as merely a mechanical protection for the cable.

I had hoped that the discussion would have better ventilated the relative advantages of fibrous and homogenous insulation.

Mr. Verity. It would certainly appear, without other proof to the contrary, that, for high tension work at any rate, a good continuous insulated covering, such as vulcanised india-rubber, must possess many advantages over a fibrous material, whatever the insulating oil may be that it is soaked in.

It is very encouraging to hear from Mr. Erskine of the Kensington Court Company's experience, and really it makes one think that much we hear of the frightful difficulties encountered is a bogey, but perhaps the gas and water companies are particularly obliging in this district. My own experience is that even if they are legally obliged to remove the pipes, it is an exceedingly difficult job to get them to do it unless they are quite agreeable and see some advantage to themselves in the arrangement.

Mr. Farquharson has given us a good practical receipt for testing rubber, and, as it has stood the test of so many years' experience with the Admiralty, no doubt we can take it as perfectly reliable. After what has been said about insulation tests, it is obvious that these are totally inadequate and are really no test at all of quality, bad rubber showing quite as high insulation as good when first made.

I have to thank the members of this Society for the courteous way in which they have received my paper, and hope that if, in the future, I may have the opportunity of again bringing the subject before them, it will be when many miles of underground work are in successful operation in this city, and when a wider experience and deeper knowledge will make it one of the first importance to all electrical engineers.

A ballot took place, at which the following were elected:—

*Members:*

| | |
|---|---|
| Frank Erskine Dempster. | Hugh Theodore Pinhey. |

*Associates:*

| | |
|---|---|
| John Melville Coode. | Orlando V. Thomas. |
| Henry Chevly Alexander Goodall. | Augustus Wolfe. |

*Student:*

John Allison Muir.



The meeting then adjourned.

The One Hundred and Ninety-second Ordinary General Meeting of the Institution was held at the Institution of Civil Engineers, 25, Great George Street, Westminster, on Thursday evening, April 25th, 1889—Professor W. CROOKES, F.R.S., Vice-President, in the Chair.

The minutes of the Ordinary General Meeting held on April 11th were read and approved.

The names of new candidates for admission into the Institution were announced and ordered to be suspended.

The following transfers were announced as having been approved by the Council:—

From the class of Associates to that of Members—

Sidney Dobson.

From the class of Students to that of Associates—

| | |
|---|---|
| Peter V. McMahon. | W. H. Wickham. |

The CHAIRMAN: The painful duty now devolves upon me, Gentlemen, to announce the death of an old and very eminent member of this Institution, and a very old personal friend of my own—Mr. Warren De la Rue, who died on Good Friday last. I will ask Mr. Preece to move a resolution, which the Council desire to submit to you, expressive of the deep regret which I am sure all must feel upon the occasion.

Mr. W. H. PREECE, F.R.S.: One of the most depressing duties attached to those who continue to live in this world, is to have to express regret every now and then at the loss of friends. It happens that I became acquainted with Dr. Warren De la Rue in the year 1853, and from that year to 1889—36 years—I have incessantly, in one form or another, received from him attentions and kindnesses which can never be forgotten. Dr. Warren De la Rue had a magnificent laboratory at the back of Portland Place, which was always open to anybody, and especially to young

electricians, who desired to make experiments or to test their ideas; and one had only to express to him a wish to make an experiment in his laboratory, to use his magnificent battery of 15,000 cells, when he would at once cheerfully not only allow the experiment to be made, but would invariably be present, and, if he could, would himself conduct the experiment. I have received during the last 36 years such attentions from Dr. Warren De la Rue that I can never forget, and it was a source of extreme regret to me that owing to ill-health I was unable to pay my last tribute of respect to his remains at Kensal Green last Tuesday. I have therefore with great grief, but with a certain amount of personal satisfaction that such a duty devolves on me rather than on one who knew him less, to propose the resolution which has been already approved by the Council, viz.—"That the Council and "members of the Institution of Electrical Engineers desire to "express their deep regret at the great loss which they, as well "as the scientific world generally, have experienced by the death "of Dr. Warren De la Rue, D.C.L., F.R.S., and they desire to "convey to Mrs. De la Rue the expression of their sincere sym-"pathy with her in her great bereavement."

Professor SILVANUS P. THOMPSON: I ask permission, Sir, to second that resolution. We must all feel deeply grieved that one who was not only honoured by all the learned Societies in this country, but who was also so highly respected by those of other countries, has gone away from our midst; and we cannot but feel that we have sustained, in common with the many other Societies of which Dr. Warren De la Rue was a member, a most severe loss.

The motion was unanimously carried.

The following paper was then read:—

## ON LIGHTNING, LIGHTNING CONDUCTORS, AND LIGHTNING PROTECTORS.

By Dr. OLIVER LODGE, F.R.S., Member.

### INTRODUCTION.

About this time last year I delivered two lectures to the Society of Arts on the subject of "Lightning Conductors," in pur-

suance of a bequest made to the Society in memory of the late Dr. Mann. In those lectures I abstained, as far as possible, from practical recipes and from anything like authoritative advice, contenting myself with calling attention to certain aspects of the subject which had been overlooked. I ventured to imply that none of the older electricians had any notion of the real conditions of the problem; that they all, from Franklin to Faraday and down to the present day, treated it as a much easier matter than, in fact, it is; and that there had been very little real progress in this particular department since the time of Franklin.

Recent advances in electrical theory made it easy for me to see further into the matter than the far greater men of the past had had any chance of doing; and a few very simple and easy experiments soon brought the conditions of the problem clearly before me.

It was these conditions upon which I laid emphasis in my lectures to the Society of Arts. Any practical outcome I left to a later period, and very likely to other hands. The first requisite seemed to be to grasp the conditions of the problem as illuminated by theory; the second, to carry out practical reform in the light of a large experience.

Such few practical recipes as I did surreptitiously introduce I fully expected to hear criticised, nor did I lay any stress on them at the time. (I may say, however, that I have seen no occasion since to importantly modify any of them, and that my position in respect of these practical hints is stronger now than it was then, inasmuch as I find that the large experience of the Belgian electricians, who have given more attention to this subject than perhaps any other school, has led them to recommend in recent years almost precisely the same methods of protection as those I advocated; their views being acquired almost entirely by practical experience and scarcely at all by theory.)

The theoretical and experimental portions of my lectures were, however, matter of serious consideration with me, and nothing was set down under these heads but that of which I was absolutely sure. These portions have by all pure physicists been, so far as I know, universally accepted; as, indeed, they contain nothing

but what any one of them could have easily arrived at had he happened to give sufficient thought and attention to the subject. They have not been attacked, nor do I believe they are at all attackable from the side of theory.

But from the side of experience an attack has come, and, strange to say, it has been directed, not at the few practical suggestions, which might be expected to be vulnerable on that side, but at the theoretical and purely scientific portions.

At the Bath meeting of the British Association, the then President of Section G, speaking in the light of a most gigantic experience—a personal acquaintance with over half a million conductors—led a most good-humoured attack against the doctrine of my lectures, and, exercising his well-known rhetorical and oratorical powers, succeeded probably in impressing the general bulk of the audience with the belief that he had, on the whole, the best of the battle; though, at the same time, I expect they were in a state of utter bewilderment as to what the conflict had been all about, and where the difference "'twixt tweedledum and tweedledee" precisely lay. The battle was, however, admittedly a drawn one, and at its close Mr. Preece magnanimously offered me another field, with an audience of an altogether different calibre to the general public of Bath, by getting your Secretary to invite me to read a paper before this Institution.

Thus much by way of historical note and introduction. Now for the scientific portion of the paper.

I begin by running over some of the conclusions at which I arrived, and proceed to re-establish and demonstrate their correctness either by theory or by fresh experiment, whichever may seem the most simple and satisfactory under the special circumstances.

### Two Main Cases of Lightning Flash.

1. All discharge is virtually that of a Leyden jar. There are always two conductors separated by dielectric, and the discharge is a breaking down of the dielectric at its thinnest or weakest place.

In a thunderstorm the charged conductors are obvious, being

either two clouds, or else a cloud and earth, and the dielectric is the air between.

2. It must sometimes happen, when one cloud discharges into another, that the potential of this other is suddenly raised high enough to cause it to discharge into the earth, even though no strain previously existed in the air between it and earth. The same thing may happen in various other ways when two clouds spark into each other, as indicated by the diagrams (Fig. 1).

3. There are, therefore, two main cases—(*a*) When the strain in the dielectric near the earth has been of gradual growth, in which case the path of discharge will be prepared inductively beforehand; (*b*) when the strain arises so suddenly that there is no time for any pre-arranged path. The first I call "steady "strain;" the second, "impulsive rush." It is most important to recognise these two cases, and to understand the extremely different conditions attending the two. The first case only was ever contemplated by the older electricians; in fact, so far as I know, it was my experiments last year which first called attention to the other case.

### Conditions of Protection and of being Struck, under the Circumstances of each Case.

4. I will now illustrate experimentally* the conditions under which discharge occurs in each of these two main cases; and I will take first the case of steady strain, or case *a*. This insulated sheet of tin plate is supported horizontally a foot or two above another plate lying on the table, and it is electrified by a Wimshurst machine. It represents a charged cloud hovering over the land, the lower plate representing the earth. Between the two I can erect buildings, and lightning conductors terminated in various ways. The typical terminals which I will here use to illustrate the conditions are four—viz., a large knob, or, as I shall call it, a "dome;" a small knob, which I shall call "knob;" a sharp point; and a gas flame, to represent a chimney or other furnace current of rarefied air. Putting the knob and the dome between the plates,

* These experiments were shown with a splendid machine most kindly brought over and erected for the purpose by Mr. Wimshurst himself, whom I hereby cordially thank.

we find the knob struck by preference, even though the dome stands at a much higher elevation. Introducing the point, we find it protects both, by a silent discharge, until it is lowered very considerably; and that then several points may protect when one does not. Replacing the point by the flame, we find that it protects too, but not so efficiently as the point, and that it gets curiously beaten down and darkened in the act of protection. The point is not struck by a noisy flash until it is raised pretty close to the upper plate, when it is struck; but a bunch of points is even then not easily struck, sometimes continuing to discharge with a constant fizz right up almost into the cloud.

5. Try the effect of a bad earth or other very high resistance interposed in the path of the conductor, say a capillary water tube or a bit of wet rag or a wet string.

The violence of the spark is greatly lessened, and the sound is now gentle, but that which was struck before is still struck: resistance, so long as it be something short of infinite, makes no difference to the ease with which a given object is struck under the circumstances of this case 1. Insert the wet rag in path terminated by point, and it still protects, practically as well as before. Insert it in path terminated by knob, and it gets struck at the same elevation as before.

The fact is that the path of the disruptive discharge is all negotiated and pre-arranged in the air above, especially on the surface of any small conductor reared into this space, and the resistance which the flash may ultimately have to meet with in its passage to earth is a thing of subsequent consideration.

6. So much for the conditions attending case *a*, the steady strain. Now attend to case *b*, the impulsive rush. We shall find everything very different.

Alter the connection so that a charged Leyden jar must, when it discharges, discharge direct into the upper insulated tin plate, and thence overflow to the ground if it is able to raise the potential high enough. If the plate is too far above ground for a flash to occur, the Leyden jar does not completely discharge; it only produces a number of fizzes and spits, and the greater part of its charge remains in it. But when the upper plate is within

sparking distance of the ground (and the sparking distance under these circumstances is surprisingly great by reason of the impetus with which the electricity rushes into the top plate), then the jar discharges completely, and we have a violent crack both between the knobs of its discharger and in the air gap between the two plates, which is in the path of the discharge; the arrangement being really two condensers in series, but only one charged (Fig. 2).

The only object of the Leyden jar in case *a* is to give more body to the flash. In case *b* the Leyden jar, or its equivalent capacity, is essential.

7. Putting the dome and the knob between the plates arranged as in case *b*, we find that one gets struck as easily as the other; the knob has now no advantage: whichever is the higher, that gets struck, without reference to other considerations. Introducing the point also, we find precisely the same is true for it; its protective virtue, so much insisted on by the older electricians (among whom I suppose I am right in reckoning my chief antagonist), is entirely non-existent. It gets struck no more easily, and no less easily, than the dome, and it gets struck by a flash of precisely the same noisy character as the others get struck with. A bunch of points acts in exactly the same way. A comb of 24 needle points protects nothing, and gets struck just the same as anything else.

8. Now introduce the flame, and one notices a marked difference. In case *a* it protected less well than the point, but it was not struck noisily any more than the point was. In the present case it gets struck with violence, and it gets struck much more easily than anything else. Adjust dome and knob and point at about the same level, and they get struck, one or other, at random. Adjust the flame a great deal lower, and it protects them all, not by silent discharge, but by getting struck itself instead.

"Protection," however, is in this case not the word to use. The flame better represents a chimney requiring protection, while the point corresponds to the pointed terminal of a lightning conductor raised a good deal higher with the intention of protect-

ing it. Protect it, it does not, however; the flash strikes down the column of hot air and through the flame, while it avoids the more lofty pointed terminal altogether.

Bring a point or a knob, or anything, *into* the column of hot air above the flame: *then* it gets struck easily enough, and protects the flame, but not if it is on one side of the hot-air column. The experiment has an obvious moral in relation to the protection of chimneys: it suggests that the Continental plan of a bar or arch across the mouth of the chimney may after all be justified.

9. Try the effect of resistance in the path of the discharger *now*, and we find it is altogether different to what it was in case *a*.

In case *b* things get struck according to their height, independent of the shape of their terminals, but not independent of their resistances.

Interpose a wet rag in the path of any one, and that one fails to be struck; it is not struck, and it fails to protect the others from being struck, even though it be reared up till it touches the top plate.

The top plate need not therefore be insulated at all carefully for this case *b* experiment.

### Imitation of Lightning.

10. Now let us modify case *b* by making the top plate a sieve full of water, so as to get the flashes in a shower of rain. One cannot well try case 1 in a rain shower: the plate would be discharged too rapidly and continuously by the water-drops for its potential to fully rise. But in case 2 the top plate is not necessarily charged at all until the rush comes, and so the rain shower does no harm.

Flashes in the rain can be got of surprising length and shape, for they make use of the water-drops as stepping-stones. By adding salt to the water they become longer still, but there is no need thus to improve its conductivity for what I want to show.

Notice how the flash contorts itself, taking sometimes extraordinary paths as it jumps from drop to drop, but yet exhibiting its instantaneous character by showing the drops as stationary in its illumination.

11. Remove the things standing on earth-plate just beyond fair striking distance, and what do we see? Appearances precisely like those which are observed in many lightning photographs.

A crowd of violet discharges fill the rainy air—forks, and branch and multiple flashes, not very bright or very noisy, but extraordinarily numerous, and striking on innumerable places at once. A rot is set up in this air at every attempt of the jar to discharge, exactly as happens in one of the most striking of the photographs belonging to the Royal Meteorological Society.

Masts and spars and deck and ocean may be simultaneously struck by these interesting flashes; and, though they do not here appear very violent, yet I expect that on the larger scale of Nature they are not very safe, and may easily have a heating effect sufficient to ignite bodies. These experimental ones are able to ignite gas in the midst of the rain.

12. While we have this arrangement at work we may as well try an interesting little experiment on what happens when lightning reaches water. The rain water has here been collected in a zinc tray some three inches deep, and by bringing a knob from the top plate, some six inches or so above the water, a flash strikes it. It prefers to strike anything metallic if it can, but if there is nothing else within reach, it will strike the water. On reaching the water the flash forks out and ramifies in all directions in a crow-foot pattern, giving the same sort of appearance, only coarser, as that obtained by Mr. J. Brown by taking sparks on to a photographic dry plate and then developing.

Bring the knob nearer and nearer, the same thing happens, until the water is touched by the knob, and even after it is submerged. But as soon as the metallic conductor is submerged the ramifications get less and less vigorous, and when a sufficient surface is immersed they cease. I have never seen these ramifications spread of themselves *below* the surface of the water. They appear to me to keep entirely to the surface. But I have not yet finished investigating these appearances.

13. Immersing a half-full beaker in the water, sparks can be got to the water inside it, though they prefer to curl round and go

outside. The noise the sparks make when they go inside is a curious one, and sounds as if the glass cracked each time, but it does not. If the glass is moist, a brush cascade round its edge can be seen in the dark. If it be dry, the water inside gets charged, and fizzes audibly back to the knob for a second or so after the spark has ceased, a dimple being visible in the water below the knob.

14. Live things in the struck water—worms, flies, fish, &c.,—will most certainly get struck; but so they do under far less violent disturbances than what they would here be subject to. There is nothing surprising in the fish of a pond or lake being killed by a flash of lightning; and it has often happened.

When H.M.S. "Conway" was struck many years ago, and protected by its lightning conductors, it is related that the sea-water was seen to be luminous on all sides of the ship. This is exactly the effect I now imitate.

15. There is one more experiment on discharge in water which I have just tried, and which it is interesting to show.* I take a pointed rod, and, protecting it by a glass tube, immerse its point under water in a beaker containing a plate connected to the other coat of the jar, and pass a spark. With the point negative, there is a bright glow region round it every time, but the discharge is quiet. With the point positive, the flash is of a dazzling white, and is accompanied by a great deal of noise and violence, threatening to smash the Leyden jar, and throwing down the copper plate towards the bottom of the beaker with fury.

### Oscillatory Character of Lightning.

16. Before leaving the outdoor department of our subject I must say a few words on one branch of it concerning which there is evidently considerable uncertainty and haziness abroad—I mean the oscillatory character of a lightning flash.

That a Leyden jar discharge is usually oscillatory must now be regarded as so extravagantly proved that any doubts that may have existed on the subject must surely by this time be cleared

* This experiment was not shown at the meeting, because in trying it over in the afternoon the protecting glass tube was electrically broken.

away, at least for the case where the discharge has to utilise a wire circuit. But perhaps it is still doubted for the case when a jar overflows its edge, or, still more, when it merely sparks through its own dielectric, straight between the coatings.

Now, as I have insisted all along, a lightning flash is a spark through the dielectric of a jar whose two coatings are either two clouds, or else cloud and earth. Hence, if any importance is attached to the fact (as I believe it) that lightning flashes are oscillatory, it is necessary to prove it for a Leyden jar sparking direct between its coatings, especially when the coatings are not very close together.

17. The reason I do attach importance to the oscillatory character of a discharge is because I have worked out the quantitative behaviour of conductors on that aspect of the matter; and though, as Professor Fitzgerald said at Bath, everything would hold just as well for a single oscillation—viz., one violent rise and decay of current (which without any doubt *must* accompany a lightning stroke or any other quick discharge whatever)—if rapid enough, yet the rapidity of such a charge as this does not seem to me probably at all sufficient to account for some of the effects. The rapidity of variation of current in that case would be directly connected with the total duration of the flash; and though we have evidence that it is very momentary, yet we have no evidence that it is so instantaneous (say a millionth of a second) as the semi-period of one of the oscillations may be, a dozen or more of which may accompany an entire flash. However, I admit, of course, that all I want is a tremendously rapid variation of current; and if I can be given this by one oscillation, the rest are unnecessary, and may be dispensed with.

18. When I speak of the oscillatory character of a flash, let it be understood once for all that I do not mean in the least such a thing as can be analysed by waggling the head. Flashes analysable by waggling the head must be multiple ones, and the interval of time between their constituents (which may be, say, the fiftieth of a second or thereabouts) is a long period compared with that of an oscillation such as I mean, bearing the same sort of ratio to it as a quarter of a century bears to an hour.

19. A direct experimental proof that lightning is oscillatory will be obtained when photographs of it are taken on a sensitive plate revolving 1,000 times a second. Something short of that speed would cause the image of the flash to blur, but that speed might be sufficient to analyse out the oscillations, when examined carefully with a magnifier, the focussing being good.

Till then the easiest proof that it is oscillatory is a theoretical one, and it can be put in a few words.

20. Consider an air condenser with two coatings, each of area A, separated by the distance $h$, and let it burst its dielectric. It is well known that the discharge is oscillatory when the whole resistance met with by the discharge is anything less than a critical value.

$$R_0 = 2\sqrt{\left(\frac{L}{S}\right)}.$$

Now, attending only to the straight part of the discharge, and ignoring the current rushing up in the plates to the spark path, the self-induction of a straight conductor of length $h$ and sectional radius $a$ is very approximately

$$L = 2\,\mu\,h\,\log\frac{4\,h}{a}.$$

The capacity of the discharged condenser is

$$S = \frac{K\,A}{4\,\pi\,h}.$$

Hence the critical resistance which must not be exceeded is given by

$$R_0^2 = \frac{32\,\pi\,\mu\,h^2}{K\,A}\log\frac{4\,h}{a};$$

or

$$R_0 = 10\,\mu\,h\,\text{"}v\text{"}\sqrt{\left(\frac{1}{A}\log\frac{4\,h}{a}\right)}$$

$$= 300\text{ ohms} \times \sqrt{\left(\frac{1}{A}\log\frac{4\,h}{a}\right)} \times h.$$

The important thing to notice in this value of $\sqrt{\frac{L}{S}}$ is that it is approximately proportional to the first power of $h$, the distance between the plates of the condenser.

21. Next consider the resistance of the discharge path. It

too will be proportional to the length $h$, and it may be written

$$R_1 h,$$

where $R_1$ is the resistance per unit length of the discharge path.

The condition for oscillation, then, is that this $R_1 h$ shall be less than $R_0$; or *the resistance of unit length of the spark must be less than*

$$300 \text{ ohms} \times \sqrt{\left(\frac{1}{A} \log \frac{4h}{a}\right)}.$$

22. The term under the square root will take different values according to circumstances, and it may be greater or less than 1 per metre. Ordinarily, however, it will be greater than 1 per metre, unless the area of charged surface is considerable. The important thing is the way in which $h$, the distance between the plates, enters into the expression. It does not come in very prominently at all, but so far as it does influence the result it permits the discharge to be osciliatory more easily when the plates are a good distance apart than when they are close together.

23. Take a couple of typical examples.

First, a Leyden jar bursting its glass. A fine needle may be just put through the hole usually made in these cases, so we can take the sectional radius $a$ as something like a tenth of a millimetre. The thickness of the glass may be 2 miliimetres, hence $4^h/_a = 80$ or thereabouts; and the natural logarithm of this is about 4. Suppose the area of coated surface is half a metre square, then the critical resistance which a metre of the spark must not exceed is 1,200 ohms, and so the 2 millimetres of it must not exceed 2·4 ohms.

It is difficult to say whether this is or is not a large resistance for such a short spark, and hence it is difficult to be sure that such a spark is anything more than a mere one-directional discharge. To go into it more fully the currents in the metal coatings would have to be considered.

Next take as example a cloud area at an elevation of one kilometre; and because a lightning discharge usually

makes perforations of fair diameter, we may suppose $a$ to be about a millimetre. (A widely erroneous estimate in this quantity makes but little difference in the result.) In such a case $4^h/_a = 4 \times 10^6$, and the logarithm of it is between 15 and 16. Hence the charged area may be 15 or 16 square metres without bringing down the square root term below 1 per metre; so the resistance of the whole flash may in that case be anything below 300,000 ohms without checking the oscillations. The discharged area may indeed be as much as 1,500 square metres without bringing the critical resistance which the flash must not exceed below 30,000 ohms, or 30 ohms per metre. (Understand that "resistance" here does not mean impedance. It means true dissipation of energy-resistance (sec. 25), and the current squared is so enormous (sec. 29) that the resistance-coefficient may be quite small).

In another place (*Phil. Mag.*, August, 1888) I have shown reason for believing that the area of cloud discharged at any one flash is usually very moderate; and hence on the whole I consider it proved, so far as elementary theory can do it, that the lightning flash usually takes place under conditions favourable to oscillation.

24. And these oscillations are extremely rapid. The rapidity depends on the inverse geometric mean of L and S, and this is practically almost independent of $h$. Referring back to their values we see that

$$\mathrm{L\,S} = \frac{\mu \mathrm{K A}}{2\pi} \log \frac{4h}{a};$$

and so the number of complete alternations per second is

$$\frac{\text{“}v\text{”}}{\sqrt{\left(2\pi \mathrm{A} \log \frac{4h}{a}\right)}},$$

which, in the second example of section 23, with A as 100 square metres, becomes

3 million per second.

If the discharged area were as great as 10,000 square metres, the rate of alternation would still be a third of a million per second.

### Material of the Lightning Conductor.

25. A few words may suffice to explain the nature of the impedance which alternating currents meet with in passing through a conductor, and of the reason why iron is as good as, or even better than, copper for the purpose of conveying currents alternating with extreme rapidity.

A rising current has to magnetise the space all around it, and the production of this magnetisation delays and impedes the rise of the current to its maximum value. A falling current permits the magnetisation of the space all round it to decay, and the dying out of this magnetisation delays and impedes the fall of the current to its minimum. The more rapidly the current changes, the more powerfully felt is the influence of the accompanying magnetisations and demagnetisations. Now the total magnetisation produced by a current—its total number of lines of force, or its total "magnetic *induction*," as it is often called—is proportional to the current strength: equal to it multiplied by some constant, which we may call L, and write

$$I = L\,C,$$

where I is the total induction produced by the current C. L is a coefficient characteristic of the circuit, its value being defined by this equation, and is called the coefficient of induction excited by the current's own self, or the coefficient of self-induction.

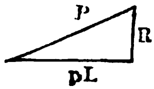
If the current goes through $p/2\pi$ complete alternations in a second, it can be shown that the impedance it meets with, due to the reversals and re-reversals of its own magnetic field, is $p\,L$.

This is not the whole obstruction it meets with, but it is the only part which does not dissipate energy and cause its vibrations to decay. It may be called the *inertia* part of the impedance. The remaining part of the total impedance is resistance, R, the dissipation of energy coefficient, defined by

$$\text{heat per second} = R\,C^2;$$

and the total is the resultant of these two as if they were

at right angles to each other. So that, calling P the total impedance,




$$P^2 = (p\,L)^2 + R^2.$$

Now in respect of the R term, iron is much worse than copper, not only 7 times worse, hundreds of times worse; but then for very rapid alternations the R term is altogether insignificant compared with the $p$ L term.

26. In respect of the $p$ L term does the material of the conductor matter?

Well, in so far as the magnetisation spoken of is that of the space surrounding the conductor, of course the substance of the conductor itself matters nothing. But in so far as the conductor itself gets magnetised, the material of which it is made does matter. Now a linear current magnetises at right angles to itself everything surrounding it—most intensely the things close to it. A hollow cylindrical current magnetises everything outside itself, but nothing inside. If the current were to distribute itself uniformly through the section of the wire, the outside of the wire would get magnetised in concentric cylinders; but if the current were to confine itself to the outer surface, and flow as a hollow cylinder, it would escape the necessity of magnetising the wire at all. In cases where the $p$ L term is much more important than the R term this is precisely what it does therefore. It always flows so as to meet with the least possible total obstruction; and it finds less total obstruction by cramping itself into the periphery of the wire than it would find if it utilised the whole section. The cramping into the periphery increases R, but it decreases $p$ L; and on the whole with rapidly alternating currents this is an advantage, and gives a smaller value to P.

Slowly changing currents think most about R, and use the biggest cross-section they can find. Rapidly changing currents think most about $p$ L, and avoid having to magnetise more than they need. Especially must they avoid having to magnetise the conductor if it consists of iron: hence in that case they cramp themselves tremendously into its outer skin, and thereby avoid

having to overcome much more total impedance than they meet with in the case of copper; but though they thus keep down impedance, they increase their dissipation of energy term, and get their oscillations damped out far more quickly than when they only have to pass through copper. The violence of the flash therefore subsides more quickly in an iron, than it does in a copper, conductor; and so I find it experimentally.

Nothing here said is in the least hypothetical. It is all absolutely clear and certain, and has been abundantly verified. I will not go into the history of the subject. It is well known.

27. Let it be clearly understood once for all, that in comparing copper and iron conductors I never mean comparing them as of unequal thickness, *i.e.*, of equal conductivity. Such a comparison is ridiculous in the present state of knowledge. They are to be compared when of the *same* thickness; and under those circumstances I assert iron to be a trifle better, certainly not a whit worse, than copper; irrespective of all its other manifest advantages, cheapness, fusing-point, etc., etc. Want of flexibility is sometimes urged against iron, but stout telegraph wire is flexible enough; and, until experience decides to the contrary, I feel sure it is thick enough for lightning conductors.

The one and only thing on which anything can be said against iron is on the subject of its durability; and this being a chemical question, I offer no positive opinion; at the same time I am absolutely certain that any slight disadvantage in that respect is a hundredfold compensated in most localities by its other superlative advantages.

## Current and Potentials during Discharge.

28. In cases where a conductor is pretty thick, say anything like a quarter inch diameter, or even a tenth, and of any moderate length, such as 100 yards or less (not many miles), the two terms of impedance are so unequal that it is for many purposes needless to think about R at all, the impedance is practically $p$ L simply; and this, as I have shown in *Phil. Mag.*, August, 1887, is under any given circumstances half the critical *resistance* which determines whether the discharge shall be oscillatory or not under the

same circumstances. The total impedance is commonly to be reckoned in hundreds or even thousands of ohms.

29. The strength of current passing in the alternations can be estimated by considering that the whole quantity stored up in the discharged body has to be transmitted in a quarter of one oscillation period—say, for instance, in the millionth of a second. If the quantity discharged were one coulomb this would mean a current of a million amperes. In any case the current must be hundreds or thousands of amperes.

30. The difference of potential needed to drive so strong a current through so great an obstruction is enormous, being equal to the product P C, and may be reckoned in millions or hundreds of millions of volts; hence it is that lightning conductors afford no easy path for lightning, but that it tends to spit off in all directions, even to what would seem, and indeed are, very inferior conductors. It will spit off from a well-earthed stout copper rod to bits of wood and to perfectly insulated bodies.

## Effects in the Neighbourhood of a Discharge.

31. Putting together the enormous electrostatic potentials existing at the different points of a conductor conveying a discharge, and the violent oscillation to which so strong a current is subject, we perceive how great must be both the electrostatic and the electro-magnetic induction in all space anywhere near it.

These disturbances, thus rendered certain and accounted for, can easily be experienced experimentally. I proceed to relate a few experiments out of a multitude which I have made.

32. I took a considerable length of highest conductivity No. 0 electrolytic copper wire, kindly lent me by Messrs. Thos. Bolton & Sons, and arranging one end so as to be accessible to Leyden jars, etc., carried the wire up to a high gallery and then down to earth. Earth was made in several ways on different occasions: water pipes, gas pipes, hot-water pipes which ramified the whole building, outside gas mains, bars buried in ground; but the most effective and indeed perfect return circuit could be had when wanted by connecting the end metallically with the outside coat of the jar, without any earth contact at all. When this direct

contact was not made, the outer coat of jar had of course to be connected to earth also, in order to complete the circuit. Very often they were both connected to each other and to earth as well. It makes no essential difference; whatever may be considered the most satisfactory method, that may be adopted, and the phenomena will go on just as well. They may be briefly summarised as follows:—

(1.) If the conductor pass within an inch or two of any uninsulated piece of metal, it gives off a violent side flash to it.*

If the far end of the conductor is neither earthed nor connected to jar, but is left insulated in air, side-flashing from it occurs a trifle more easily, but not very markedly so.

(2.) If the conductor pass within, say, half an inch of an insulated conductor, it gives off a side flash to it; the strength of the flash depends on the capacity of the insulated conductor, being considerable if it be large; but some side spark occurs to an absurdly small body perfectly insulated, *e.g.*, such a thing as a coin on a stick of sealing wax; and this when the conductor is absolutely well earthed at its far end.

(3.) Sparks can be obtained from everything, even quite uninsulated things, connected to the conductor: for instance, if it be connected to the gas pipes, small sparks will fly to the finger or to an insulated body from all the gas brackets about, and these sparks are sufficient to ignite gas.

(4.) Sparks can be obtained between the ends of any long curved conductor, be it insulated or uninsulated, if they are brought close enough together to form a nearly closed circuit.

(5.) Sparks can be obtained from or between insulated bodies in the neighbourhood of the conductor, and not connected to it at all, every time a flash occurs.

* This easy and striking experiment I stupidly omitted to show at the meeting.

For instance, a large piece of wire gauze connected to nothing gave off sparks to a gas bracket and ignited the gas. Moreover, one piece of wire gauze sparked small sparks into another, neither connected with anything. [*In illustration of this, some gilt key-pattern high up on the wall of the hall, 25, Great George Street, was seen to be sparking while Leyden-jar discharges were going on through a wire lying on the floor. They were not so bright as the sparkings in the Royal Institution wall paper, but the gilding was further from my wire, and I believe is quite far from any wire. As illustrating the electric currents produced in conductors during the act of reflecting electro-magnetic waves, they were therefore still more satisfactory.*]

(6.) Sparks can be obtained from quite uninsulated bodies, even when not connected with the conductor: *e.g.*, from hot-water pipes, from gas pipes, from water pipes, from strips of brass let into the table, from gas brackets in other rooms, from a wire lying on the floor of a distant corridor; and, in fact, all over the building, with few exceptions. The sparks can be taken by a penknife held in the hand; sometimes they can be taken to the knuckle or finger-tip even when pressed against them.

(7.) Sparks can be got to pass between two totally uninsulated things, neither of which have any connection with the lightning conductor. For instance, let the conductor be thoroughly earthed in some outdoor and distant manner, and under favourable circumstances a bright short spark can be seen passing at every discharge between a gas tap and a water tap of my lecture table which happen to approach each other closely. Let the gas escape near these sparks and it ignites.

(8.) Sparks can be got between two thinly insulated electric light wires if they lie close enough together, and if a storage battery be connected to them an arc will be started, destroying the insulation and burning the wires.

(9.) If at any distant place, or out of doors in daytime, the sparks are too feeble to be seen, disturbances can still be easily detected by means of a telephone; connecting one terminal to the thing—say, the roof of a shed, or a wire fence—and holding the other end in one hand. Or, of course, by connecting the two terminals to two different things, or to different parts of one thing.

(10.) Arrange Abel's fuses between gas and water pipes, between pieces of wire gauze and gas pipe, between hot-water pipe and a bit of sheet metal, between the lightning conductor and a 6-inch metal sphere on long glass stem, between two large insulated bodies, between the gas bracket of another room and an empty Leyden jar; in short, in almost any place, likely or unlikely. Then take a few strong discharges through the thick copper rod with end well earthed, and the fuses will pop off, some at one discharge, some at another. Very visible sparks can sometimes be seen passing into the fuses, if they be not closely connected, without exploding them. A certain energy of spark is necessary to ignite the composition: much more than is sufficient to excite the retina.

## Lightning Protectors.

33. Now let me say a word about the possibility of protecting telegraph and other instruments from damage by lightning currents which may have entered the aerial lines.

There is indeed no guarantee that burying wires beneath a pavement, or even beneath water, will effectually secure them from lightning disturbance, as I have now fully illustrated, but certainly overhead wires are more exposed.

The ordinary and well-known form of lightning protecting arrangement is to attach a pair of plates, or a double set of points, or a pair of points in a vacuum, or some other small air space, as a shunt to the instrument or coil of wire to be protected. Here are arrangements of the kind. Now it is perfectly easy to

see that the protection such things afford is of the most utterly imperfect kind.

Take a coil of tangled silk-covered wire, or any other coil that you don't mind damaging, and attach it as a shunt to one of these protectors. On discharging a jar through it, the insulation of the coil is pierced in heaps of places. Or one may use a short fine wire, and see it deflagrated by the branch discharge.

34. It may be said that my air space is too wide. Very well, then, abolish it altogether. Bring the plates of your protector into direct metallic contact. By so doing, the coil is indeed shunted out of the circuit, and if it were a telegraph instrument no signal could be given, for no appreciable fraction of the current takes that route; but with a Leyden-jar flash it is otherwise. Although the plates of the protector are in contact, soldered together if one pleases, and led up to by stout wire or rod, a branch flash still breaks through the insulation of our wire tangle, or deflagrates our little bit of thin wire.

35. It will be said the joints are bad. Well, then, do without joints; take a stout rod of electrolytic copper bent in an arc, say, 2 feet long, and, bridging it across with the wire tangle, discharge a jar round it. Still a portion takes the thin wire, even though it offer a path yards in length.

Take a straight bar of copper an inch thick, and arrange an invisibly fine Wollaston wire of greater length as a tapping circuit. Some of the discharge shall leave the bar, and spark across a minute air gap at each end, in order to make use of the hair-like platinum wire.

Go further still than this. Connect a coil of thin-covered wire *by one end only* to a wire conveying a discharge, standing the reel upon a block of paraffin or other good insulator, and connect the other end to any little thing of any capacity at all—say, a bullet lying on the block of paraffin: you can see sparkings through the insulation of the wire on the reel at every discharge.

These experiments render manifest the hopelessness of any simple shunt arrangement as a lightning protector.

36. The easiest mode of exhibiting the essentials of these experiments—one that can be tried by any one possessing a

Leyden jar, a pair of discharging tongs, and a yard or two of fine silk-covered wire—is to hang a tangle of the wire loosely on to the tongs, not necessarily making any sort of good contact, and then use them to discharge a jar in the very ordinary way. Some of the flash will take the thin wire, and will spark through the insulation at a number of points (Fig. 3).

37. It may be very well objected to me that it is pretty useless if I only point out the imperfection of present methods, and offer no suggestion as to a proper lightning protector. Well, this struck me too, and the result is that I have devised and made what I think I may call an absolutely perfect protector—one into which great flashes may be sent, and yet the galvanometer or instrument intended to be protected shall not wink, nor shall the slightest palpable or visible disturbance be discernible, notwithstanding that complete metallic contact is maintained all the time, and not a trace of the signalling or useful current wasted.

The thing seems so good that I am making it the subject of a patent, and you will therefore pardon me for saying no more about it just yet.

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38. The next part of this paper is largely controversial. I do not take much pleasure in this portion, and wish it were unnecessary. But it is a purely impersonal controversy; and so long as the old-fashioned views are in existence, one way of arriving at the truth is to try and thrash them out of existence. If there is any real vitality in them, the attack will fail. It will be well believed that I have no feeling of hostility to the Lightning-rod Conference, when my best scientific friend, Professor Carey Foster, was one of its members. Had I been one myself, I no doubt should at that time have signed the very documents which now, in a few places, I criticise. Had I indeed so signed, I would abuse what I now see to be its erroneous portions with still more vigour than I now permit myself to employ.

### Summary of Points of Difference and Controversy.

39. It may be convenient here to summarise a few of the points wherein the doctrines which I advocate differ from the views

held by the older electricians. And the summary will give me an opportunity of emphasising the incorrectness of the older views in many instances.

I quote a few statements prefixed by capital letters from an abstract I made for the *Electrician* after the Bath meeting of the British Association. (See *Electrician*, Sept. 21 and 28, 1888.)

*A*. Rods as at present constructed, though frequently successful, may and do sometimes fail, even though their earth is thoroughly good; the reason being that they offer to a flash a much greater obstruction—a much worse path—than is usually supposed: an obstruction to be reckoned in hundreds or thousands of ohms, even for a very thick copper rod. N.B.—This is not resistance proper, but impedance.

I may be permitted here to repudiate the doctrine which has several times been attributed to me since the Bath meeting, that it is safest to be without lightning conductors altogether. The long experience of persons learned in this art is by no means to be despised, and until an agreement as to improvements has been arrived at, the safest plan for ordinary persons is to adhere to existing practice. Nevertheless, that conductors sometimes fail, is as certain as that they often succeed. My statement is that customary arrangements are not perfect, and are susceptible of improvement. The statement of the Lightning-rod Conference is that "there is no authentic case on record where a properly con-"structed conductor failed to do its duty." Mr. Preece calls the statement "most decisive." It is certainly *decided*, and in the light of other matter contained in the same red covers I assert it to be in its natural and intended signification decidedly false. The only signification which makes it true makes it also senseless; as if one should record the statement that white things are white. In my Society of Arts lectures, I said nothing against the report of the Lightning-rod Conference, because the work done by that body, in collecting information, abstracting papers, and recording instances, was obviously very valuable, and much of the report itself is correct; while as for the occasional rash statements in that document I imagined the signatories would wish them to sink into oblivion in silence. But Mr. Preece has revivified them,

and conspicuously made himself afresh responsible for them;* accordingly I now extract one or two more sufficiently dogmatic and unfortunate statements from the same source.

"A man may with perfect impunity clasp a copper rod an "inch in diameter, the bottom of which is well connected with "moist earth, while the top of it receives a violent flash of "lightning."

"If all these conditions be fulfilled; if the point be high "enough to be the most salient feature of the building, "no matter from what direction the storm may come, be of "ample dimensions, and in thoroughly perfect electrical con-"nection with the earth, the edifice with all it contains will "be safe, and the conductor might even be surrounded by "gunpowder in the heaviest storm without risk or danger." To adhere to such views as these now, with tenacity sufficient to cause them to be promulgated as authoritative scientific statements, would be, in my opinion, little less than criminal.

"All accidents may be said to be due to a neglect of these "simple elementary principles." Certainly this "*may be said*," because it already has been said over and over again; but it cannot be said with truth.

Whenever an accident happens, a believer in the modern exponents of the Lightning-rod Conference who could not point out a flaw, or a bad joint, or a bad earth; or a possible flaw, or a possible bad joint, or a possible bad earth; would feel himself disgraced as a practical man. An instance occurred in quite a recent number of *Nature*. The writer of "Electrical Notes" records that a number of fish had been killed in a pond into which the earth end of a conductor had been led, and concludes with the ejaculation, "When will people learn to make proper "earth connections?" I select this instance as typical of the extraordinarily contradictory advice often bestowed on that long-suffering body the British Public. Before an accident, the pond

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* Saying at Bath, as a prelude to a quotation from the Report, "The "Report itself is most decisive and most important; there is a true ring about "it—the ring of men who knew what they had done, and what they were "writing about."

would be pointed out as an excellent earth, as indeed it most likely was. After the slaughter of the fish, the erector of conductor is impersonally ridiculed for having utilised it. So with any struck building. After an accident defects *must* be forthcoming, because else there would be "an authentic case on "record where a properly constructed conductor failed to do its "duty," which *ex hypothesi* is absurd and impossible; therefore it was not constructed in accordance with the directions of the Lightning-rod Conference, therefore it was defective. Q.E.D.

*B.* When a Leyden jar is charged it corresponds to a bent spring, and its discharge corresponds to the release of the spring. Its discharge current alternates, therefore, in the same way and for much the same reason as a twitched reed or tuning-fork vibrates. The vibrations decay in either case because of frictional heat production, and because of the emission of waves into the surrounding medium. A single spark of a Leyden jar, examined in an exceedingly fast revolving mirror, is visibly drawn out into a close succession of oppositely-directed discharges, although its whole duration is so excessively minute.

It is very likely that this statement will now no longer be denied. So I pass to the next.

*C.* A lightning flash is a spark between cloud and earth, which are two oppositely electrified flat surfaces, and the flash corresponds therefore to the internal sparking between the two plates of a great air condenser. All the conditions which apply to a Leyden jar under these circumstances are liable to be true for lightning. Sometimes the resistance met with, either in the cloud itself or in the discharger, may be so great that the spark ceases to be oscillatory, and degenerates into a fizz or rapid leak; but there can be no guarantee that it shall always take this easily manageable form; and it is necessary in erecting protectors to be prepared for the worst and most dangerous form of sudden discharge. The apparent duration of a lightning flash is due to its frequently multiple character, and indicates successive discharges, not one long-drawn-out one. Nothing that lightning has been found to do disproves its oscillatory character; because Leyden jar discharges, which are certainly oscillatory, can do precisely the same.

This was in answer to Mr. Preece's contention that lightning could not be oscillatory, because it magnetised steel bars and deflected ships' compasses. The next is an antidote to the continually made statement that the one thing needful for an efficient lightning conductor is *conductivity*.

*D*. Although some conductivity is necessary for a lightning conductor, its amount is of far less consequence than might be expected. The obstruction met with by an alternating or rapidly varying discharge depends much more on electro-magnetic inertia or self-induction than upon common resistance. So much obstruction is due to this inertia, that a trifle more or less of frictional resistance, in addition, matters practically not at all. It is very desirable to have a good and deep earth in order to protect foundations and gas and water mains from damage, and in order to keep total impedance as low as possible.

I find it sometimes thought that I have argued against the need of a good earth. This is not so. I have argued against the exclusive and exaggerated attention that has been paid to this need.

In opposition to the following statements, *e*, *f*, *g*, *h*, *i*, the substance of which may be considered as hitherto orthodox, I make the statements subsequent, labelled *E*, *F*, *G*, *H*, *I* :—

*e*. No danger is to be feared from a lightning conductor if only it be well earthed and be sufficiently massive not to be melted by a discharge. All masses of metal should be connected to it, that they may be electrically drained to earth.

*f*. The shape of the sectional area of a conductor is quite immaterial; its carrying power has nothing to do with extent of surface; nothing matters in the rod itself but sectional area or weight per foot run, and conductivity.

*g*. Points, if sharp, should constitute so great a protection that violent flashes to them ought never to occur.

*h*. Lightning conductors, if frequently tested for continuity and low resistance by ordinary galvanic currents, are bound to carry off any charge likely to strike them, and are absolutely to be depended upon. The *easiest* path protects all other possible paths.

*i*. A certain space contiguous to a lightning rod is completely protected by it, so that if the rod be raised high enough a building in this protected region is perfectly safe.

These statements I say are erroneous. The following I believe to be correct :—

*E.* The obstruction offered by a lightning rod to a discharge being so great, and the current passing through it at the instant of a flash being enormous, a very high difference of potential exists between every point of the conductor and the earth, however well the two are connected; hence the neighbourhood of a lightning conductor is always dangerous during a storm, and great circumspection must be exercised as to what metallic conductors are wittingly or unwittingly brought near or into contact with it. When a building is struck, the oscillations and surgings all through its neighbourhood are so violent that every piece of metal is liable to give off sparks, and gas may be lighted even in neighbouring houses. If one end of a rain-water gutter is attached to a struck lightning conductor, the other end is almost certain to spit off a long spark, unless it also is metallically connected. Electric charges splash about in a struck mass of metal, as does the sea during an earthquake or when a mountain top drops into it.* Even a small spark near combustible substances is to be dreaded.

*F.* The electrical disturbance is conveyed to a conductor through the ether or space surrounding it; expressed more simply, lightning currents make use of the periphery of a conductor only, and so the more surface it exposes the better. Better than a single rod or tape is a number of separate lengths of wire, each thick enough not to be easily melted, and well separated so as not to interfere with each other by mutual induction.

The liability of rods to be melted by a flash can be easily over-estimated. A rod usually fails by reason of its inertia-like obstruction, and consequent inability to carry off the charge without spittings and side flashes; it very seldom fails by reason of being melted. In cases where a thin wire has got melted, the energy has been largely dissipated in the effort, and it has acted as an efficient protector; though, of course, for that time only.

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* *See* the Report of the Krakatoa Committee.

Large sectional area offers very little advantage over moderately small sectional area, such as No. 5 B.W.G.

*G.* Points, if numerous enough, serve a very useful purpose in neutralising the charge of a thunder-cloud hovering over them, and thus often prevent a flash; but there are occasions, easily imitated in the laboratory, when they are of no avail: for instance, when one upper cloud sparks into a lower one, which then suddenly overflows to earth. In the case of these sudden rushes, there is no time for a path to be prepared by induction, no time for points to exert any protective influence, and points then get struck by a violent flash just as if they were knobs. Discharges of this kind are the only ones likely to occur during a violent shower, because all leisurely effects would be neutralised by the rain-drops better than by an infinitude of points.

*H.* The path chosen by a galvanic current is no secure indication of the course which will be taken by a lightning flash. The course of a trickle down a hill-side does not determine the path of an avalanche. Lightning will not select the easiest path alone; it can distribute itself among any number of possible paths, and can make paths for itself. Ordinary testing of conductors is therefore no guarantee of safety, and may be misleading. At the same time it is quite right to have some system of testing and of inspection, else rust and building alterations may render any protector useless.

*I.* There is no space near a rod which can be definitely styled an area of protection, for it is possible to receive violent sparks or shocks from the conductor itself; not to speak of the innumerable secondary discharges which, by reason of electro-kinetic momentum and induction, by reason of electro-magnetic waves, and of the curious recently discovered effect of the ultra-violet light of a spark, are liable to occur as secondary effects in the wake of the main flash.

### Instructive Extracts from Reports of Damage by Lightning.

40. Reading between the lines of existing reports on damage done, one can frequently find evidence of many of the phenomena to which I have now called attention. Of course they are not

recorded in any prominent manner, because they are to the observers ill-understood and puzzling facts: it is very difficult to note what has exactly happened in any given case unless some clue or expectation has been formed beforehand. The bad conductivity clue, which was the only one prominently available to the skilled recorders in the following cases, is a very partial one, and in many cases is quite insufficient to account for the facts without undue pressure being put upon it.

Under such circumstances the record of the facts is of course far more valuable than the comments made upon that record; and in the following extracts the theoretical remarks should be eliminated or slurred over.

One minor imperfection, common to many accounts, is that they are not sufficiently alive to the possibility of all manner of branching discharges: so that the discharge is said to leave a conductor and go to something else, when the truer statement would be that some *portion* of it branched off at such and such a point.

In the following quotations the references are usually to the pages of the volume of the Lightning-rod Conference, published by Spon in 1882:—

41. Illustrating Side Flashes and Surging Circuits.

"*L.R.C.*, p. 39.—*J. Murgatroyd. St. Mary's, Crumpsall, near Manchester.*—A lightning conductor from spire touched the eaves gutter, and a gas pipe touched the end of this gutter. The lightning passed from the conductor along the gutter to the gas pipe, melted it, and set the church on fire by igniting the gas."

"*L.R.C.*, p. 39.—*Wyatt Papworth.*—Tall spire struck. The church stands in an open position with no large trees near. It was provided with an iron lightning conductor $\frac{3}{4}$ in. diam., fixed with iron holdfasts, and carried down inside the spire and tower into ground; the top of it was said to be attached to a bold copper finial on the spire about 150 feet from the ground, and 50 feet above ridge of roof; the lightning is supposed to have first struck the finial, it slightly deranged some beds of masonry in upper part of spire, then descended by iron rod to belfry, melted a gas tube in the floor, and set fire to the belfry by igniting the gas."

*From Abstract of Report on the Destruction by Lightning of a Gunpowder Store at Bruntcliffe, Yorkshire. By Major V. D. Majendie, R.A.* (*L.R.C.*, p. 77.)

"The gunpowder exploded at 4.30 p.m. on August 6, 1878, during the greatest intensity of a violent thunderstorm. The building was brick, with brick arched roof, length 9 feet, width 5 feet, height 6 feet (internal dimensions). The store had a uniform thickness of three bricks, and was furnished at one end with an iron door, at the other end with a lightning conductor. The conductor consisted of a copper wire rope, 10 gauge copper wire, the rope being $\frac{7}{16}$ inch thick, having four points at the top (one large one in the centre, and three smaller ones round it); it extended to about 13 feet above the top of the building, and about the same length was carried into the ground and terminated in a drain. The conductor had been erected in 1876 by Mr. John Bisby, of Leeds, and was fixed to a pole distant about 2 inches from the end of the building opposite to that in which the iron door was fixed (it was not connected with the iron door in any way). No one was near the store when the powder exploded, and it seems probable that [the earth connection of the conductor was bad, that] the mass of iron in the door offered at least an equally good path—and that the gunpowder was ignited by a flash passing between the two imperfect conductors."

To make this report more completely scientific, it would be well to omit the words I have put in square brackets.

*Extract from a reply of Mr. Baldwin Latham, C.E.*:—

"It is no uncommon thing for buildings provided with what are called lightning conductors to be damaged by lightning, and the cause is due to the inadequacy of the conductor to carry the electric fluid, which will leave the conductor for a better or a larger conductor." (See also sec. 43.)

42. As illustrating that a good conductor affords no absolute security, the following document is worthy of reproduction in full, as given in *L.R.C.*, p. 115:—

"*Ueber Blitzableiter und Blitzschläge in Gebäude welche mit Blitzableitern versehen waren. Von G. Karsten. Keil.* **8vo.** 1887. (*Abstracted by R. Van der Broek.*)

"In this pamphlet Dr. Karsten gives an account of two cases in which buildings that were provided with lightning conductors were damaged by lightning. The author states that the statistics for the year 1873 show that in Schleswig-Holstein twenty-six per cent. of all the cases of fire were caused by lightning; $\frac{1}{130}$ part of these cases occurred in the towns and the remainder in the country.

"Do lightning conductors guarantee absolute protection? The author answers this question as follows:—There is no absolute certainty in empirical matters; each new case may direct our attention to circumstances that had been overlooked. If lightning conductors cannot be said to ensure perfect safety, they certainly afford a very high degree of protection.

"The flash of lightning which struck the church at Garding, on the 18th of May, 1877, fractured the conductor in fifteen places, and pierced the wall of the steeple in two places. The inefficiency of the conductor resulted from the carelessness with which it was fixed; the line was laid down the north side of the steeple and fastened with twenty-five wall eyes; these wall eyes were hammered too deep into the wall, thus damaging the line and forming a short and sharp bend in each case, besides also unduly straining the wire. The damage to the steeple was the consequence of a neglected secondary circuit. There are an excessively large number of tie-rods in the steeple; the heads of these rods are not connected together, neither are they, except in one case, in close proximity to any of the larger masses of metal that are about the building. The conductor passed close to one of those heads; the south side of the steeple, where the opposite head is, becoming wet through the rain, a secondary circuit was formed, and a return shock followed; the damage to the steeple was trifling.

"The rod was provided with a conical point, rather blunt, but surmounted by a short platinum point. The copper line-wire

was of good material—not of a uniform thickness, but at the weakest places not weighing less than 240 grammes per lineal metre (8 oz. per yard, or rather less than ¼ inch diameter if solid). The earth-plate was sunk into a well 10 metres deep, and tested faultless after the discharge."

43. The following series of notes are better not separated, though they bear upon different points:—

*L.R.C.*, p. 93 *et seq.—From Abstract of Statistics of Buildings and Ships struck by Lightning. By F. Duprez, Member of the Academy.*

*Illustrating occult dangers from secondary disturbances in the neighbourhood of conductors.*

"The author cites three cases of buildings set on fire though protected by lightning rods. But the precise cause of the fire was not ascertained."

*Illustrating multiple flashes.*

"In each of two cases the lightning struck at once the three rods fixed to a building."

*Illustrating that a poor earth is not necessarily fatal to the efficiency of the conductor, except as to the protection of the soil itself and of things buried in it.*

"Out of fifteen cases of lightning rods struck, in which the conductors were simply buried more or less in the soil, they carried off the strokes in eleven without the buildings being injured or any trace being left of it, except that the ground was upheaved where the latter was too dry."

*Illustrating a matter important, if true: suggested by Professor Fitzgerald theoretically at Bath: not yet verified by me.*

"In two cases the stroke broke the conductor at points where its direction was abruptly changed."

*Illustrating side flash.*

"In two other cases the lightning left the conductors struck, and fell upon buildings near, without causing damage to those on which the rods were fixed."

*Illustrating brush discharge from conductor, and surging circuits its neighbourhood.*

"Two electrical phenomena are to be noted as sometimes occurring when a lightning rod is struck. First, when a conductor is formed of metallic plates a peculiar noise is heard like water pouring on a fire. Second (independently of the form of the conductor), electric sparks are emitted from bodies near. The author cites examples at Berne, 1815."

44. The following illustrate the carrying power of fairly thin wire, and the fact that thin wires may protect although themselves deflagrated. *From Mr. Preece's paper on "Lightning" to Society of Telegraph Engineers, November*, 1872 (*L.R.C.*, p. 101):—"There were only two cases in the past season where line wires (No. 8 iron, 0·17 in. diam.) were absolutely fused."

*From letter by Admiral Sullivan* (*L.R.C.*, p. 195):—"You will like to know a case in which a copper wire acted as a perfect conductor, *though fused throughout its length.* It was at Monte Video, in the house of the English Consul; a flag-staff was struck and conducted the lightning through a flat roof near the bell wire of a suite of rooms (the wire ran in sight near the cornice) through a hole in each dividing wall, and then down to the bell in the basement: the wire was melted into drops like shot, which burnt a row of small holes in the carpet of each room. A dark mark on the cornice above showed where the wire had been. At the bell there was a slight explosion and some little damage, but I do not recollect whether anything acted partially as a conductor from that point and so carried off that part of the charge.

"This, I think, shows that even an ordinary bell wire will act as a conductor for a rather strong stroke of lightning, as the large flag-staff was shattered."

45. As illustrating that experience has led to the perception of the value of large surface to a rod, I may quote the standard American work, *Spang's "Practical Treatise on Lightning Conductors." Philadelphia*, 1887 (*L.R.C.*, p. 113):—

"A conductor of large surface exercises a much greater pro-

tective action than the same quality of metal in the form of a wire or solid rod.

"Not because electricity in motion resides on the surface, but that the expansive action of a discharge may have a wider scope through the metal."

[The incoherence of the reason does not destroy the correctness of the stated fact.]

Messrs. D. Munson & Co., of Indianapolis, Indiana, have sent me specimens of their rods, which have flanges and sharp edges, and in various ways aim at large surface. Their rods are composed of copper and iron mixed, which is curious, and their mode of attainment of large surface is needlessly complex.

The following illustrates the contradictory views about manner of conduction by lightning rods. *Letter from Admiral Sullivan* (*L.R.C.*, p. 199):—

"I firmly believe in the surface theory of Harris. I had been with him often when he made experiments nearly fifty years since, and witnessed a strip of tinfoil of the thinnest kind, and about ¼ inch wide, protect a model mast of about six inches in diameter from electric shock, that without it split the mast to pieces, aided by a small hole through its centre filled with gunpowder. And I always thought that the surface-conducting theory of Harris was indisputable. But about 20 years since, having to approve a proposal of the Trinity House for a new conductor of a lighthouse, which, like previous ones, was an inch in diameter copper rod called 'Faraday's Plan,' I thought I would go up to the Royal Institution and ask him why he did not use a copper tube instead, giving much greater conducting power with less copper. I did so, and he asserted positively that the conducting power depended entirely on the volume of copper in the section of the conductor, no matter whether it was in a bolt, plates, or tube; and that if Harris said differently, 'he knows nothing whatever about it;' of course I approved the rod-conductor. But singularly enough, though I had not seen Harris for years, he came to town a few days after, and came to the Board of Trade to see me, and bring me a piece of his large tube conductor, with a connection that he was fitting to the Houses

of Parliament. When I told him what Faraday's opinion was, he answered, 'Then he knows nothing about it.'"

46. I may call attention to some apparently sound doctrines in a paper by Capt. Bucknill, R.E., abstracted in *L.R.C.*, p. 242.

That lightning can penetrate into collieries is proved by an account of a meeting of Mining Engineers (*L.R.C.*, p. 237).

The same sort of thing is illustrated by the accident at Bootham Bar, York (*L.R.C.*, p. 219), when the lightning struck down into a cavity surrounded by high buildings with lead roofs, iron rain-water pipe, and an iron portcullis, to get at a street bracket 11 ft. 6 in. above the pavement, melting its pipe, and setting a house on fire by the large flame produced (*L.R.C.*, p. 219).

47. That the existence of gas pipes make houses more difficult to protect, and in fact causes dwelling-houses to require almost as much attention as powder magazines, can be illustrated by a good number of extracts. The following may serve (*L.R.C.*, p. 239):—

"On July 13, 1880, during a thunderstorm, the large 400-light gas-meter of this mill, though locked up in a cellar, and with no light near it, exploded, and the gas, which is supplied through a 4-inch main, was ignited. This was repaired, but on July 5, 1881, during another thunderstorm, precisely the same accident occurred."

From an interesting report, signed J. Gavey, on damage to a church at Cardiff (*L.R.C.*, p. 218), I make the following extract:—

"On examining more closely the surroundings of the lightning conductor, I observed that the church gas-pipe, an iron one, about 1¼ inches in diameter, passed through the wall of the building about 6 feet from the conductor, and was carried in a direction corresponding with the hole caused by the explosion (see plan). I immediately concluded that this explosion was due to the current breaking across from the conductor to the gas-pipe, and on opening up the hole I found this to be the fact. The conductor crossed the gas-pipe at nearly a right angle, being about a foot above it. The under portion of the conductor bore evident marks of fusion, and, more interesting still, the gas-pipe was slightly coated with a very thin deposit of copper, so thin that it perished

in my attempt to remove it; but still there was an undoubted coating at one spot."

Continuing the account of this kind of damage, the following interesting remarks by Professor Kirchhoff have a bearing on the important practical question whether gas-pipes should or should not be utilised for earth; but their main utility lies in the proof afforded of the unique kind of danger which gas-pipes introduce:—

*Injury to Gas and Water Pipes by Lightning.**

The city gas company of Berlin, having expressed the fear that gas-pipes may be injured by lightning passing down a rod that is connected with the pipes, Professor Kirchhoff has published the following reply:—

"As the erection of lightning-rods is older than the system of gas and water pipes as they now exist in nearly all large cities, we find scarcely anything in early literature in regard to connecting the earth end of lightning-rods with these metallic pipes, and in modern times most manufacturers of lightning-rods, when putting them up, pay no attention to pipes in or near the building that is to be protected." Kirchhoff is of the opinion, supported by the views of a series of professional authorities, that the frequent recent cases of injury from lightning to buildings that had been protected for years by their rods, are due to a neglect of these large masses of metal. The Nicolai Church, in Griefswald, has been frequently struck by lightning, but was protected from injury by its rods. In 1876, however, lightning struck the tower and set it on fire. A few weeks before, the church had had gas-pipes put in it. No one seems to have thought that the new masses of metal which had been brought into the church could have any effect on the course of the lightning, otherwise the lightning-rods would have been connected with the gas-pipes, or the earth connection been prolonged to proximity with the pipe. A similar circumstance occurred in the Nicolai Church in Stralsund. The lightning destroyed the rod in

* See also on this subject two Abstracts in *Journal of Institution of Electrical Engineers*, No. 77, vol. xviii., 1889, of papers by Prof. W. Kohlrausch and A. Voller respectively.

many places, although it received several strokes in 1856, and conducted them safely to the earth. Here, too, the cause of injury was in the neglect of the gas-pipes, which were first laid in the neighbourhood of the church in 1856, shortly before the lightning struck it. The injury done to the school-house in Elmshorn, in 1876, and to the St. Lawrence' Church, at Itzehoe, in 1877, both buildings being provided with rods, could have been avoided if the rods had been connected with the adjacent gas-pipes.

"If it were possible," says Kirchhoff, "to make the earth connection so large that the resistance which the electric current meets with when it leaves the metallic conducting surface of the rod to enter the moist earth, or earth water, would be zero, then it would be unnecessary to connect the rods with the gas and water pipes. We are not able, even at immense expense, to make the earth connections so large as to compete with the conducting power of metallic gas and water pipes, the total length of which is frequently many miles, and the surface in contact with the moist earth is thousands of square miles. Hence the electric current prefers for its discharge the extensive net of the system of pipes to that of the earth connection of the rods, and this alone is the cause of the lightning leaving its own conductor."

[This, like many another theoretical remark, is not to be supposed true.—O. L.]

Regarding the fear that gas and water pipes could be injured, he further says: "I know of no case where lightning has destroyed a gas or water pipe which was connected with the lightning-rod, but I do know cases already in which the pipes were destroyed by lightning because they were not connected with it. In May, 1809, lightning struck the rod on Count von Seefeld's castle, and sprang from it to a small water-pipe, which was about 80 metres from the end of the rod, and burst it. Another case happened in Basel, July 9, 1849. In a violent shower one stroke of lightning followed the rod on a house down into the earth, then jumped from it to a city water-pipe, a metre distant, made of cast iron. It destroyed several lengths of pipe, which were packed at the joints with pitch and hemp. A third case, which was related to

me by Professor Helmholtz, occurred last year in Gratz. Then, too, the lightning left the rod and sprang over to the city gas-pipes; even a gas explosion is said to have resulted. In all three cases the rods were not connected with the pipes. If they had been connected the mechanical effect of lightning on the metallic pipes would have been null in the first and third cases, and in the second the damage would have been slight. If the water-pipes in Basel had been joined with lead instead of pitch, no mechanical effect could have been produced. The mechanical effect of an electrical discharge is greatest where the electric fluid springs from one body to another. The wider this jump the more powerful is the mechanical effect. The electrical discharge of a thunder-cloud upon the point of a lightning-rod may melt or bend it, while the rod itself remains uninjured. If the conductor, however, is insufficient to receive and carry off the charge of electricity, it will leap from the conductor to another body. Where the lightning leaves the conductor, its mechanical effect is again exerted, so that the rod is torn, melted, or bent. So, too, is that spot of the body on which it leaps. In the examples above given it was a lead pipe in the first place, a gas-pipe in the last place, to which the lightning leaped when it left the rod, and which were destroyed. Such injuries to water and gas pipes near lightning-rods must certainly be quite frequent. It would be desirable to bring them to light, so as to obtain proof that it is more advantageous, both for the rods and the building which it protects, as well as for the gas and water pipes, to have both intimately connected. Finally, I would mention two cases of lightning striking rods closely united with the gas and water pipes. The first happened in Düsseldorf, July 23, 1878, on the new Art Academy; the other August 19, last year, at Steglitz. In both cases the lightning-rod, the buildings, and the pipes were uninjured."—*Deutschen Bauzeitung.* (Quoted in *The Building News*, September 10, 1880.)

48. That lightning sometimes does things which is on the hitherto available views inexplicable, is evidenced by the following record by one who, of all others, made lightning conductors his hobby, who acquired an immense amount of experience concern-

ing them, and to whom the carrying out of probably the most perfect system of protection in actual use is largely due—I mean the late M. Melsens:—

*Note on the Lightning Flash at Antwerp Railway Station, July* 10*th*, 1865. *By M. Melsens.**

"I have described this lightning stroke, which was very harmless, since all the damage was confined to the breaking of a square of glass in the roof of the covered station of the railway at Antwerp; but in connection with it I found myself confronted by so extraordinary a phenomenon that I had to search through all the descriptions of lightning strokes which might offer a certain analogy to this with which I have to do. It was only on looking through my notes again, and returning several times to the place, that I have ventured to describe this very extraordinary stroke, and I have only published it after a long time, when it seemed to me that I could establish it upon careful observations and experiments which seemed of a nature to corroborate my conclusions. [Mention made of 19 lightning flashes, which present some analogy to that which struck Antwerp Station.]

"A few words are sufficient to show the peculiarity of the phenomenon. The lightning crossed a square of glass and produced in it a hole similar to that which would be produced by a projectile thrown upwards from below at a slow rate, say from 30 to 50 metres per second, and travelling from earth towards the sky; the edges of the hole were melted. I have given a description of the experiments I made, together with Ruhmkorff, to prove that in reality lightning does travel from the earth towards the sky; the proof which I give of it seems to me decisive.

"The extraordinary fact, as I have remarked, is to see the lightning pass, by means of a very bad conductor, through a square of glass 4 millimetres in thickness, forming a parallelogram of $0^{m.}\ 35 \times 0^{m.}\ 38$, having angles of 83° and 97°.

* "Paratonnerres: Notes et Commentaires," par M. Melsens. Brussels, 1882. Extracted from Reports of Belgian delegates to the Paris Exhibition, 1881.

"The opening produced was at a distance of some centimetres from iron and lead conductors, which were in perfect metallic communication with all the iron of the station. The weight of this latter exceeds 120 tons. But the anomaly does not stop there: to the right and left of the glass roof in which the broken pane was, the covered platform of the station has a roof of zinc No. 13, presenting to the lightning a surface of 3,000 square yards; the weight of this zinc is not less than 15 tons; the three tall rods of the lightning conductor are in immediate metallic contact with this zinc, and with the conductor, and the whole is connected to 28 hollow columns, serving to carry off rain-water. The collection of these metals would allow one to suppose that they were well adapted to carry off lightning or any such disturbance easily; but in this instance it despised them, and chose a path entirely unexpected. Let me add that there was a shed some 62 metres from the station built on hollow columns and iron framework, and the greater part of it roofed with zinc; moreover, about 40 metres on the opposite side were more sheds roofed with zinc; the metallic surface was not less than 2,000 square metres. Moreover, in my investigation, I might mention plenty of other metals in communication more or less perfect with the earth—grids, sconces, gas-pipes, telegraph wires, rails, etc. Besides, the whole of the platform may be considered as one large lightning conductor in perfect communication with a very damp earth, and offering hardly any resistance to the passage of the current from the building."

49. Of all the buildings in the world not wholly made of metal, the Hôtel de Ville at Brussels was and is the most perfectly and elaborately protected. No electrician exists but would a year ago have asserted, had he gone over it, that it was absurdly and exaggeratedly safe from damage by lightning. Last July it was struck and set on fire.

The case has been investigated and published in the *Bulletin de la Société Belge d'Électriciens* for September and October, 1888.

It seems to have been owing to secondary or induced electric surgings in a horizontal bar of metal totally disconnected from anything; not pretending to lead toward earth, and, being some

little distance below a stretch of lightning conductor, not offering itself as an object to be struck. On all the old views it was utterly insignificant. However, as a matter of fact, although not struck, although it did not (probably) even receive a side flash, yet the induced surgings set up in it, induced by Maxwell and Heaviside's electro-magnetic waves, were so violent as to ignite some gas and cause a small fire. Had it been connected to the conductor, the sparking from it would probably have been still stronger.

This occurrence, and certain sparkings in the wall paper of the Royal Institution (see *Electrician* or *Nature*, March, 1889),—the sparkings, too, in the gilding of this present hall (sec. 32),—are plain and straightforward intimations that the old views on the subject of electric conduction are hopelessly, and absurdly, and dangerously inadequate. It is time that the prophets of that old superstition were slaughtered by the brook Kishon.

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## Practical Questions.

50. There remains to consider what is to be the practical outcome of all this. What improvements in the erection and testing of lightning conductors are possible. This is a matter well worthy of discussion, and eminently suited to it. It is a matter on which I have not the slightest wish to be dogmatic; and if I make a few apparently definite assertions, it is only by way of expressing such judgment as I have been able to form at present, and they are to be taken as intended more by way of suggestion and question than anything else.

With this proviso, I should be disposed to make some such statements as the following:—

51. All parts of a lightning conductor, from points to roots, should be of one and the same metal, to avoid voltaic action.

52. Joints should be avoided when possible, and should be made substantially when necessary. Allowance for expansion and contraction must not be forgotten.

53. Sharp bends, and corners, and curves, and roundabout paths to earth should be avoided as far as possible.

54. The use of copper for lightning conductors is a needless extravagance.

55. Iron has advantages over every other metal.

56. The shape of cross-section is but little matter. Flat ribbon has a slight advantage over round rod, but not enough to override questions of convenience.

57. Liability to be deflagrated by a powerful flash determines the minimum allowance for size of cross-section. No consideration of conductivity and greater ease of path has the least weight in this connection.

58. It is hopeless to pretend to be able to make the lightning conductor so much the easiest path that all others are protected. All possible paths will share the discharge between them, and a lot of apparently impossible ones.

59. A good and deep earth should in general be provided, independent of water and gas mains.

60. If the conductor at any part of its course goes near water or gas mains, it is best to connect it to them.

61. If the place to be protected has water or gas pipes inside it, the conductor should be connected to their mains underground.

62. At all places where water and gas pipes come near each other, and, in general, wherever one metal ramification approaches another, it is best to connect them metallically.

63. The neighbourhood of small-bore fusible gas-pipes, and indoor gas-pipes in general, should be avoided in erecting a lightning conductor.

64. Into powder magazines and such like places no gas or water pipes should be permitted to enter, unless the whole building is made of metal, and they are elaborately connected to it at the point where they enter.

65. It is not wise to erect very tall pointed rods above the roof of a building.

66. A number of points all along the ridge of a roof is better than only a few.

67. Any part of a building is liable to be struck, and, to make quite secure, every prominent part of the outside should have a rod running along it.

68. Earth-connected as well as insulated bodies are liable to spit off sparks.

69. No complete security can be attained unless the whole building be metal-lined, floor and all.

70. In ordinary houses it may be well to try and insulate the lightning conductor from the walls so as to lessen the chance of side flash to metal stoves and things inside.

71. In chimneys it may be well to use insulators to protect the bricks from concussion.

72. The cheapest way of protecting an ordinary house is to run common galvanised iron telegraph wire up all the corners, along all the ridges and eaves, and over all the chimneys; taking them down to the earth in several places, and at each place burying a load of coke. Rain-water spouts and other outside metal, if all well connected together, may likewise be utilised.

73. Connecting a lead roof or other such expanse with a lightning conductor is not an unmixed good, for it virtually increases the dangerous proximity of the lightning conductor, and may inadvertently bring it near to many objects which else might have escaped.

74. One of the most difficult things is to know what to connect and what to avoid.

75. The orthodox rule, "Connect all pieces of metal to the "lightning conductor," requires modification thus:—Connect all pieces of metal to each other and to the earth, but not to the lightning conductor.

76. It may be always reckoned safe to earth things independently. It is often not safe to connect them to the lightning conductor: *e.g.*, an inside lining of a chimney should be well earthed, but should not be used as lightning conductor nor connected with it. The same with rain-water pipes and gutters. The same also, probably, with lead roofs.

77. In connecting pieces of metal to each other, if they happen to form a nearly closed circuit, the circuit should be metallically completed.

78. Over the top of tall chimneys it is well to take a loop or arch of the lightning conductor, made of any stout and durable metal.

79. Lightning conductors should be always outside and easily visible.

80. A conductor detached from the building to be protected is safer than one in close contact with it.

81. For powder-magazines and such like, an outer cage surrounding the building, with sky points and earth roots, and an inner cage on the building, with independent earth terminals only, is the safest plan.

82. If under these circumstances there be no gas-pipes nor much ramifying metal work inside the building, and no metal at all going near either cage, the interior may be considered perfectly safe.

83. The inner cage may often be conveniently made of continuous sheet iron. The outer cage need not then be at all small-meshed; in fact, it need be little more than a dozen vertical conductors.

84. The resistance of an earth may be tested in the customary way to guard against actual breaches of contact by rust or workmen; but no overweening confidence must be felt, even though the resistance turn out the thousandth of an ohm or less.

85. A Wimshurst machine and couple of Leyden jars afford a convenient mode of testing a conductor for flagrant defects. The testing should be done in the dusk or in moonlight, so that there may be light enough to work by and yet sparks be visible.

86. Telephones are the handiest things to detect electric surgings in conductors inside the house while discharges are being made to the conductor. But vacuum tubes, gas leaks, Abel's fuses, etc., etc., can also be employed.

87. Another mode of testing can be carried out on an insulated rod, with an induction coil and spark gap after the manner of a great Hertz oscillator. This is probably the most searching plan. Sparks will then be probably obtained from all the gas brackets, water taps, and picture rails in the house.

88. Telegraph stations, and houses supplied with electricity from overhead wires, should have an efficient lightning protector at the place where the wires enter the house.

89. When a number of houses are wired up together for lighting, even by underground wires, it may be well to disconnect them by means of intervening lightning protectors on the principle of fire-proof doors.

90. A central lighting station, having a tall chimney connected to its boilers and dynamos, should be, by a lightning protector, disconnected from the leads which carry the current from it; because a small fraction of a stroke getting into the leads might destroy a number of lamps, especially if they were already working at something like their full power.

91. Telephone arrangements, and any long length of close-packed insulated wires (even underground), should be protected by lightning protectors, else the insulation is apt to be sparked through and spoiled.

The CHAIRMAN: We have had a most interesting and instructive discourse, and under ordinary circumstances, if time allowed, it would be at once followed by a discussion, which would also doubtless prove very interesting; but our permitted period this evening has already elapsed, and therefore the discussion will be adjourned until our next meeting, which will take place on Thursday, May 9th.

I have to announce that the Council, in compliance with a memorial, have extended the period during which Associates, transferred from the class of Students, may attend the Students' meetings, from *one* year to *two* years after the date of such transfer.

A ballot took place, at which the following candidates were elected:—

*Foreign Members:*

Camilio A. Carrizosa.
Theodore Guilleaume.

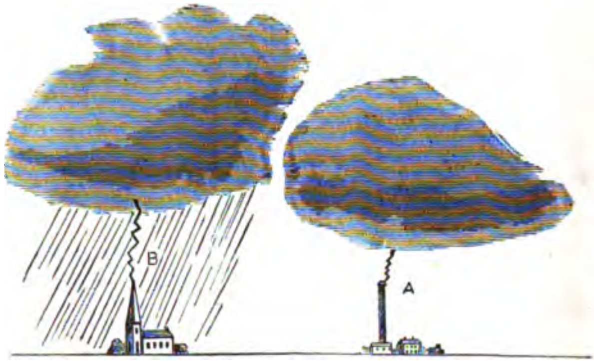
*Associates:*

James Gemmil.
James Walter Grimshaw.
Ernest de Mèrindol Malan.
Harry Neville Moody.
Frederick A. Nixon.
Edward William Snook.

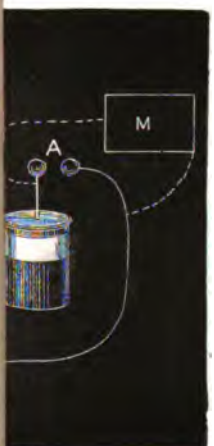
*Student:*

Donald Barton.

The meeting then adjourned.




CASE *b*2.



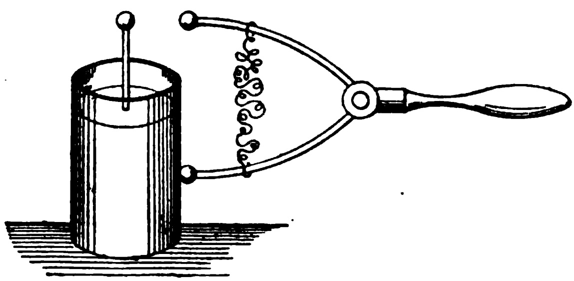



FIGURE 3.

Case *a* in each figure is the steady-strain case. The others are preliminary strain. Clouds correspond to coatings of jars; spaces
enience. The leak, or imperfect conductor, in the second case, *b*2
shower. In case *b*, the rain-shower, or leak, is permissible, but

# ABSTRACTS.

## E. F. HERROUN—DIVERGENCE OF ELECTRO-MOTIVE FORCES FROM THERMO-CHEMICAL DATA.

(*Philosophical Magazine, Vol.* 27, *p.* 209, 1889.)

It has already been remarked by many observers, that the actual measured E.M.F. of several voltaic elements is not the same as the value calculated from the known reactions of the constituents of the element. Such deviations might be explained by the non-occurrence in the cell of the exact reactions anticipated, but some different reaction may occur which evolves heat to an extent sufficient to account for the observed E.M.F. Such action might be due to the coating of the metals with films of salts or gases. The chief interest of the question, however, lies in the probability of the evolution or absorption of sensible heat in the cell, and the loss or gain in electric energy.

The author has investigated the behaviour of several metals, including mercury, silver, lead, and tin, and has arrived at the following general conclusions.

The primary factor in determining the E.M.F. of a voltaic cell is the relative heat of formation of the anhydrous salts of the two metals employed.

The E.M.F. may set up chemical charges of a different direction and character from those to be expected from the heat of formation of the dissolved salts.

The E.M.F. may be supplemented by a portion at least of the energy due to the solution of the solid salts.

Where there is little or no attraction between the water and the salt, the negative heat of solution is derived from sensible heat, and is not supplied by the free energy of the chemical change.

When metals whose salts have purely negative heats of solution are opposed to metals whose salts they can replace, the E.M.F. set up is in excess of the total thermal change.

Hence no cell exists which can furnish an E.M.F. in excess of the free energy of the chemical change.

Some metals have a tendency to form films of sub-salts on their surfaces, giving rise to an unexpected thermo-chemical reaction, and therefore to an E.M.F. differing from the calculated value.

Finally, the author concludes that the E.M.F. of a voltaic cell furnishes a more accurate measurement of the "free energy," and therefore of true chemical affinity, than data derived from calorimetric observations.

## A. POTIER—MEASUREMENT OF CURRENTS BY MEANS OF ELECTROLYSIS.

(*Comptes Rendus, Vol.* 108, *p.* 396, 1889.)

The exactitude of the measurement of a current by this means depends on the electro-chemical equivalent of the deposited metal and on the exactness of the chemical reaction. A comparison of a voltameter containing a solution of mercurous nitrate with one containing silver nitrate, both being placed in the same circuit, showed that the former always lead to somewhat lower values of the current than the latter. The cathode used was of platinum; and it appears that the difference of values noted is due to the liberation of hydrogen gas in the first instance. After the gas has ceased to be evolved, the platinum becomes amalgamated. Similar results, but more persistent, were noticed if the platinum were replaced by silver or copper. The cathode is therefore not entirely passive in the electrolysis, but the condition of its surface has an influence on the phenomenon. The case is analogous to what is observed in ebullition.

The electrolytical measurement of a current can therefore be considered rigorously exact only on the condition that the electrodes show no trace of polarisation. It is generally admitted that this is attained when the electrode consists of the metal the salt of which forms the electrolyte; but the experiments show that this is not always the case.

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## J. MOOSER—EXPERIMENTAL RESEARCHES ON MICROPHONIC CONTACTS.

(*La Lumière Electrique, Vol.* 31, *p.* 451, 1889.)

The experiments were carried out with a Blake microphone, in which contact is made between a semi-spherical platinum point and a flat carbon disc. The first cause of the variation of the contact resistance is undoubtedly pressure. An apparatus was arranged in the form of a balance, the platinum point above the carbon disc being counterpoised by a vessel into which a very fine stream of water was allowed to flow. A current from a Daniell cell was sent through a resistance box and a galvanometer, and the curve plotted from the values of the deflections and of the unplugged resistance. The resistance box was then replaced by the microphonic contact, and, using the same current, a second curve was plotted for decreasing pressure of the platinum point. A comparison of the two curves gave the microphonic resistance. It appears at once that the resistance varies inversely as the pressure.

The resistance must also depend on the extent of the surfaces in contact, and is inversely proportional to that area. Herz has shown that if $d$ is the diameter of the circle of contact of a sphere of diameter D, with a plane surface, if $p$ is the pressure of the sphere, and K a constant depending on elasticity, etc., then

$$d = \sqrt[3]{\frac{3\,p\,K\,D}{16}}.$$

In the present case, this formula becomes simply

$$d = c \sqrt[3]{p}$$

or, since $R = \frac{c_1}{d^2}$, therefore $R^3 p^2 =$ constant.

The curve plotted from this formula does not coincide with that derived from experiment, but the latter rises more rapidly, though their general curvature is similar.

It results further from the experiments that the resistance varies inversely with the current. The total resistance may therefore be represented by the equation

$$R = c' \sqrt[3]{\frac{1}{p^2}} + \frac{c''}{i^2}.$$

The former constant ($c'$) depends chiefly on mechanical properties of the contacts, the latter ($c''$) on thermal properties; it is the second term which accounts for the non-coincidence of the observed and calculated curves already mentioned. The increase of temperature with the pressure is much greater for large currents than for small ones. This increase of temperature depends on the pressure and on the extent of the surfaces in contact. If the upper platinum point is delicately suspended, and a strong current used, the contact is broken. This is not so much due to the repulsion of two successive portions of the same current, according to Ampère's law, as to the heating of the point of contact, which causes expansion to occur.

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## W. H. SCHULTZE—THE ELECTROLYTIC BEHAVIOUR OF MICA AT A HIGH TEMPERATURE.

(*Annalen der Physik und Chemie, Vol.* 36, *p.* 655, 1889.)

A plate of glass and a plate of mica were placed alternately between three plates of metal, and the whole put into a hot air oven. The wires from the upper and middle, or from the middle and lower metal plates, could be at will connected to the measuring instruments, so that the insulation of the glass and of the mica could be readily compared while they were both at the same temperature.

It appears that plates of mica cut parallel to the cleavage planes possess, equally with glass, the property of becoming a better and better conductor as the temperature is raised. After reaching a maximum of conductivity, further increases of temperature have a contrary effect, and for very high temperature the conductivity becomes infinitely small. The comparison of the two bodies shows that at all temperatures mica is a better insulator than glass.

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## K. SCHREBER—ELECTRO-MOTIVE FORCE OF THIN FILMS OF HYDRATED SUPEROXIDES.

(*Annalen der Physik und Chemie, Vol.* 36, *p.* 662, 1889.)



The object of the investigation was to ascertain how thin a film of a body would still show the characteristic behaviour of the body. Previous experi-

ments by other physicists have shown that for capillary phenomena the limit is reached at a thickness of about $50 \times 10^{-6}$ mm., for electro-motive phenomena at 1 to $3 \times 10^{-6}$ mm., and for optical phenomena at $4 \times 10^{-6}$ mm.

The experiments were carried out with films of the superoxides of manganese, lead, and bismuth; the method being to expose the thin films on platinum plates to the action of electrolysis when opposed to clean platinum, and to note the alterations in the electro-motive force. The area of the plate being accurately known, and its weight before electrolysis, the loss of weight at once made it possible to calculate the thickness of the remaining film. The limiting value was taken as that at which the last and most considerable fall of E.M.F. was noted; and it was found to be for manganese superoxide $2{\cdot}3 \times 10^{-6}$ mm., and for the lead oxide $4{\cdot}8 \times 10^{-6}$ mm. These values are of the same order as, and approximate to, those found by Oberbeck for zinc, cadmium, and copper; and tend to show that groups of atoms at this point did not materially differ from molecules.

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## H. HERTZ—RAYS OF ELECTRIC FORCE.

*(Annalen der Physik und Chemie, Vol. 36, p. 769, 1889.)*

The author has been able to produce actual rays of electric force, and to carry out with them all the elementary experiments which are commonly performed with rays of light and of radiant heat, *i.e.*, he has been able to show that the rays of electricity proceed in straight lines, that they can be polarised, reflected, and refracted.

The apparatus used consisted of two vertical parabolic mirrors of sheet zinc, secured to wooden stands. In the focus of one mirror were placed vertically, one above the other, the two metallic bodies terminating in knobs, between which sparks could be made to pass by means of a small induction coil. The length of the two conductors within the mirror was approximately equal to half the wave-length which corresponds to oscillations in straight wires. In the focus of the second mirror was placed the secondary circuit, consisting of two wires 50 cm. long, arranged in the same vertical straight line, and with their inner ends 5 cm. apart. These vertical wires were connected by wires passing horizontally through the mirror, with a sparking arrangement capable of adjustment, and placed at the back of the mirror, so that it could be observed from behind. The whole arrangement much resembled two American bottle-jacks with their respective reflectors.

The first experiments were made without the reflectors. On starting the oscillations in the primary circuit, it was possible to observe by means of a movable circular conductor all the phenomena described in previous papers, *i.e.*, up to a distance of 2 metres under the most favourable conditions. The effect was increased by the presence of a conducting wall parallel to the oscillations; and it may be concluded that the wave in the air corresponding to the primary oscillations has a half wave-length of 30 cm.

If now the primary circuit were fixed in its position in the parabolic mirror, and its effect on the movable circular conductor tested, it was found

that the effect was nil behind and at the sides of the mirror, whilst in the focal axis the effect was now apparent at a distance of 5 to 6 metres, which might even be increased to 9 or 10 metres if a plain conducting wall were interposed in the path of the rays. This, however, was only true in close proximity to the wall. At other points the direct and the reflected wave interfered with one another, producing nodes. Four such nodes were found: at the wall, at 33 cm., at 65 cm., and at 98 cm. distance from it. The half wave-length would therefore be 33 cm., and its period of oscillation 1·1 thousand millionth of a second. The half wave-length in wires was 29 cm.

The author next made use of both parabolic mirrors, and found that he could obtain effects up to a distance of 16 metres. This was the limit of space at his disposal; but, as will be seen, the direct distance of the two mirrors might probably have been increased 20 metres. A distance of 6 to 10 metres is, however, the one best adapted to the experiments.

A sheet of zinc interposed between the two mirrors prevents any spark in the secondary circuit. The same effect is produced by a sheet of tinfoil or goldleaf, or by the presence of a human being between the mirrors (*e.g.*, Hertz's assistant). Insulators do not cut off the rays, which can pass through a wooden door; as the author remarks, "It is not without astonishment that one sees "sparks appear inside a closed room." Metal sheets placed on either side of the rays and parallel to them have no effect so long as the distance between them is not less than the aperture of the mirror, viz., 1·2 metres. If they are brought closer than this the secondary sparks decrease, and finally disappear when the sheets are half a metre apart. The sparks also disappear if the focal axis of one mirror be rotated through an angle of 15°.

If the primary mirror and its circuit be kept vertical, whilst the secondary circuit and mirror are turned gradually into a horizontal position, the sparks decrease more and more, and finally disappear when the two mirrors are crossed, even though they be brought close together.

The author constructed an octagonal wooden frame, on which he wound parallel copper wires 3 cm. apart. If the two mirrors were placed with their foci parallel, and the frame so disposed between them that the wires were at right angles to the direction of the foci, it produced scarcely any effect on the secondary sparks. If, however, the screen were placed so that its wires were parallel to the foci, the rays were entirely cut off. The screen acted as a tourmaline would do in the case of a plane polarised ray of light. The mirrors were then crossed, and so long as the wires on the frame were vertical or horizontal, *i.e.*, parallel to the one or the other mirror, no sparks appeared in the secondary circuit; but as soon as the frame was placed so that the wires made an angle of 45° with the horizon, the secondary sparks reappeared. Evidently the screen of wires resolved the oscillations into two components, and only allowed that one to pass which was perpendicular to the wires. The phenomenon is precisely analogous to that produced by a tourmaline plate placed between two crossed Nicols.

With the primary circuit in a vertical position, the oscillations are in a vertical plane, and are entirely wanting in a horizontal plane; from the

observations, however, it undoubtedly results that the electrical oscillations are accompanied by magnetic oscillations, these latter being entirely in a horizontal plane; the polarisation of the rays, therefore, results in a separation of the electrical from the magnetical oscillations.

The two mirrors were now placed alongside each other, with their apertures turned in the same direction, and their focal axes converging at a point about three metres distant. There was no secondary spark. A vertical sheet of zinc was then placed opposite the mirrors, and perpendicular to the line bisecting the angle formed by their axes. Secondary sparks were obtained easily; they disappeared, however, as soon as the zinc plate was turned 15° about its vertical axis. Sparks could still be obtained when the zinc plate was at a distance of 10 metres from the mirrors, *i.e.*, when the ray had to traverse 20 metres of air.

The ray from the primary mirror was directed along a wall; at a door in this wall the zinc plate was fixed vertically at an angle of 45° to the path of the ray, and the secondary mirror was placed inside the inner room with its focal axis at right angles to that of the primary mirror. The ray was reflected from the one room into the other, even when the door was shut. The secondary spark, however, disappeared as soon as the zinc plate, which acted as a plane mirror, was rotated out of its position. Reflection of the ray of electricity therefore takes place along a straight line, and the angle of incidence is equal to the angle of reflection. In this experiment both mirrors were placed vertically, and therefore the plane of oscillation was perpendicular to the plane of incidence. On turning both mirrors into a horizontal plane, the same phenomenon of reflection was observed; but if the mirrors were crossed, no secondary sparks were obtained. The mirrors being again placed side by side, the wire-wound frame was used as a reflecting surface. Reflection only took place when the wires were parallel to the plane of oscillation.

By using a prism of pitch, the ray was readily refracted; the refracted ray being traced for a distance of 5 or 6 metres from the face of the prism. The refractive index was found to be 1·69; that for light is between 1·5 and 1·6; the difference being probably due to impurities in the pitch used.

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## J. BERGMANN—OBSERVATIONS ON THE CHANGE IN THE CONDUCTIVITY OF METALS PRODUCED BY HEATING.

(*Annalen der Physik und Chemie, Vol.* 36, *p.* 783, 1889.)

The metals experimented upon were copper, aluminium, magnesium, zinc, and german silver. The conductivity was compared by means of the induction balance with that of a disc of mercury 70 mm. in diameter and 1 mm. thick, taken as a standard.

In the first instance discs were cut out of sheet metal. The conductivity was first determined for the ordinary temperature, and then after the discs had been heated for an hour in a hot air bath at a temperature of 300° C. The conductivity was higher after heating than before in all cases of simple

metals, but the alloy showed a very slight falling-off. The percentage charge is shown in the following table, where the two columns refer to different pieces of metal:—

| | % | % |
|---|---|---|
| Copper ... ... ... ... | +3·63 ... .. | +2·47 |
| Aluminium ... ... ... | 7·14 ... ... | 3·69 |
| Magnesium ... ... ... | 6·65 ... ... | 6·92 |
| Zinc ... .. ... ... | 2·48 ... ... | 2·43 |
| German silver ... ... | −0·18 ... ... | −0·13 |

In order to make sure that mechanical processes, such as hammering, rolling, etc., had nothing to do with the result, the experiments were repeated with four discs of copper electrolytically deposited. The percentage increases in the conductivity were as under:—

3·72 3·17 5·73 4·94

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## S. TERESCHIN—SPECIFIC INDUCTIVE CAPACITY OF SOME ORGANIC COMPOUNDS.

(*Annalen der Physik und Chemie, Vol.* 36, *p.* 792, 1889.)

The experiments were made by the electrometric method, the various bodies, which were very numerous, being successively used to replace the air as the dielectric in an electrometer of special construction.

The results obtained with homologous compounds show that the specific inductive capacity diminishes as the homologous series is ascended, which is opposed to Tomaszewski's observations on the aromatic series. The specific inductive capacities of metameric compounds are different. The differences between the specific inductive capacities of corresponding members of the formates and acetates, and of corresponding members of the formates and benzoates, are approximately equal. It has not been possible to trace any close relationship between specific inductive capacity and molecular weight or other constant. The following tables contain some of the values obtained:—

I.

| | Alcohol. | Formate. | Acetate. | Benzoate. |
|---|---|---|---|---|
| Methyl ... | 32·7 | 9·9 | 7·8 | 7·2 |
| Ethyl ... | 25·8 | 9·1 | 6·5 | 6·5 |
| Propyl ... | 22·8 | — | 6·3 | — |
| Isobutyl ... | — | 8·4 | 5·8 | 6·0 |
| Amyl ... | 16·0 | 7·7 | 5·2 | 5·2 |

II.

| | |
|---|---|
| Ethyl formate ... ... ... ... | 9·1 |
| Ethyl acetate ... ... ... ... | 6·5 |
| Ethyl propionate ... ... ... | 6·0 |
| Ethyl butyrate ... ... ... ... | 5·3 |
| Ethyl valerate ... ... ... ... | 4·9 |

## D. GOLDHAMMER—INFLUENCE OF MAGNETISATION ON THE CONDUCTIVITY OF METALS.

(*Annalen der Physik und Chemie, Vol.* 36, *p.* 804, 1889.)

A powerful electro-magnet was made use of to produce the magnetic field, the magnetisation being measured by means of the current induced in a fixed coil. The resistances were measured by means of a Thomson bridge. The metals submitted to experiment were bismuth, nickel, and cobalt. The paper contains very elaborate tables of the results obtained, from which the author draws the following conclusions:—H is the magnetisation measured in C.G.S. units;

$$\mu \text{ is the ratio } \left(\frac{\Delta r}{r}\right)_{\phi=0} \Big/ \left(\frac{\Delta r}{r}\right)_{\phi=90},$$

where $\phi=0$ means that the bridge current in the plate of metal was parallel to the lines of force, and $\phi=90$ that it was perpendicular.

In the case of bismuth the increase of resistance is proportional to the square of the strength of the field, both when the plates are parallel and perpendicular to the direction of the lines of force. Since, however, in the case of this metal, the magnetisation is also proportional to the strength of the field, it may be said that the increase of resistance is proportional to the square of the magnetisation. It appears also that the greatest increase of resistance occurs in the direction perpendicular of the lines of force.

In the case of nickel the resistance increases in the direction of the lines of force, and decreases in the direction at right angles to them; $\mu$ is, however, always greater than unity. As the increase of resistance is independent of the direction of H, therefore $\Delta r/r$ is a function of $H^2$. The curves corresponding to this equation $\left(\Delta r/r = f\ (H^2)\right)$ follow a remarkable path, viz., they are all asymplotic, *i.e.*, for very large H, $\Delta r/r$ approaches a maximum.

Cobalt behaves very similarly to nickel; but $\mu$ is in this case often less than unity. The maximum change also occurs much earlier than is the case with nickel, *i.e.*, for lower values of H.

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## H. BÄCKSTRÖM—CONDUCTIVITY OF SPECULAR IRON ORE, AND THERMO-ELECTRICITY OF CRYSTALS.

(*Beiblätter, Vol.* 13, *p.* 172, 1889.)

The general results of the experiments described go to show that there is a similarity between electrical and thermal conductivity, though the values obtained are of somewhat different order. It was found that the resistance of rods cut from specular iron ore was the same for all directions in the principal plane of symmetry, but was twice as great along the principal axis. This ratio is, however, diminished by a rise of temperature, although there are no indications of any polarisation.

## B. NEBEL.—A REMARKABLE DISINTEGRATION OF COPPER BY AN ELECTRIC CURRENT.

(*Beiblätter, Vol.* 13, *p.* 177, 1889.)

The instance referred to is that of a Bernstein glow lamp of old type, in which the filament was clipped to thick copper wires by means of spiral metal springs, which were embedded in enamel. The copper wires between the upper end of the enamel and the filament showed signs of peculiar disintegration, fine copper threads, blackened by carbon, which extended from the cracked layer of surrounding oxide.

The author recalls the formation of similar copper and silver growths by the electrolysis of solid sulphides of copper and silver. Possibly the copper wires in the glow lamp contained sulphur.

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## H. DUBS.—THE COUNTER E.M.F. OF THE VOLTAIC ARC.

(*Beiblätter, Vol.* 13, *p.* 197, 1889.)

Two carbon plates were placed side by side and one millimetre apart; they were connected to a galvanoscope. If the lower plate were exposed to the action of a blowpipe flame which carried carbon particles over on to the upper one, there was produced a weak galvanic current, which was opposed in direction to the blast. Also, if a plate strongly heated in a hydrogen blast flame were laid on another, a weak current flowed from the cold to the hot plate. With plates of copper the current was still weaker, and nil with iron plates. This is analogous to the counter E.M.F. of the voltaic arc, and the author thinks that this depends, at any rate in part, on the mechanical force of the current.

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## ANON.—FRITSCHE'S DYNAMO WITH RADIAL-BAR ARMATURE.

(*Elektrotechnische Zeitschrift, Vol.* 10, *p.* 134, 1889.)

The arrangement of the magnets is similar to that in flat-ring dynamos, or in many types of alternate-current machines. The novelty lies in the armature; this is composed of radial bars of iron, which, however, are arranged at an angle to the radius of the circle formed by the armature. The iron bars are connected into two symmetrical groups. The ends of the bars, lying on the periphery of the armature, themselves form the commutator. The whole of the bars form one continuous closed circuit, and the two parallel groups are connected in series, so that the brushes are placed $22\frac{1}{2}°$ apart in a sixteen-pole machine $\left(\frac{360°}{16}\right)$. The particular method of coupling many bars up, allows of a given E.M.F. being obtained with a much lower speed of rotation than in dynamos of other construction. The bars being insulated from each other by air spaces, there is much freer radiation of heat and a greater current density may be safely admitted in the bars.

## F. NEESEN—LIGHTNING CONDUCTORS.

*(Elektrotechnische Zeitschrift, Vol.* 10, *p.* 145, 1889.)

The author, who has had much practical experience of the testing of lightning conductors, insists on the absolute necessity of thoroughly testing them with a galvanometer; any superficial inspection is quite insufficient to discover faulty places, which may be due to oxidation invisible to the eye. He recommends that one terminal of the testing apparatus should be connected to each rod, and the other terminal to the earth connection.

In Berlin, with which city he is best acquainted, the earth is generally formed by the gas or water mains, and their surface resistance is practically nil.

He does not speak very highly of the method of testing by means of alternating currents; the connection wires are in such cases generally coiled on a small drum, and the portion left coiled is liable to introduce errors, owing to induction. In all possible cases the author strongly advocates the introduction of a mechanical joint between the conductor and the earth connection. The two can then be interrupted at pleasure, and each independently tested. He has frequently noticed that the extreme point of a rod which has been struck is more or less bent over, as though the heat of the discharge had sufficiently heated the metal to allow of its bending.

The author gives exact details of a remarkable case in which lightning struck a water-mill in Holland. From the roof of the house the discharge passed down outside the wall, then broke through the wall into the house, and spread itself over a looking-glass. From the glass the discharge passed a short distance down the wall inside, and then broke its way through the wall a second time to the outside, completing its path to earth through a shutter fastened back against the wall.

# LIST OF ARTICLES

RELATING TO

# ELECTRICITY AND MAGNETISM

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Z. ZETLIN—Best Grouping of Cells.—*Beiblätter*, vol. 13, p. 230, 1889.

L. SOHNCKE—Source of the Electric Current in a Battery.—*Beiblätter*, vol. 13, p. 233, 1889.

## II.—DYNAMOS AND MOTORS.

M. DEPREZ—Governing the Speed of a Motor.—*C. R.*, vol. 108, p. 645, 1889; *Lum. El.*, vol. 32, p. 51, 1889.

W. C. RECHNIEWSKI—Rate of Working of Dynamos.—*Lum. El.*, vol. 32, p. 101, 1889.

E. GERLAND—Desroziers' Disc Dynamo.—*El. Zeit.*, vol. 10, p. 199, 1889.

## III.—ELECTRO-CHEMISTRY AND ELECTRO-METALLURGY.

Dr. G. GORE—Loss of Voltaic Energy of Electrolytes by Chemical Union.—*Phil. Mag.*, vol. 27, p. 353, 1889.

H. PELLAT—Potential Difference at the Point of Contact of a Metal and a Salt of the same Metal.—*C. R.*, vol. 108, p. 667, 1889.

N. PILTSCHIKOFF—Electrolytic Polarisation by Metals.—*C. R.*, vol. 108, p. 898, 1889.

A. MINET—Introduction to the Study of Electro-Chemistry.—*Lum. El.*, vol. 32, pp. 126, 175, 1889.

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# JOURNAL

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ERRATA IN PART 80, PLATE AT END.

The middle diagram of Fig. 2 should be labelled "Case *b*1";

And the last sentence of the letterpress underneath should run: "In case *b*1, the rain shower, or a leak, is permissible, but "unnecessary."

paper by Professor Oliver Lodge, on "Lightning, Lightning Conductors, and Lightning Protectors." I see that Mr. Preece is here, and, as Professor Oliver Lodge has designated him his chief antagonist, it is perhaps fitting that I should call upon him to take the lead in the discussion.

Mr. W. H. PREECE: I entirely disclaim, sir, the title, of being considered as the principal antagonist of Professor Oliver Lodge. There is nobody in this room who appreciates more highly than I do the good work that he has done, or who is more ready at all times to support him when he advocates before this Institution the truth. But when he takes the opposite course, and advocates that which I am going to endeavour to prove to you Mr. Preece.

Mr. Preece. to be not the truth, then, if the term antagonist is properly applicable to me, I am willing to accept it. All antagonists are supposed to be, to a certain extent, inimicable to each other, but there never has been any ill-feeling between Professor Oliver Lodge and myself. Scientific controversy is impossible when passion enters.

I am representing to-night a certain school—I am here as the advocate of a conference that sat some years ago, and issued a report, and I am the defender of a class of men who Professor Oliver Lodge has called the "older electricians."

It is essential at the outset that we should clearly understand what are the differences between these two schools; broadly, they may be said to be the principles enunciated by experience, and those deduced from pure theory. Professor Oliver Lodge has announced himself to be the advocate of theory, and I pronounce myself to be the advocate of experience. Well, Professor Oliver Lodge carefully distinguished the other night what he meant about theory. In describing experiments by Kirchhoff, he made this remark: "This, like many another theoretical remark, is not to be supposed true." That is exactly the point that I want to urge—that whatever is theoretical must not at once be assumed to be true. On the other hand, experience is the result of observation, but it does not necessarily follow that our deductions from experience are always exact.

Professor Oliver Lodge has divided his subject into two parts, which he calls case *a* and case *b*.

With respect to case *a*, there is no difference between us whatever. Case *a* is exactly the case that was taken up by the Lightning Rod Conference; case *a* is exactly the same case that I have always taken up whenever the subject has been brought before this Institution; and case *a* is, as I am going to try to prove to you, the case of common sense. There is just one question that Professor Oliver Lodge has raised, and which he takes a little credit to himself for having shown, and that is, that iron is the proper material for lightning conductors, and is better than copper. Well, I have always maintained that iron is as good as copper, from an economical point of view, and

from a purely £ s. d. point of view, I say that iron, under some circumstances, may be better than copper. In the year 1872 I read a paper before the Society of Telegraph-Engineers, in which I pointed this out in the following words: "Hitherto, "in accordance with the directions of Sir William Snow Harris, "in his application to ships, heavy copper rods, ropes, or plates "of great expense had been used, but I find that an ordinary "galvanised iron wire known as No. 4, which is one quarter of "an inch in diameter, and is used so much for telegraphic pur-"poses, is amply sufficient for any dwelling-house." As a matter of fact, there may at the present moment be ten million lightning conductors in this world—I do not know the number, that is a mere guess,—but I do know, of my own experience, of half a million. In America they have used lightning conductors since the days of Franklin, and invariably they use iron; and I am perfectly certain that if you were to take the probable ten million conductors that exist in this world you would find 9,500,000 to be of iron, therefore it is useless or needless to contest the superiority of iron over copper. Mr. Preece.

Just before I branch off from this part of the subject I may state that I have in my hand the report of the Lightning Rod Conference, and there they discuss most judiciously and carefully the relative merits of iron and copper, and the only reason they give in favour of copper as against iron is, that in the long run copper is more durable than iron, and probably cheaper.

It has been sometimes said that the Lightning Rod Conference knew nothing about self-induction, and did not treat the question as though self-induction entered into the matter at all. It is an absurd notion. In this very paragraph that I read it is said most distinctly, "We will assume the conductivity of equal lengths "and weights of iron to be in the case of *steady* currents . . . ," giving the relative resistances of the two; and then, in another part we read, "the suddenness of lightning discharge which "modifies the conductivity when dealing with iron," and the fact that such sudden discharging of electricity affected the conductivity was as well known in the year 1881 as I think it is in the present day. So I say, on the question of case *a*, there is

Mr. Preece. no difference between Professor Oliver Lodge and the members of the Lightning Rod Conference.

But when we come to case *b*, we come to a very different case indeed; and here we have an instance, not only of how electricity behaves itself in the way which Professor Oliver Lodge has defined as an "impulsive rush," but we also find a splendid illustration of the way in which an electrician can deal with questions of this kind in an impulsive rush. I do not want to apply one single idea against the beauty of the experiments that Professor Lodge has brought before us; I do not want to employ one single word to qualify the skill and the ability with which he has handled the question. There is nobody who knows better than I do how earnestly he has worked at this question for now nearly twelve months, and I have watched his progress throughout the whole of this period. But what I do urge is this—and I want to point it out strongly to you—that there is a very vast difference between the conduct of experiments and the deduction that is to be drawn from the results of those experiments. I do not think any of us could have appreciated these new facts that have been brought before us if it had not been for the great advance that has been made in the construction of frictional machines by Mr. Wimshurst. For years and years I had a splendid influence machine that was made by Cromwell Varley, but nobody can believe the difference between it and the Wimshurst machine that gave those magnificent effects we saw at our last meeting. The first point I want to urge upon you is this, that there is no similarity in the action of a Wimshurst machine in the hall of the Institution of Civil Engineers and the production of those effects of atmospheric electricity in the atmosphere which result in lightning. This case *b* of Professor Oliver Lodge is entirely dependent upon the sudden, magnificent, rapid effects produced by a Wimshurst machine; but in nature, in the production of atmospheric electricity, everything is by comparison gradual, stately, and solemn. We do not know how thunder clouds are excited when they cause these brilliant discharges of electricity. It is a question whether there is an operation going on in the interior of these clouds generating those charges of positive and

negative electricity that result in lightning flashes. We do not know whether there may not be some remarkable action in the atmosphere itself, and that the cloud, as it passes through the atmosphere, simply acts the part of a collector. We do not know this; but what we do know is, that throughout all the effects of atmospheric electrification, they are not sudden, just the reverse—they are quiet and slow. Mr Preece.

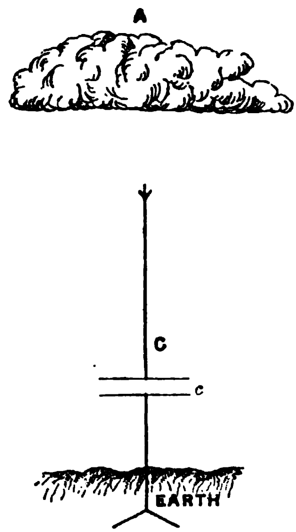
Professor Oliver Lodge has endeavoured to illustrate his case *b* by the diagram (Fig. *b*2) where there are two clouds that have been charged, the one positive and the other negative, and there is below *this* cloud another cloud in a state of neutrality or quietude. Now, by some unexplained action the discharge between those two clouds that produces neutrality is supposed also to induce a charge from *that* cloud to the earth, through the unfortunate church shown in the diagram. How that happens we do not know.

In the case of the experiments shown you by the Wimshurst machine, there was a tin plate of about two or three square feet in area, placed at a short distance above another plate, and the effects were thus produced; there was no doubt about them, we all saw them, and we could all judge for ourselves of their suddenness and of their effects. But there a tin plate was dealt with which, for the purpose, was a perfect conductor; a cloud is a very imperfect conductor, and when I say that it is a very imperfect conductor I say so, on the authority of Professor Oliver Lodge himself.

[Mr. Preece here obtained the assent of the meeting to continue his remarks beyond the usual time allowed.]

Now A is a cloud, and C is a lightning conductor. What I say is, that all the effects that Professor Oliver Lodge showed us the other night are effects that would happen if you were to take a lightning conductor, break it into two at *c*, and insert two tin plates of about three or four feet area, and then between those two plates, if a flash passed, you would have the effects that he showed us in that space; but in nature itself it is absolutely impossible to reproduce such a space between cloud and earth or between cloud and cloud. That which regulates the discharge of

Mr. Preece. a true lightning flash is the *field* of electric force that exists between *this* point and *that* cloud; that which regulates the spark that flows across these two plates is *resistance.* Professor Oliver Lodge's space is resistance—resistance of the air between the two; and all the effects we see *there* are simply to be attributed to the difference of the behaviour of a field and the difference of the behaviour of the resistance of a small column of air. I do not speak without the book here. Experiments in




another form were done very nearly fifty years ago by Mr. Andrew Crosse. I do not know whether anybody in this room has read the "Memorials of Andrew Crosse:" there is no more interesting electrical book published. He established lightning conductors a mile away, upon his estate in Somersetshire, and led them to his room; he charged jars, he formed plates, he performed a great many of those experiments, and he succeeded in reproducing many of the things we saw here the other night, although he was not able to understand them, and he certainly could not have explained them to an audience in the charming way in which my antagonist explained them the other night. Well, gentlemen, as regards case *b*, I want to point out that it is a case of field *versus* resistance, it is a case of suddenness *versus* time,

it is a case of an "impulsive rush," as Professor Oliver Lodge has called it, against steady uniform growth; and what I assert most strongly and most positively is this, that those experiments that Professor Oliver Lodge showed us the other night have absolutely nothing whatever to do with lightning, and any deduction from them is simply erroneous. Mr. Preece.

As regards case *a*, then, I agree; but as regards case *b*, I utterly and absolutely disagree.

I now take the next part, that of the oscillatory character of a discharge. I have been certainly amused to see, especially in the American press, statements that I entirely distrusted or objected to the fact that the discharge of a Leyden jar was oscillatory. I never did anything of the kind. There is no doubt whatever that under certain circumstances the discharge of a Leyden jar must be oscillatory, and it was shown to be so, as Professor Lodge has himself pointed out at the Royal Institution, and by one of my old masters, Professor Henry of New York. It was proved to be so by Professor Helmholtz, or rather he threw it out as a suggestion in 1847, and it was proved that it must be so under certain circumstances, by Sir William Thomson in 1853. Now, what makes me say it must be so? Why I do so is this, that if in the path of the discharge of a Leyden jar you insert sensible self-induction, then there must be this remarkable action that results in the production of an opposing electromotive force that tends to excessively rapid vibrations of charges between the two plates. In fact, Professor Lodge himself has gone so far as to calculate what takes place, and he has done a great deal more than calculate here, for he has shown the effect by experiment; and those who were present at the charming lecture he gave at the Royal Institution will remember well how he made it convincing to all of us, that by inserting self-induction in the circuit of discharge of a Leyden jar, oscillation was rendered evident to our ears, for, as he increased the amount of self-induction present, he lowered the note, falling from a high note to a low note. I say that while it is possible to have this oscillation in the case of a Leyden jar, it is impossible in the case of lightning. I took up this

Mr. Preece. point very strongly at Bath when we discussed this question before, and I gave various reasons then why I did consider it possible that lightning could be oscillatory. A lightning flash is a crack between the inner and outer coatings of a Leyden jar, where the conditions that determine self-induction do not exist. The current must be unidirectional. Needles were magnetised by lightning, as they are under certain circumstances by Leyden jars. That was the weakest of all my arguments, and it is the one that has been most seized upon; but I think that Professor Ewing has given very good reasons why it may not be strictly true, and how, under certain circumstances, needles may be left magnetised even with an oscillatory discharge. But we will waive the question of needles; we will take another. We will take the question of magnetising iron wire. I agree absolutely with Professor Lodge in this, that if there be self-induction present, there must be oscillation; and he, I am quite sure, will agree with me if I say that if there be no oscillation there is no self-induction.

Well, now, lightning can speak for itself; it does not require Professor Lodge, who has imbued flashes of electricity with a certain sentient power of selecting their proper path, to speak for it. In days gone by we used to work the telegraphic system of this country with a beautiful system, called Bain's system, which was one by which the dots and dashes were recorded on chemically moistened paper in blue dots and blue lines, and it was necessary that a positive current should pass through this moistened paper in order that electrolysis might take place, and so produce the chemical changes that resulted really in the formation of Prussian blue. The character of the currents, whether constant or variable, whether positive or negative, whether uniform or vibratory, are recorded with clear, unmistakeable signs There are men in this room—my friends Mr. Spagnoletti and Mr. Graves, and no doubt many others—who remember the Bain's instrument well, and who know that there never was a lightning storm passing over the country that did not record itself in these blue characters on paper—long lines, letters, and marks. Now, if a lightning flash was oscillatory in its character, if it was

a succession of positive and negative currents following each other at the rate of millions a second, would it have been possible to make a uniform line on the paper? But more than that, I have seen a lightning conductor, where it entered the ground, that was marked with a copper of a beautiful bright colour, indicating that that the charge, whatever it was, that passed through it must have acted electrolytically upon the copper and left a deposit of pure copper behind. Every submarine cable throughout the world, of any length, is worked by the Thomson recording syphon, and there is never a storm, even though it takes place on the line of the cable a thousand miles from the shores of the Atlantic at Valentia, that is not shown on the recorder by "kicks" and marks indicating the production of a current. Would there be such kicks and marks, however, if those lightning flashes and currents were oscillatory? A very careful observer, Mr. Burke of Nevin, to whom I am very deeply indebted for the experiment, and who made it entirely "off his own bat," only last Sunday, was in charge of one of our relay stations in North Wales, and, without a word from me, he allowed, during the passage of a severe thunderstorm, his Wheatstone receiver to run. Now the Wheatstone receiver makes its marks through the action of a polarised electro-magnet, and it is quite impossible that any such magnet having high resistance and great self-induction could give any indication whatever if the currents discharged from these lightning flashes were in the least degree oscillatory in their character. Ewing's explanation of the magnetised needle does not apply here. We are dealing with unidirectional currents of great and uniform strength and of sensible duration. Now Mr. Burke allowed his receiver to run for half-an-hour, during the whole time that the storm passed. Lightning flashes passed at an average of about every 30 seconds, and every lightning flash that passed over Nevin, within the range of the Snowdon mountains, recorded its mark in clear characters, so that "he who runs may read," and I will defy any man in this room to say that there is the slightest indication on that paper of any oscillation in those lightning flashes. Mr. Preece.

One more very important illustration is this: if the discharge

Mr. Preece. of a lightning flash be oscillatory, if there be self-induction in the lightning conductor or the wire taking the discharge, Professor Oliver Lodge has told us, and we all know, that under such circumstances the charge will not enter the wire. What happens, then, whenever a lightning conductor is struck? What happens whenever a telegraph wire is struck? It is fused. It is fused so beautifully that you can see——I am very sorry I have not specimens here, I have one for the purpose of illustrating another point; but these fusings, these broken wires, have been for years and years past so common, that they are not curiosities, and we do not care very much about them; but invariably when a wire, and especially an iron wire that is supposed to be so free from oscillatory currents in its interior, whenever such a wire is fused by lightning, there are distinct indications that the fusion commenced in the middle, and not on the exterior.

Another point is this. If there is one case where self-induction would resist the passage of an oscillatory current through it, it is in the case of electro-magnetic apparatus. Relays, Morse recorders, electro-magnets, bells of all kinds have very high self-induction; the self-induction of a Siemens relay, for instance, is very high. I forget the exact figure, but it is, in the language of Professors Ayrton and Perry, five or six secohms. Well, now, if it were possible for a Siemens relay to speak to an oscillating flash of lightning, it would say, "You must not "come this way, but must go some other way; do not come "near me." But what is the result if the apparatus is struck? It is fused, not outside, but in the interior; and when you find a relay or an electro-magnet fused by lightning you will find clear indications in the inside of it that the electricity has gone through and through it, giving unmistakable evidence that lightning is not oscillatory in its character.

I think I have given you sufficient evidence to show that experience on this particular point is against Professor Oliver Lodge.

I will take the next point, and that is the efficiency of lightning protectors. There is no proverb in my experience

that is so true as this, that "nothing succeeds like success;" and Mr. Preece. if success is to be taken as a criterion of the efficiency of lightning conductors, then lightning conductors act according to the theory of the Lightning Rod Conference and against the theory of Professor Oliver Lodge.

Take Her Majesty's Navy. There are ships in Her Majesty's navy that have been for fifty years fitted up according to the principle of Sir W. Snow Harris. Since 1872 there has not been an accident recorded in the Admiralty, although there are always 500 or 600 ships under commission in the navy, sailing in every sea, subject to every climate, at all times and in all seasons.

Take the War Department. We have two admirable representatives of that department present to-night, and in that department they have applied this principle to their magazines all over the world; and, although I do not know what they are going to say, I am perfectly certain they will say this—that the system in use is sufficiently successful, and represents the doctrines that have been promulgated here. Take the churches. Does anybody know how many churches there are in this country? In England alone there are 13,500 churches protected by lightning conductors, and if one is struck it is announced in all the papers and creates a great fuss; but it is only one in 13,500.

Take lighthouses. My friend Sir James Douglass, who has all the lighthouses under his charge, is here. I do not know what he is going to tell you, but I feel convinced that there has never been a lighthouse struck by lightning since they came under his charge. Take telegraphy. Here is Mr. Graves, who can speak of over half a million of poles throughout the whole of this country; they are all protected with lightning protectors, most of which were not put there originally for lightning conductors, but really were found to be so, and similar ones have consequently been of late years put up specially to protect our poles from lightning.

Take apparatus. On this point I want to put myself at direct issue with Professor Oliver Lodge, who says, "The ordinary

Mr. Preece. "and well-known form of lightning protecting arrangement is "to attach a pair of plates, or a double set of points, or a pair "of points in a vacuum or some very small air space, as a shunt "to the instrument or coil of wire to be protected. Here are "arrangements of the kind;" there were the other night. Now he says this—mark the words please, gentlemen: "Now it is "perfectly easy to see that the protection such things afford "is of *the most utterly imperfect kind.*" Well, there is a good deal of charming coolness about that statement, because I can speak from authority and experience, and I can give him some figures, and he cannot give me any. In 1872, when we discussed lightning conductors here, I pointed out that in the half-year from January to July, 1872, there were 9·46 per cent. of the instruments in this country damaged by lightning. That was only for the half-year, and that was not the worst part of the half-year, which is generally the autumn, and if we double that it would show that in 1872 just 19 per cent. of our instruments were damaged by lightning. I believe that would be quite true. What is the result in the present day. The fact is—and this is one of the great benefits of bringing a paper of this character to this Institution—it often really teaches those who write papers a great deal more than it teaches those who listen to them; and I learned from writing that paper in 1872 what serious damage had been done to our instruments, and at once proceeded to urge lightning protectors and to apply them. Well, now, in the twelve months ending March 31st, 1889, instead of the damage to instruments being 19 per cent., we have applied this "utterly imperfect apparatus," and have brought the damage down to 1·3 per cent; so that at the present moment our apparatus is only damaged to the tune of 1·3 per cent. My own impression is that even that 1·3 per cent. is due to the fact that we have not applied lightning conductors to all the instruments, and that the instruments damaged were not protected. I assert this decisively before you, that there is no more perfect instrument in this world than this lightning protector that Professor Oliver Lodge recklessly asserts is of the "most utterly imperfect kind."

Now, Professor Oliver Lodge was kind enough to criticise some of the conclusions to which the Lightning Rod Conference came, and expressed in their report. Critics, especially if they are inclined to be hostile—mind you, I do not use the term hostile in a bad-humoured sense; I know Professor Oliver Lodge means to criticise good humouredly our proceedings;—but when a critic takes up a paper or a book to criticise it hostilely, he picks all the holes in it he can, and you can generally judge from the number and character of the holes the value of the criticism. Professor Oliver Lodge has picked three desperate holes in this report, and what are they? The first is that the Lightning Rod Conference had the impudence to say that there is no authentic case on record where a properly constructed conductor failed to do its duty. Well, there never was a truer statement made; and when we say a "properly constructed conductor," we mean a conductor constructed in accordance with the rules of the Lightning Rod Conference, and do not mean one constructed in accordance with the rules of Professor Oliver Lodge or any other person. But we assert, and re-assert, there is not a signatory to that Lightning Rod Conference Report who will not stand up in this room and assert, as I do, distinctly and decisively, that there is no case on record where a properly constructed conductor has failed to do its duty. Mr. Preece.

Then as to the second point, on which I cannot speak from experience, for I have not yet had the opportunity. I do not say I would not do it if I had the chance, but I have not. "A man may with perfect impunity clasp a copper rod an inch in diameter, the bottom of which is well connected with moist earth, while the top of it receives a violent flash of lightning." Gentlemen, I would not hesitate for one moment to do it. If you were to give me a solid copper rod, one inch in diameter, fixed with any good earth, and put me within the range of a good thunderstorm, I would stand there for the good of science and for the honour of the Institution of Electrical Engineers!

Then No. 3 is this, that "if all these conditions be fulfilled; if the point be high enough to be in the highest position of a building, no matter from what direction the storm may come;

Mr. Preece. "if it be of ample dimensions and in thorough, perfect electrical connection with the earth, the edifice with all its points "will be safe, and the conductor might even be surrounded by "gunpowder in the heaviest storm without risk or danger." If I am prepared to stand with my hands clasped round a copper rod, I think I should be equally prepared to sit upon a barrel of gunpowder without fear of personal results.

Those are the three points; and what is the conclusion that my friend comes to? "To adhere," says he, "to such a "view as this now, with tenacity sufficient to cause it to be promulgated as an authoritative scientific statement, would be, in "my opinion, little less than criminal." Well, like some Irish patriots, I am criminal, and every signatory to that Lightning Rod Conference Report is criminal; and why? Because he believes in himself, and not in Professor Oliver Lodge.

I will not say much about the accidents, but if you read the records which he has made, and will compare them with the principles of the Lightning Rod Conference, then you will find that the accidents may be just as easily explained on the principles of the Lightning Rod Conference as they can upon those of Professor Oliver Lodge.

I should like to have criticised one or two more points, but time flies so quickly and I have already overtaxed your patience. I will simply add that this language that I have referred to, in the Lightning Rod Conference Report, is language not addressed to the Institution of Electrical Engineers, it is not addressed to the Physical Society or to the British Association, but it is addressed to all the householders of this country, to people who build houses and to people who live in them, people who have interest in schools, halls—vicars, churchwardens, and people who are interested in the safety of their structures; and why should we go and talk to them of the "value of L," or of the oscillatory character of a lightning discharge, or of the views of Professor Oliver Lodge? We like to speak to them in plain anguage that they can understand, and they can perfectly understand such language as we used; in fact, the whole tenor of the report of the Lightning Rod Conference is to instil in the public

mind an idea of safety and confidence in lightning protectors. Mr. Preece. What does Professor Oliver Lodge do? He labels every lightning protector with a great notice, "Beware of this; it "bites, it fizzes, it spits, it does all sorts and kinds of dangerous "things; do not trust it, it is of no use. Sleep in your house "in comfort, and do not mind those Lightning Rod Conference "people."

I should just like to make one more reference. Professor Oliver Lodge has drawn a great distinction between what he called the old and the new school of electricians. As I mentioned just now, I was brought up under the influence of Faraday, Snow Harris, Wheatstone, and of the old electricians. There are very great differences between the two schools; I cannot refer to all of them. There is, however, one great difference, and it is that the old electricians were tolerant—they were tolerant of their opponents, they were tolerant of opposite views, they listened to the arguments of all sides. But what of the present, the new school? The new school, in the language of Professor Oliver Lodge, says of the old school, that "it is time that the prophets of that old superstition were slaughtered by the brook Kishon." We do not retaliate. The poet Cowper said:

> "O for a lodge in some vast wilderness,
> Some boundless contiguity of shade,
> Where rumour of oppression and deceit,
> Of unsuccessful and successful war,
> Might never reach me more!"

My greatest wish is that Professor Oliver Lodge may find himself in some such desert, subjected to lightning flashes, surrounded by coils of wire and armed with apparatus, and I am sure he will speedily come to the conclusion that it is better to come back to the arena of the Institution of Civil Engineers in sack-cloth and ashes, pronounce that he was wrong, join the old school, and cry *peccavi*.

Mr. Charles W. Vincent: May I say a dozen words as a note Mr. Vincent. to the address which Mr. Preece has given, especially with reference to the paragraph respecting "oscillation?" Many more

Mr. Vincent. years ago than I am quite willing now to recollect, I assisted Professor Faraday in preparing a number of lecture experiments with the electric machine of the Royal Institution (one of the largest in the country at that time) and the Leyden battery of fifteen large jars. The experiments made were of the discharge of this battery through various substances, wires of different thicknesses, and other conductors; but in no case was there any sign whatever of "oscillation" in the sense that the current was sometimes in one direction and sometimes in the opposite. "Oscillation" in the sense of retardation of the battery discharge, by the substitution of a number of successive impulses for a single great one, he had in plenty. As is well known, such "oscillations" are the ordinary means of discharging any body which has electricity induced in it. Imperfect conductors, conductors offering resistance, converted the charge of the powerful battery into a feeble spark discharge, or, rather, continuous current, which deflected a galvanometer, precipitated iodine from iodide of potassium, decomposed various salts, the current from the Leyden jar being proved to be a true current. There was no indication of oscillation in the sense of reflex action. Had it existed, Faraday, with his keen perception, would have immediately perceived the "back current." Electricians, unless they *know*, are not very ready to rush into a fray, but I imagine the majority of them do not accept the idea of oscillation in the discharge of a Leyden jar, if by that is meant a current alternate in direction.

Mr. Farquharson. Mr. J. FARQUHARSON: Gentlemen, I should like to say a few words, not so much as touching on Professor Lodge's paper, as on the remarks of Mr. Preece; having had considerable experience in ships' lightning conductors during a period of twenty years at the Admiralty, during which it was part of my duty to test and examine when accidents occurred. The system adopted by the Admiralty is that introduced by Sir W. Snow Harris. The topmasts and topgallant masts being portable, the automatic connection of the conductor at their junctions is liable to get out of order. Generally speaking, when ships have been struck, I have found little difficulty in seeing how the

damage may have occurred, on the assumption that the conductor was at the time imperfect; but several cases have occurred in which I am quite unable to understand or explain what actually happened. As an illustration of my difficulty, take a typical case—H.M. gunboat "Coquette" (I believe Mr. Preece has the particulars, as reported by me some sixteen years ago). The main topmast was struck and damaged, although fitted with an ample conductor, which was uninjured. The conductor on the main lower mast was also uninjured, but the portion leading from the mast to the ship's side was torn from the beam although it had been well secured by 1½-inch nails, but the conductor itself was uninjured and showed no signs of heating. The wood hammock-berthing next the mainmast shrouds was torn to pieces for a distance of from 15 to 20 feet. On examination a fault in the main conductor on the lower mast was found to exist a few feet below the masthead, where the conductor had been cut and imperfectly joined. The resistance below this point, including the portion torn from the beam, was found to be nil by a bridge, only going to $\frac{1}{10}$ ohm. What I fail to understand is why the damage was done below the fault, where the conductor was ample. Assuming that the resistance at the fault sent a portion of the current down by the iron-wire shroud, it must have leaped a distance of 4 feet to the wood hammock-berthing, itself a bad conductor. Why this circuitous course should be taken, instead of the iron chain plates leading from the shrouds in a direct line to the water, I do not understand; nor why it should make such a leap at all, seeing that the painted butt at the fault could have been bridged by a small fraction of an inch. These facts have come under my own notice, and I find it difficult to account for them on the basis of the accepted theory of atmospheric discharge. Mr. Farquharson.

Mr. J. WIMSHURST: Sir, Professor Oliver Lodge states that there are two main cases of lightning flash, the one of gradual growth, which he calls the A flash, the other when the strain arises so suddenly that there is no time for any pre-arrangement of its path. The first he calls the steady strain, the second the impulsive rush. I think it is important to recognise the difference Mr. Wimshurst.

Mr. Wimshurst. between the two cases, for it was the first, or the A flash, that was contemplated by the older electricians, and Professor Lodge's experiments have called attention to the other case. Certainly I did not know that there was anything new about the B flash, or the impulsive rush, for more than five years ago I made some influence machines, in which the terminals were arranged at the back of the stand, from which this impulsive rush was to be taken. Professor Lodge, Mr. Preece, and Professor Forbes, I know, have these machines in their possession, but certainly, even so far back as that, I did not consider there was anything new about the impulsive rush.

Professor Lodge next illustrated what he was pleased to call the older, or the *a* conditions, with the dome, the knob, and the point, and he showed very clearly, with the conditions he gave, that the point protected everything near to it, notwithstanding that considerable resistances were introduced between it and the earth. In these experiments, however, he must have made the cloud negative, and there is a very wide difference between making the cloud negative and the cloud positive. He then illustrated the B flash, or the impulsive rush, in which he showed the point afforded no special protection, for the dome, the knob, and the point were each struck, one as readily as the other, according as it was the higher. It appears to me to be clear that some additional illustration is required, for it is difficult to believe that the older electricians had such a limited knowledge as is now attributed to them. As shown by Professor Lodge, the cloud evidently was positively electrified, hence they were all equally struck. I purpose now to show you the great difference in the behaviour of the spark when the cloud is made negative. In this case you will see the point again affords the protection required of it, for it is always struck in preference to the dome, although it be twice the distance from the cloud. In fact, the only difference which I can find between the steady strain and the impulsive rush is that in the former the charge is dissipated slowly, as a brush discharge, while in the latter the actual spark occurs from the point. This arises from the shorter time to effect the discharge. The B flash, however, is always weaker than the A flash.

I have an arrangement here similar to that used by Professor Lodge. The metallic cloud is supported on a glass pedestal. Here is a board with tin foil on the top; here a metal dome with metal connection; here is a piece of fine binding wire, which I shall press down by means of this rod, and you will see the variation of the length of the spark. I purpose showing you first the conditions when there is little preference. We will turn the machine and show its polarity. [Machine does not act readily.] This machine is now nearly five years old; it has never yet hesitated as at the present moment. Mr. Wimshurst.

Professor OLIVER LODGE: The failure of a Wimshurst machine is of itself a striking experiment. Professor Lodge.

The CHAIRMAN: Would it be satisfactory to you, Mr. Wimshurst, if somebody else went on with the discussion while you are preparing your experiment? Dr. Hopkinson.

Mr. J. WIMSHURST: Yes; quite so.

The CHAIRMAN: Perhaps Professor Adams will go on.

Professor W. GRYLLS ADAMS: I regret, sir, that in the investigation of the subject of Leyden jar discharges and their supposed application to flashes of lightning, Dr. Lodge should give such a sweeping condemnation to everything with which he may not quite agree, or that he should think that the results of previous scientific research and investigation are so easily to be swept away. The discoveries made by Hertz, with regard to the discharges of Leyden jars, are of very great interest, and must modify our ideas on the subject; but when Dr. Lodge says that lightning discharges are only the discharges of Leyden jars, and asserts that all the effects observed in sudden discharges from two plates of metal near together must also be true of lightning, without taking into account the effects of distance, capacity, and time of discharge, he begs the whole question. The discharge of lightning from a cloud, consisting of detached portions of matter with very slight powers of conduction, is something more than the rapid discharge between the metal coatings of a Leyden jar. It will also appear that any effects of resonance or side flashes with which Hertz or Dr. Lodge have as yet made us acquainted are small compared with the original electrical disturbance which Professor Adams.

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is competent to produce them; and on this account the lightning discharge itself is far more to be guarded against than any effect of resonance or side flashes in conductors which may result from it. If, as Professor J. J. Thomson has shown, a high polish is necessary to get these subsidiary discharges, even in bodies of a suitable capacity to give forth resonance discharges, then they are not so likely to do damage as the original lightning flash.

As Chairman of the Lightning Rod Conference, I must dissent at once from Professor Lodge's statement that the older electricians, from Franklin to Faraday, knew nothing of the real conditions of the problem; neither did they nor the members of the Lightning Rod Conference treat it as at all an easy matter. The Lightning Rod Conference took it up because there was great confusion about it, and this is how they put it in their report: "To consider the "*possibility* of formulating the existing knowledge on the subject "of the protection of property from damage by electricity." Now, it is perfectly clear that if they questioned the *possibility* of a thing they could not have regarded it as easy. The members of the Lightning Rod Conference did a very good piece of work when they collected together and threw into a condensed form in their report all that was known up to that time with regard to lightning conductors. Their report is the condensation of that knowledge as gathered from the evidence given in the volume itself, and affords the true scientific basis for the construction of lightning conductors. Nor am I as yet aware that the scientific basis, the combined knowledge and experience of the past, has been overturned by the conjectures or surmises as to lightning flashes which Dr. Lodge has put forward, arising out of the discoveries of Hertz as to the discharges of Leyden jars. I regret that I have not had more opportunity of seeing Dr. Lodge's experiments, either at the Bath meeting of the British Association or at the meeting of this Society a fortnight since. Let me draw attention to some of the points referred to by Dr. Lodge in his paper, and see what were the views of the members of the Lightning Rod Conference upon them.

Professor Lodge says that the Belgian electricians (I should rather say M. Melsens) have been using for years the method

which he himself is now recommending for the protection of buildings, and that he has seen no occasion to modify his conclusions on the forms of lightning conductors. At the Bath meeting of the British Association, Dr. Lodge stated of the Hotel de Ville, at Brussels, that "everything was carried out in the most approved "style, regardless of expense," and speaks of it as the best protected building in the world. The account which Dr. Lodge gives of the protection given to to the Brussels Town Hall is altogether exaggerated. He says: "No electrician exists but would a year "ago have asserted that it was absurdly and exaggeratedly safe "from damage by lightning." Yet within the past year this building, so amply protected by M. Melsens, has been struck by lightning. Now, if we refer to the Report of the Lightning Rod Conference, we find that the system of protection of the Town Hall at Brussels transgresses the rules laid down by the Lightning Rod Conference. On page 138 there is an account given of the actual system set up by M. Melsens to protect the Brussels Town Hall, and it appears as clearly as possible that it is not what the Lightning Rod Conference would regard as a perfect system. In the report, some eight distinct characteristics of M. Melsens' system are enumerated, and whilst most of them we should all certainly agree with, there is one which is directly contrary to the express instructions given in the Report of the Lightning Rod Conference. The account says: "The conductor consists of numerous thin "wires, which are very flexible, so as easily to be led round all the "corners of the building." Now, in the instructions of the Lightning Rod Conference, it is stated that no conductor must be bent at a greater angle than 30°; and to objections raised to the use of copper tape, the Committee say: "The objections to tape will be "found to be objections, not to tape *per se*, but to bad practice on "the part of some persons who have fitted it up, and availed them-"selves unduly of its flexibility." Now this applies far more to M. Melsens' fine wires than to copper tape, and this is the system which the Committee condemns beforehand, but which Dr. Lodge advocates and wholly approves, and in spite of which, or possibly in consequence of which, the Brussels Town Hall has actually been struck. Now, does Dr. Lodge think that when the Lightning

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Rod Conference object to such bending of the conductor, they have no thought of the effect of self-induction or electric inertia? which is no such new thing as he suggests, but which was known to Faraday and to those who followed him.

Let me draw attention to a few other statements in Dr. Lodge's paper. He says, in paragraph 41, that the instances there given illustrate side flashes and surging circuits, but certainly in two of the three instances given the gas is set fire to because the gas-pipe is melted, being in contact with, and forming the main conductor. I doubt if even, according to Dr. Lodge's principles, the side flashes can have sufficient energy to melt a gas pipe, when the main discharge is carried away by the lightning conductor without doing any other damage. As regards the third instance, Major Majendie's account may be trusted, and quite accounts for the accident, even omitting the words which Dr. Lodge encloses in square brackets.

In paragraph 42, Dr. Lodge misrepresents the work of the Lightning Rod Conference. The case cited (see L.R.C., p. 115) cannot be taken "as illustrating that a good conductor affords no "absolute security," but is distinctly quoted by L.R.C. as showing how a faulty conductor may be the cause of damage. "The flash "of lightning fractured the conductor in fifteen places," as stated in the account given in L.R.C. "The inefficiency of the con-"ductor resulted from the carelessness with which it was fixed; "the line was fastened with twenty-five wall eyes, which were "hammered too deep into the wall, thus damaging the line, and "forming a short and sharp bend in each case, besides also unduly "straining the wire." This is what Dr. Lodge calls a good conductor. Is it at all likely that a good conductor will be fractured in fifteen places by a lightning flash?

In paragraph 41, Dr. Lodge quotes from the Report of the Lightning Rod Conference. "It is no uncommon thing for build-"ings provided with what are called lightning conductors to be "damaged by lightning, and the cause is due to the inadequacy "of the conductor to carry the electric fluid, which will leave the "conductor for a better or a larger conductor." He should have continued the quotation, and he would have seen that there was

cause for this statement. This is the continuation: "The lightning descended the conductor to a certain point. At this point "the iron flue enters the shaft, but some distance from the "conductor; the mass of metal located there was a better conductor than the wire rope, so that in leaving the rope for the "better conductor, the electric fluid passed through the brickwork "and caused the damage." He might also have cited the case of the Board House at Purfleet (see L.R.C., p. 78). "The "lightning struck one of the iron cramps that hold the coping. "The iron cramp was situated over a plate of lead, and the ends "of it inserted in the stone came within 7 inches of that plate, "which communicated with the gutter, and served as a fillet to "it; this gutter was part of the main conductor of the building. "The lightning struck through the stone to the corner plate of "lead, fusing a small portion of it." Now, can it for one moment be supposed that this bursting through stones and melting of lead can be due to secondary discharges or side flashes, rather than to the diversion of the whole or the main part of the discharge, because of the presence of a more ready conductor? Professor Adams.

In paragraph 47, Dr. Lodge quotes from Mr. Gavey's account of a church being damaged by lightning at Cardiff, but does not mention that the earth contact gave a resistance of 115 ohms. The members of the Lightning Rod Conference would scarcely have been satisfied with Dr. Lodge's mode of accounting for the damage by begging the question. For instance, in paragraph 43, he puts down to secondary disturbance the setting on fire of buildings, where there is no evidence at all as to how the fire took place. The Committee preferred to say, "The precise cause of "the fire was not ascertained."

Dr. Lodge quotes a passage from our report as "illustrating "that a poor earth is not necessarily fatal to the efficiency of the "conductor." The quotation rather shows, and is intended to show, the necessity of good earth connection to prevent damage, "the ground was upheaved where it was too dry." In speaking of lightning discharges, there is a great tendency to regard it as simply a discharge of electricity of one kind which starts from the cloud and proceeds through the air and the conductor to bury

Professor Adams. itself in the earth. I think we should rather regard it as a rushing together from the two ends of the positive and negative charges, and a meeting of the charges in the middle, as shown in Wheatstone's original experiments on the velocity of electrical discharge. I think that this explanation will fit in better with many of the phenomena which are observed in lightning discharges. Thus, when the cloud has a positive charge upon it, the earth and lead roofs and gutters, and all conductors having good earth connections, have an induced negative charge upon them before the lightning discharge takes place—that, in fact, the negative charge from the earth which is to reduce the potential of the positive charge from the clouds is to a great extent already outside the buildings, at the top of metal roofs and good lightning conductors, so that the principal part of the shock takes place in the air above the buildings so protected.

From this point of view we may see that a good earth is most important, for a bad earth prevents the negative charge from spreading quickly and with sufficient rapidity to the tops of the buildings and the conductors before the actual disruptive discharge takes place. Under the heading (G), Dr. Lodge states that "in the case of sudden rushes there is no time for a path to be prepared by induction." Is he then prepared to say that the discharge takes place without previous induction? In such a case is there nothing to determine whither the charge shall go? Or would he suppose that there is action at a distance? Under the induction of the charge in a cloud, the lightning rod with a good earth connection carries the earth's charge up to the top of the lightning rod, and to all metal on the tops of buildings connected with it, or connected to earth, so that the building below is, as it were, enclosed within a charged conductor, and the main effects of the discharge take place between the tops of buildings and the cloud. Starting from both ends at the same time, the clashings and oscillations, if any, take place for the most part in the upper air. If, as Dr. Lodge directs in his rule 75, large masses of metal are connected to earth, but are not at the same time connected to the lightning conductor, then the discharge from the cloud may take place to the large masses of metal, and

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may strike and scatter stones and brickwork in the process, and I believe it is most unsafe, as he directs in his rule 76, to earth things independently. Had Dr. Lodge known how common a fault it is for a workman to put up a lightning rod without making good earth contact, he would probably have urged quite as strongly as the members of the Lightning Rod Conference that special care should be taken to make a good earth, for it is the one defect which they found to be most common, and of which they could not urge the importance too strongly. Dr. Lodge now says that he does not now argue against the need of a good earth, but I believe Dr. Lodge suggested at Bath that points might be placed at the bottom of the lightning conductor just as well as at the top. With such an arrangement, I believe—and my impression is derived from facts which were before the Lightning Rod Conference—that the lightning rod, as far as it would do anything, would be a cause of mischief. We had many instances of the ill-effects produced by broken or faulty conductors. Take, for instance, the Caterham accident (L.R.C., p. 210), which is worthy of careful study, to show how a lightning conductor should not be constructed. In this case the copper tube was carried about 12 inches into dry chalk. Parts of the conductor consisted of copper tubes screwed together and giving no proper electric contact, some parts of the conductor being fused by the discharge.

In one case, the bottom end of the conductor was led into a bottle: perhaps the man who put it up had some idea that a lightning flash was like a Leyden jar discharge. The kind of earth contact often met with is not unlike the case met with at Middlesbrough (L.R.C., p. 203), where a good conductor was carried underground for 9 ft. from the chimney and terminated at a depth of about 4 ft. in hard, rather dry clay, the end being wrapped round a common brick buried in the ground. The conductor making earth contact was a flue entering the shaft, and in leaving the proper conductor for this flue the discharge passed through the brickwork and caused the damage.

There is one other case to which I will refer, which is not recorded in the Report of the Lightning Rod Conference. On May 17th, 1885, at 2 p.m., the Lower Lighthouse at the South

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Foreland was struck by lightning. I was asked by the Elder Brethren of the Trinity House to examine and report as to the cause of this lightning stroke, it being supposed that the lighthouses were well and efficiently protected. As described in Appendix I. of the Report of the Lightning Rod Conference, p. 185, the instructions for protecting lighthouses were drawn up by Faraday, and if his instructions had been fully carried out at the South Foreland there would probably have been no damage done. On examination it was found that there was a very excellent lightning conductor in the shape of a half-cylinder, 1¼ in. by ⅝ in., attached to the iron-work of the lighthouse, passing down the inside of the wall from top to bottom, and making perfect contact with the earth plate, which was 4 ft. by 2 ft. 6 in., and about $\frac{1}{16}$ of an inch in thickness, of very excellent copper. Faraday's instructions for the securing of lighthouses (as detailed in L.R.C. Report, p. 186) seemed to have been completely followed out; but on following the conductor to the earth plate it was found that the workman who carried out the work had placed the copper plate at a distance of about 4 ft. outside the building, lying flat at a depth of only about 1 ft., or at the lowest part 1 ft. 6 in., below the surface of a gravel path in dry chalk *debris*. The large copper plate had just been put out of sight in the driest place possible, where it could have no proper earth connection. To obtain good earth contact in the chalk is by no means an easy matter, hence the large size of the plate used in this case; but in this, as in many other cases, the object was defeated. The Lower Lighthouse stands on the edge of the cliff, at a height of 290 feet above the level of the sea, and is about 540 feet to the east of the engine-house. In order to get water for the engines, it is necessary to go down to sea level. The depth of the well by the engine-house is 280 feet. But what became of the flash of lightning which struck the conductor and ironwork of the lantern of the lighthouse? The internal ironwork, on which one of the keepers was standing at the time, was in connection with the lightning conductor, and in that framework there was a bolt, the head of which was about two inches below the

axle of the trolly which carries the electric lamp. The lightning passed from the bolt-head to the axle of the trolly; then from the trolly by a screw, tearing away part of the screw-head and depositing it on the base of the electric lamp, which was injured; then to the insulated terminal of the electric light mains and away by the underground mains, a distance of 180 yards, to the engine-room, through the Holmes magneto machine, demagnetising the magnets; and then by some glow lamps connected to the mains and supported from the metal tie-beam of the iron roof, which has a lightning conductor leading to a big tank, which is in direct connection with the metal work of the pump leading into the deep well of the engine-house, which reaches to the sea level. In the magneto machine a large spark a quarter of an inch in length passed between the iron and the copper conductor. One of the keepers was inside the lantern, standing on the ironwork, which was in good electrical connection with the part of the lantern struck, and with which the bolt which carried the discharge to the axle of the trolly was connected. Thus the good earth, by the metal work of the pump being raised from the sea level to the roof of the engine-house, supplies the earth charge which reduces the potential of the lightning flash from the cloud; here a very powerful discharge passes along good underground conductors over long distances and does very little damage. We may well say, with Lord Rayleigh, that "it is only by actual experience of lightning "conductors on a very large scale that the question can ever "be finally settled." Laboratory experiments may be most important as suggestions, but no one would adopt or change any system of lightning protection without actual experience. Professor Adams.

Mr. J. WIMSHURST [proceeding with experiment]: I will now make the cloud positive, and you will see that all of them (the dome, knob, and point) now are about equally struck. The point, as you see, gets a slight preference. These conditions only were shown us the other evening. But now let me make the cloud negative, and you will see a very great difference. There is 125 per cent. in distance in favour of the point, and that is with the impulsive rush. I think that shows that there is a manifest advantage in favour of the point. Mr. Wimshurst.

Professor Lodge.

Professor OLIVER LODGE: I did not notice whether it was positive or negative. It was sometimes one, sometimes the other.

Mr. Wimshurst.

Mr. J. WIMSHURST: Indeed. But there is great difference in the behaviour of the spark with the positive as compared with the negative cloud. [Experiment.]

Let us consider three of the most important points. 1. The enormous resistance, as Professor Lodge calls it, of the large copper conductor. 2. The small importance which Professor Lodge places upon a thoroughly good earth: he says it may be made as good as heaven, but still be useless. 3. The propriety of connecting all metal in the house as in a ring, and connecting that ring to earth.

To test the first of these, we had better repeat the experiment which Professor Lodge calls the by-pass: it consists in giving the Leyden jar discharge the choice between the conductor to be tested and a certain length of air. The long wire by-pass representing the conductor he calls L, and the spark through the air he calls B, and he assumes that the distance between the knobs at B measures the resistance offered by the by-pass or conductor. This appears at first sight to be reasonable, but it occurred to me that it would be well to analyse the direction of the B spark, in fact to ascertain whether the spark at B is a direct current of electricity passing from the outside coating of one jar to the outside coating of the other jar, or whether it be something else. The means I devised for this test was an arrangement of four balls affording two paths for the spark to select either of them, each path containing a small and a large ball. When this analyser is placed in the path without the by-pass, the selection, as I will show you, agrees with the terminals of the machine, but when the by-pass is inserted the other path is selected. (Experiment). I now place the analyser in circuit. The by-pass that I am using is about 70 feet of No. 12 gauge copper wire, wound round the legs of the table. The by-pass is now in circuit, and we will see which route the discharge takes.

The CHAIRMAN: Would it suit you, Mr. Wimshurst, to have the meeting adjourned, and have the apparatus ready next time?

Mr. J. WIMSHURST: I should be very glad to fall in with the suggestion, and will on the next occasion have the large machine ready to show the experiment. Mr. Wimshurst.

The CHAIRMAN: Before the meeting is adjourned, I would suggest to Professor Fitzgerald that, as he cannot be here next meeting, he will perhaps favour us with his remarks now.

Professor G. F. FITZGERALD: Mr. Chairman and gentlemen, I have been extremely interested in Mr. Wimshurst's experiments, which are more interesting than anything I can possibly tell you. Professor Fitzgerald.

The only points that struck me are that I do not at all attribute the importance that Mr. Preece does to the arguments which he brought forward against the oscillatory character of the lightning discharge. I think it is quite possible, notwithstanding all that has been said, that lightning discharge is oscillatory in the sense that a Leyden jar discharge is oscillatory, but it must be always recollected that the oscillation preponderates in a given direction, and is not always as much in one direction as the other; there is a preponderance in one direction, and there is a possibility of this preponderance causing a current and producing the effects which Mr. Preece has described. I do not think there is anything in the effects he has described to disprove the oscillatory character of the spark.

There are two points which, in my mind, should be brought out to make the discussion satisfactory. They are, first of all, to what extent are you by experience or experiment justified in assuming that a cloud acts like a good conductor? I do not think it can be proved, by anything we know of, that they do certainly act like good conductors. If there are any experiments which show that they do, I do not know of them, and I do not think they have been mentioned either by Professor Oliver Lodge or any one else; while there is a good deal of evidence to show that at least sideways and laterally they do not act like good conductors, because occasionally clouds lie in a valley, touching hills on each side, and discharging lightning from their centre, which they could hardly do if they acted like a metal plate: so that whether a cloud is a good conductor or no is a very interesting question. I think it is quite possible that it may act like a

Professor Fitzgerald. good conductor vertically when it is discharging rain. That seems to me to be a weak point in Professor Lodge's experiments illustrating lightning flashes. The weak point on the other side is that I do not think that any explanation that I have yet heard, even Professor Adams's explanation, fully accounts for such results as we heard described to-night with reference to the ship and the disruptive action on a conductor which is itself not sensibly heated. Professor Lodge similarly has called attention to several cases—for instance, whether the description of it is quite accurate or no it has not been disputed, that a horizontal bar of metal, totally unconnected with any other metal, gave out disagreeable sparks during a thunderstorm. That I do not think has been explained by any who have spoken on the other side, and I think it would require some such explanation as Professor Lodge has given in order to account for the injurious effects that have occurred. He says, "It has been investigated, and is reported to "have been due to a secondary charge in the horizontal "bar totally disconnected from anything." He goes on to say, "Although it did not probably receive a side flash, yet "it gave out sparks that ignited gas and caused a fire." Now, I think that the point that practical electricians ought to devote themselves to is to explain these curious and unexplained phenomena, and not to be stating that everything with reference to lightning is quite understood, for I do not think that everything with respect to a lightning flash is completely understood. I would not like, for instance, to repeat the experiment suggested by Mr. Preece, and would be very sorry to see him try it, of holding on to a lightning conductor when it was struck by lightning, for, although he might do it once or twice, I am very much afraid that occasionally he would suffer very severely. I would not like to do it, and I value Mr. Preece too much to wish to see him perform the experiment. However, I think there is very little doubt, as I said at the British Association meeting at Bath, that lightning protectors and lightning conductors have been and are of immense use in protecting mankind from the destructive effects of lightning.



The Chairman: Professor Oliver Lodge will, unfortunately, be

unable to be here at the adjourned discussion next week, and he tells me that he is very willing to reply now to the discussion so far as it has gone, and I am inclined to think that we shall all of us be very glad to hear what he has to say. Dr. Hopkinson.

The meeting signified approval.

Professor OLIVER LODGE: It is very kind indeed of the meeting, and of you, Sir, to allow me to reply now, but I will be as brief as possible. I cannot take up all the points, and perhaps I may unintentionally pass over some that are important. Professor Lodge.

I intended to have begun with some remarks complimentary to Mr. Preece and his wide experience, but you must consider all that as said.

As regards the difference between theory, experience, and observation; of course there is bad theory and good theory, just as there is good observation and bad observation, especially there is *unguided* observation. Now, I say that, by mere observation unguided by theory, it is very easy to overlook a great many facts which are plain enough in the light of theory. Unguided experience is by no means satisfactory, else would discoveries be much easier than they are.

Many of the effects I have shown—sparks in unexpected places, and other things—have been observed before. Henry observed things of the kind; and Edison noticed some curious phenomena, and said it was not electricity but "etheric force" that caused these sparks; and the matter was rather pooh-poohed. It was a small part of this very thing, only the time was not ripe; theoretical knowledge was not ready for it. Professor Carey Foster says that Professor Mascart, who was present at the last meeting, told him that M. Melsens had observed many of these things, but had not felt sure enough to publish them, because he did not understand them. There is nothing much in what I have done about the matter; it is the fundamental theories of your illustrious President and of Clerk-Maxwell that have enabled me to observe these things now so easily, when before they would have been almost impossible.*



* Mr. S. A. Varley reminds me, and I cordially accept the reminder, that the ease of these experiments is also largely due to the development of the modern influence machine.

Professor Lodge.

Mr. Preece made some remarks about case *b* which I did not follow, so I will defer commenting upon them.* He says there is no difference between us with respect to case *a*. But this difference between case *a* and case *b* which has been so much spoken of, partly because I insisted upon it so much in my paper, has only a very partial application, and refers to what shall be struck, whether point or knob; it refers merely to the questions of whether there shall be a flash at all, and what kind of terminal is most likely to be struck. *When the flash has once occurred there is no difference between case a and case b.* As regards the conduction of the discharge, they are the same; their differences appear only in the dielectric during the era preceding discharge. I am glad of the opportunity of making this explanation, because I have seen it stated that only under the circumstances of the impulsive rush is the discharge sudden, and that only then are these violent shocks and side-flashes (which I did not show to the full extent I might have done last time) likely to occur. But the fact is that with any flash there are these violent disturbances, no matter how it has arisen; only it is not so easy to get a flash to points in case *a* as in case *b*.

With regard to the experiment of Mr. Wimshurst, I must say that I had not noticed the fact he has shown, viz., that under certain conditions it makes a difference, even in case *b*, whether the top plate is positive or negative. In my own case it did not, because I was using larger jars and more violent rushes. But there is evidently a slight difference, perhaps a big difference under certain conditions, whether the cloud is positive or negative, which I had not previously noticed. The difference is obtrusive in case *a*, not in case *b*; but when I get back I will repeat the experiment, and see what the conditions are.†

* I have nothing much to say about them now, except that the diagram Mr. Preece gives is scarcely analogous to my case *b* arrangement. In that diagram the cloud is one coating, the earth another, and the tin plates are interposed in the dielectric of a common case *a*. Another cloud is necessary *to represent the outer coating of the jar* in my case *b*. It will then become just like the figures in the plate at the end of my paper.

† I find I was in error in saying I had not noticed this fact. On mentioning the matter to my assistant, Mr. Robinson, I found he knew all about it, and reminded me that we had noticed that the more gentle the rush, the more did

Professor Lodge.

As regards iron and copper, Mr. Preece says everybody knew that iron was best. Well, the Telegraph Department certainly use iron wire to protect their poles, but it has not been the practice in this country to erect iron on buildings; and most certainly the reason *why* iron is as good as copper was not known. Moreover, the two were always compared when the iron was seven times the sectional area of the copper. That is now seen to be all nonsense.

What Professor Adams says, that the statements of the Conference were fairly correct in the light of then existing knowledge, is perfectly true. I have not the slightest fault to find with any statements made in the past; I do find fault with the same statements being made now. In one sense self-induction is an old idea, as old as Faraday's "extra-current," but nevertheless it may be safely asserted that in 1881 no one, not Sir William Thomson, Lord Rayleigh, nor any one, knew self-induction so intimately as it is known now. No doubt some of the members of the Lightning Rod Conference had self-induction in their mind, but they did not know the full extent of its importance, and their knowledge at that time would have led them to condemn iron vigorously. Still more recently it would have been condemned in the light of the telephone experiments of Professor Hughes, because his oscillatory currents were not rapid enough to bring out the relative merits of iron and copper under lightning conditions.

Mr. Preece said that it was the great power of the Wimshurst machine that made all the violent disturbances last time. That machine has great power, but I have not used that machine except to show the experiments here. I observed them all with a small single-pair-of-plates Voss machine, 18 inches in diameter; and this produces all the effects just as well, only rather more slowly. It does not require a powerful machine, it takes little power.

Mr. Preece says that it is the Wimshurst machine that makes

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case *b* approximate to case *a*. It all depends on the time taken by the plate to charge, and the time consequently allowed for the path to be prepared by static induction. (See Appendix IV.)

Professor Lodge.

these sudden rushes, but that "in Nature everything is slow, and "stately, and quiet, and gentle,"—especially lightning!

He says he never doubted the oscillatory character of a Leyden jar discharge when there is self-induction present; but you cannot keep it out: there is self-induction in every circuit that either he or anybody else ever saw. You do not want *coils* to make self-induction: there is self-induction in a bit of wire as long as one's finger, and in the perforation of the glass of a Leyden jar, though that is certainly the most difficult case. I cannot admit that when oscillations are absent there is no self-induction.* Then, again, the bluing of paper and suchlike proves nothing against the oscillatory character of lightning. It is needless for me to dwell upon this after what Professor Fitzgerald has said. He has shown why it should be so theoretically, and I know well that it is so in fact with many oscillatory currents. You may have noticed a letter in *Nature* a week or two back, by Dr. Dragoumis, who was using vacuum tubes in my laboratory to detect Hertz-like oscillations: he used iodide of starch paper as well, and got blue marks all right. But as to oscillations, they are hardly essential to my case: it is $L\frac{dC}{dt}$ that I want big; and though the easiest way to get $\frac{dC}{dt}$ big is to have oscillations, they are not absolutely essential: as Professor Fitzgerald said at Bath. There is no real need, therefore, to discuss them.†

As to the case of lightning protectors, Mr. Preece says I have no experience of lightning protectors connected to telegraph instruments. It is perfectly true—I have not. It would be ridiculous for me to pretend to first hand information on such matters in this presence, but Mr. Preece has; and in a text-book on telegraphy by Preece and Sivewright (6th edition, 1887), it is

* The reason I originally imagined that Mr. Preece doubted the oscillatory character of a Leyden jar discharge, was because he said at Bath that he approached the question with some diffidence because it was supported by high authorities. It was, indeed! But no high authority had supported the idea of the oscillatory character of a lightning flash. However, it is manifest that many persons doubt the oscillatory character of a Leyden jar discharge: Mr. S. A. Varley, for instance—see current *Electrical Review* articles.

† This is barely true; oscillations are essential to the most violent effects.

recorded what trouble lightning gives them: how it damages the machines, depolarises the needles, and spoils the insulation; how protectors are of some use, but are not wholly reliable; so that whenever it is absolutely necessary to keep it out—especially from cables, where lightning might be very destructive—then they apply a great number of lightning protectors, in the hope that one or other will work. That is where I got my information from;* and also from direct experiment proving that no existing protector can eliminate Leyden jar sparks, as M. Guillemin and Professor Hughes had found out before. Mr. Preece says there is now in England only 1·3 per cent. of damage,—that means destruction, I suppose, and does not take into account partial depolarisation or slight damage to the insulation of wires, which must occur in many cases without immediately breaking the wires down. But why is there 1·3 per cent. of failure if protectors are already perfect? Lightning protectors can, however, be made absolutely perfect. They are much easier to tackle than lightning conductors attached to houses and large buildings.

Professor Lodge.

Professor Adams insists that the Lightning Rod Conference has done good work. Certainly; I admit it. So also Mr. Anderson's work on "Lightning Conductors," revised by Dr. Mann, is an excellent book. Of both works I have made the greatest use. I regret that I have seemed to be in antagonism to the Lightning Rod Conference; but, while they have done good work, that gives them no claim to infallibility. Neither do I wish to bind any of its members to the terms of the report.

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* The following are quotations from pages 257-9:—"Lightning is the "most fruitful source of fault upon telegraph circuits in those countries "where thunderstorms are rife, and atmospheric electricity is undoubtedly the "greatest enemy which those employed in their maintenance have to "encounter. . . . Earth currents are only troublesome at long intervals; but "lightning is a constant source of trouble, frequently breaking down the "circuits, and sometimes, in spite of all precautions, so damaging the instru-"ments as to render their removal necessary. . . . Although the dangers "liable to arise from lightning have been to a great extent surmounted of "recent years, still it cannot be said that a thoroughly efficient form of "lightning protector for telegraphic apparatus has yet been devised."—Preece & Sivewright's "Textbook of Telegraphy." Longmans, 1887.

Professor Lodge.

Doubtless many of that Conference felt qualms when they signed it. A document signed by a number of persons is always more or less of a compromise. As generally sound advice to builders and the public, it served its purpose; but if it is to be regarded as a dam across the stream of progress, a high-water mark beyond which the ocean of discovery may not flow, then the wonder is that in this age it has lived so long.

Professor Adams made one elementary slip, which I think he will see to be a slip, where, in attempting to explain the fact quoted by Mr. Farquharson, he stated that when a cloud and a roof discharge into one another the discharge confines itself to the air; so that once the charge has got thoroughly up into the roof then the discharge occurs wholly between cloud and roof, and does not come down the conductor at all! All that, if adhered to, is a perfectly new theory, entirely contradictory of what is known concerning the continuity of electric currents; and if existing facts require extraordinary theories to explain them, they are, in ordinary language, inexplicable.

I have taken up my ten minutes, and there are many points I have not touched upon, but I think I had better keep strictly to the time.

The CHAIRMAN: Mr. Wimshurst will continue the discussion at our next meeting, which will be held on May 16th, when, if time permits, a paper will be read by Mr. Mordey on "Alternating Current Working."

A ballot took place at which the following were elected:—

*Foreign Member:*

E. Mascart.

*Member:*

Henry Sherley Price.

*Associates:*

James Dawson. | Napier Prentice.

Edward Mark Robinson.



The meeting then adjourned.

The One Hundred and Ninety-fourth Ordinary General Meeting of the Institution was held at the Institution of Civil Engineers, 25, Great George Street, Westminster, on Thursday evening, May 16th, 1889—Sir WILLIAM THOMSON, F.R.S., President, in the Chair.

The minutes of the Ordinary General Meeting held on May 9th were read and approved.

The names of new candidates for election into the Institution were announced and ordered to be suspended.

The following transfer was announced as having been approved by the Council:—

From the class of Students to that of Associates—
Arthur Henry Lea.

The SECRETARY: I wish to remind members, some of whom are no doubt aware, that there are still copies of the "Lightning Rod Conference Report" to be obtained, either from Mr. G. J. Symons, Secretary to the Meteorological Society, who edited the Report, or on application to me. The published price was 7s. 6d., but members of the Institution are allowed to have copies at 5s.

The PRESIDENT: It has been found necessary to defer the reading of Mr. Mordey's paper on "Alternate Current Working," which was announced for this evening, because the discussion on the paper by Dr. Lodge, on "Lightning Conductors," was not closed at the end of last meeting. Under these circumstances, the Council have resolved upon holding an extra meeting on the 30th instant, so as to give ample time for discussion on Dr. Lodge's paper this evening.

I have now to call upon Mr. Wimshurst to resume the adjourned discussion on Dr. Lodge's paper.

Mr. J. WIMSHURST: Sir, I wish this evening to repeat the experiments of last week with the larger influence machine, to Mr. Wimshurst.

Mr. Wimshurst. better show that the impulsive rush does prefer the point to the dome under every condition; and, further, that under any condition but one it maintains a very decided preference for the point. In most cases it will leap more than twice the distance between the cloud and the dome. I will show that experiment at once, and it will be well, perhaps, to test the polarity of the machine, so that we may see which terminal is positive and which negative. [Experiment.] You see the length of the spark is about 13 in., with the balls arranged as at present. I will shift them, the larger for the smaller, so as to show the difference which results. You will observe that this is very great. It is of the first importance to know the polarity. I cannot with this changed arrangement get a longer spark than about 7 in. We now know the polarity of the machine, and I will leave it that way for the rest of the evening; the positive terminal is the left hand—the small ball; and the negative is at the right hand, or large ball.

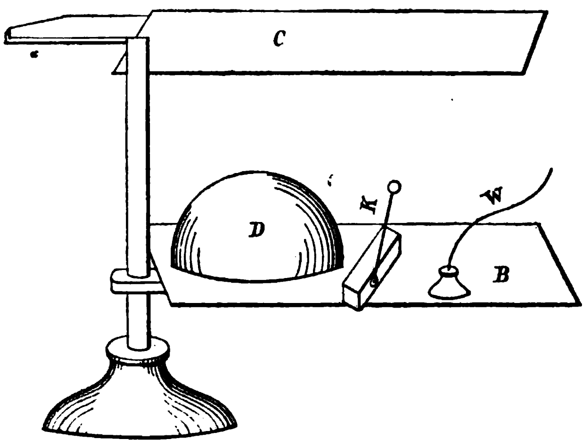



Fig. 1.

This cloud apparatus (Fig. 1) consists of the metal cloud, C; a board, B, covered with tin-foil; D is a metal dome; W is a piece of metal with a short length of fine binding wire which touches the tin-foil. The upper end I will press with a rod. The cloud, C, is now made positive, while the board, B, is made

negative. Thus arranged the dome, the knob, K, and the point are about equally struck—the point having a small advantage. [Experiment.] Mr. Wimshurst.

The PRESIDENT: The question was between the dome and this ball.

Mr. J. WIMSHURST: Just so, or the point; for there are three things.

The PRESIDENT: Oh, three things.

Mr. J. WIMSHURST: Shall I repeat the experiment again as I had it? I connect the cloud, C, to the chain from the outside of the jar, which is in connection with the large ball.

Mr. W. H. PREECE: Could you come to this corner, Mr. Wimshurst?

Professor J. PERRY: It is difficult to do so; these chains get in contact, it is much better to be close here.

Mr. J. WIMSHURST: I shall be quite ready to vary the experiment in any way you wish. I will now alter the connection and make the cloud negative. [Experiment.] So that there may be no misunderstanding I will use only the small knob, K, and the dome, D. [Experiment.] The small knob, K, is struck if about $2\frac{1}{4}$ times as far from the cloud, C, as is the dome, D.

Professor J. PERRY: The cloud is negative?

Mr. J. WIMSHURST: Yes; when the cloud is negative; you then get the small knob selected. This experiment removes all doubt about the point being supremely useful; at least, I think so.

Professor J. PERRY: At the same time, Dr. Lodge said he had never found any difference.

Mr. J. WIMSHURST: Last week when the band of my influence machine failed, from age, I was about to make some experiments upon the remaining three chief points in Dr. Lodge's paper. The first was that which he calls the enormous resistance of a large copper conductor; the second is the small importance which he attaches to a thoroughly good earth—his observation being that he might make it "equal to heaven" still it would not answer; and the third is the propriety of connecting, as he now recommends, all pieces of metal within a building together as in a ring, and

Mr. Wimshurst connecting that ring to earth. This latter seems to me to be fatal to safety; but I will show you some experiments bearing upon these points.

To test the first of them—the enormous resistance of the conductors. Let us repeat the experiment of the by-pass. Last week I explained the construction of the analyser (*b*) (Fig. 2). I

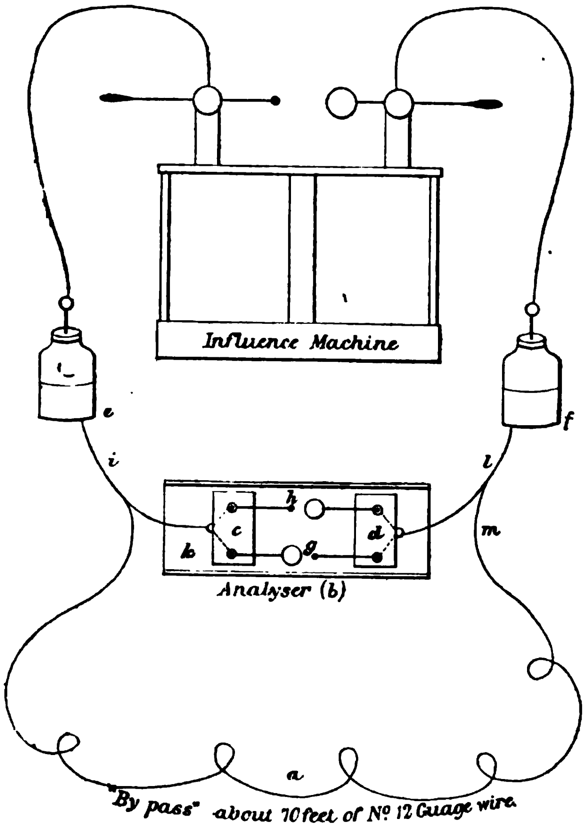



FIG. 2.

will hand it to the President that he may see. Here are blocks (*c*—*d*) of wood, each having two movable arms, one with a large ball and the other with a small ball at the end; the two arms are in metallic contact, arranged to afford two paths, so that when I bring the two blocks within striking distance the spark is

free to take either path. It is a rough arrangement, but it answers very well. I will connect this analyser (*b*) with the outside of the jars *e* and *f* and we see the result. The negative is on the outside coating of the left-hand jar (*e*), and the positive on the outside coating of the right-hand jar (*f*). [Experiment.] The positive selects the small ball, and the lower path (*g*) is used. This wire "by-pass" is about 70 feet in length. I have placed one end of it at this corner of the table; it then runs round the table legs and back again, the other end being at the other corner of the table. It is insulated by the wood of the table and is not wound in any special manner, but merely in a convenient manner for keeping it out of the way. I will connect the ends of the "by-pass," with the chains, to the analyser (*b*). The spark is now very much shorter than when the "by-pass" was not connected, and we find [experiment] the opposite path (*h*) is chosen. Mr. Wimshurst.

The PRESIDENT: Are we to understand that the balls were connected by the long wire?

Mr. J. WIMSHURST: The connection is this: the chain comes from the outside coating of the jar (*e*), loosely round to this (*k*) end of the analyser; the middle of the chain is thrown upon the (*i*) end of the wire conductor. The path for discharge is along the chain, through the wire "by-pass," and to the jar (*f*) through the other chain (*l*). The chains are loose, and are thrown over the ends (*i* and *m*) of the "by-pass;" the spark is free to pass silently by way of the "by-pass" or to leap the space between the balls of the analyser.

The PRESIDENT: What is the difference between this arrangement and the preceding arrangement, when the long spark was on the side next us?

Mr. J. WIMSHURST: At first the analyser only was in connection with the outside of the jars, the "by-pass" or conductor not being in use.

Professor D. E. HUGHES: I think you are trying to represent the A and B spark, are you not, which Professor Lodge spoke of in his last lecture?

Mr. J. WIMSHURST: Yes.

Mr. Wimshurst.

Mr. J. Wimshurst: He had previously spoken of the B spark as a measure of the resistance of the conductor, and in his paper he adopted the view taken in his former lectures at the Society of Arts and elsewhere.

The President: There is an alternative path in the previous experiment?

Mr. J. Wimshurst: In the first case I had simply the whole charge leaping from one ball to the other ball through the space ($g$), whereas now I have connected the alternative path of some 70 ft. of No. 12 gauge copper wire in circuit, and I find the spark selects the other path ($h$). I think that that is an indication that the charge from the coating of one jar passes silently through the "by-pass" to the coating of the other jar. The B spark is probably nothing more than the manifestation of an oscillation. I cannot ascribe it to anything else. Certainly it is not the measure of resistance; but the whole matter needs further investigation.

To show the importance of a thoroughly good earth connection, which lately has been so lightly spoken of, I will use the "by-pass" wire, which is of considerable length, and connect its two ends to the outside coatings of the two Leyden jars, for that arrangement supplies the best earth and allows the charge to pass through it. You will see there is very little tendency to side-flash; it is a nearly perfect state of things, and with such perfect earth it would be quite possible for a man to hold the inch lightning-rod, while the flash strikes, without injury, provided that he is insulated. Let us make the earth less perfect, by introducing some wet paper at the end of the wire—I have some in a glass here—and we shall find that the side-flash increases as the badness of the earth increases; do what we may, some side-flash will occur; for the unlike charges being suddenly released seek to equalise themselves and to avail themselves of every path which presents itself. It seems to me that this is quite consistent with the teachings of the earlier electricians. I would point out another characteristic of this side-flash—for every feature in it has to be considered—it is that the tendency to side-flash is greatest at the two ends of the wire, and there is hardly any tendency to side-

flash at the mid-length of the wire, which is almost neutral. At the one end the wire "by-pass" appears to be positive, it appears to side-flash, if I may say so, outwards, while at the other end it is negative, and it would appear to side-flash inwards. The ends of the "by-pass" are certainly oppositely Mr. Wimshurst.

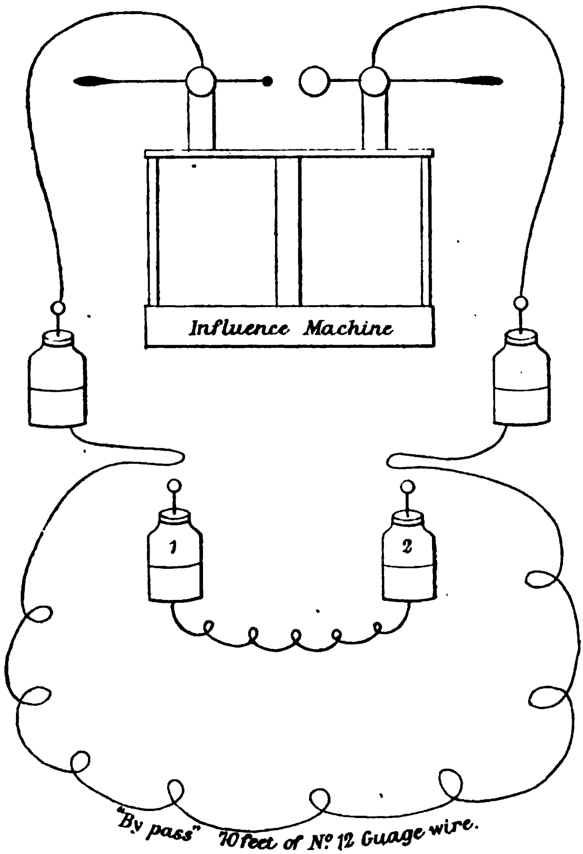



FIG. 3.

electrified. I will now place these Leyden jars (1 and 2, fig. 3) near to the conductor to be struck with the side-flashes; the one at one end of the "by-pass," the other at the other end, and allow several side-flashes to enter the jars; I will then test the jars to ascertain the charges they have obtained, when

Mr. Wimshurst. it will be found that they contain no charge whatever. This experiment appears to point to the fact that the side-flash is oscillatory in its character. I now put several of these side-flashes in, still you see the jars contain nothing. I have not tried them with an electroscope, but I have a pith ball, which you see indicates no charge; I then bring the jars together, we still see that there is no charge remaining, although we have seen many sparks enter. I will now put some loose pieces of metal together, and you see that the side-flashing materially increases as we connect the pieces together. I then connect the pieces of metal to earth, and the side-flash at once becomes violent; the electricity seeks as many paths as it can find, and a great part of the flash passes by this second path. I will now connect the "by-pass" and show this effect. [Experiment.] The whole length of the "by-pass" is in circuit between the two outside coatings of the jars. I hold the sheet of tin in my hand and test the side-flash at *this* end; then connect the two pieces together, and you will see that the side-flash increases, [experiment] for the larger the mass of material the larger must be the tendency to side-flash into it. If, on the contrary, the mass were small, say, a pin-head in size, it would not have capacity to receive a flash. Therefore, connecting pieces of metal together in a house is certainly fatal, and when they are connected to earth, which I will now show you, the enormous increase is manifest. I will now show you the increase in the side-flash with the bad earth. I take this piece of paper from the glass—it has been soaking all the evening; I place it between one end of the "by-pass" and the jar, the spark has to pass through about 5 inches length of thoroughly wet paper. [Experiment.] You saw that it materially increased the side-spark. That, I think, again is only what might be expected. If you put any resistance between the conductor and the earth the side-flash must be larger.

Professor W. E. Ayrton: Have you got your lightning conductor—that is to say, your wire—also connected with the earth?

Mr. J. Wimshurst: No, it is simply connected with the Leyden jars.

Mr. Wimshurst.

Professor W. E. Ayrton: Oh, but Professor Lodge said that your lightning conductor must be connected also with the earth.

Mr. J. Wimshurst: It is connected with the earth, but with an indifferent earth, the indifferent earth being five inches of soaked paper, and also this table. I will show you the experiment again, first without the paper and then with it.

The President: You are comparing a good earth and a bad earth?

Mr. J. Wimshurst: Yes; I will now put the wire in direct metallic connection with the bottoms of the Leyden jars.

The President: The lightning conductor is now in order?

Mr. J. Wimshurst: Yes, it is now perfect.

Professor W. E. Ayrton: When you put an earth, do you mean the thing to which the lightning conductor is attached, or the gas-pipe of this building?

Mr. J. Wimshurst: I speak of a perfect earth.

Professor W. E. Ayrton: Have you got it connected with *this* (the jar)?

Mr. J. Wimshurst: Yes; Professor Lodge himself says that that is a perfect earth. The arrangement now is that the charge from the one coating to the other circulates through the whole length of this wire, and again we get only what might have been expected, viz., less side flash.

With reference to the oscillations, I should suppose these to be proportional or about proportional to the length of the metal rod and the current circulating through it. If so, the tendency in actual practice cannot be very great; the lightning-rod is never of very great length. I have several times tried, by the use of long wires and a number of Leyden jars, to obtain some small indication of the oscillation on a sensitive-plate rotating with great velocity, but as yet my trials have been failures; I shall hope to do more in this direction. I now hand the photographs I have taken to you, sir, for you to distribute. There is no indication whatever of oscillation, as far as I can see.

With respect to the observations of Professor Lodge last week, that he used a Voss machine when making his experiments, and that any machine of that type would answer the purpose, for

Mr. Wimshurst. further investigation, I would state that, in making experiments with this high-tension electricity, it is of the first importance to know whether the charge is of a positive or negative kind. Professor Lodge, it would seem, has worked quite regardless of this knowledge; but I think all here will know, and must have noticed, the grave errors which any machines capable of readily changing their polarity are likely to entail on the user. It would be far better to fall back on the old-fashioned frictional machines when others of a more reliable character cannot be obtained.

If I may occupy your time for a few more minutes, I should like to show you an experiment with the spangled jar. It is not a very brilliant experiment, but it is the analogue to the sparking which we saw on the walls. I have here a large jar; there is no metallic connection with the inside of the jar, for I have simply a sheet of tinfoil rolled up and suspended upon the rod. Now when the charge and discharge passes, they induce an alteration of charge on the outside coating, and I get sparkling between the spangles. It is not so brilliant by any means as it would be if a direct charge were given to the inside coating. [Experiment shown with the gas turned down.] It is a charge given to one body, and a correspondingly induced charge upon the surrounding walls.

Professor J. Perry: Is there an inside coating?

Mr. J. Wimshurst: Yes, but it is quite insulated from the suspended tinfoil. The sparkling is not very brilliant, but it is enough to show.

The President: Will you tell us how the bottom of the outside coating is connected.

Mr. J. Wimshurst: It is connected to one pole of the machine, and the sheet of tinfoil to the other pole; but the sheet of tinfoil is insulated, the air space being quite sufficient for that purpose.

I would wish to add that I think I have shown that Professor Lodge has stated in his interesting paper views which probably he may reconsider, and which I have brought under your notice by these experiments. For instance, he states, "With reference to "the point, its protective virtue so much insisted on by the older

"electricians is entirely non-existent." I have shown, I think, that this is inaccurate. He then states that "the orthodox rule "of connecting all pieces of metal to the lightning conductor "requires modification;" and he suggests "connect all pieces of "metal to each other and to the earth, but not to the conductor." Here, again, the modification is certainly worse than the orthodox rule; and on many of his other suggestions grave questions may be raised. Mr. Wimshurst.

For these reasons, and as we value the safety of our buildings, it seems to me to be highly desirable that the subject should receive much more examination before the influence of this Institution is directed to the abolition of the valuable rules laid down by the older electricians sitting at the Lightning Rod Conference.

I may say that any part of this apparatus is at the service of any other speaker.

The President: I hope that as Professor Hughes was a member of the Ligtning Rod Conference he will let us have his views on the matters under discussion.

Professor D. E. Hughes: Dr. Lodge has brought before us a most interesting and remarkable paper, and it is a paper which invites criticism. Now, whilst I admire the beauty of the experiments which he has shown, I disagree with some of his statements and conclusions. I quite agree with the first portion of paragraph 3, where he says: "There are, therefore, two "main cases—(*a*) When the strain in the dielectric near the earth "has been of gradual growth, in which case the path of discharge will "be prepared inductively beforehand; (*b*) when the strain arises "so suddenly that there is no time for any pre-arranged path. "The first I call 'steady strain;' the second, 'impulsive rush.' It "is most important to recognise these two cases, and to understand "the extremely different conditions attending the two." But I cannot agree with the concluding portion of this paragraph, where he says: "The first case only was ever contemplated by the older "electricians; in fact, so far as I know, it was my experiments "last year which first called attention to the other case." Professor Hughes.



I do not think that Dr. Lodge was the first to call attention to

Professor Hughes.

the effects of an impulsive rush, and this statement hardly does justice to the labours of the older electricians, for ever since the discovery of the Leyden jar it has been employed for this object. Sir W. Snow Harris, in his work on "Lightning Conductors, 1830," in speaking about the Leyden jar, said: "The year 1752, which "marks an important era in electrical science, from the celebrated "discovery of the principle of accumulation just mentioned, gave "to the natural philosopher an easy method of concentrating "large quantities of electricity produced by artificial means, so as "to discharge it upon or through bodies with an instantaneous "and violent explosion."

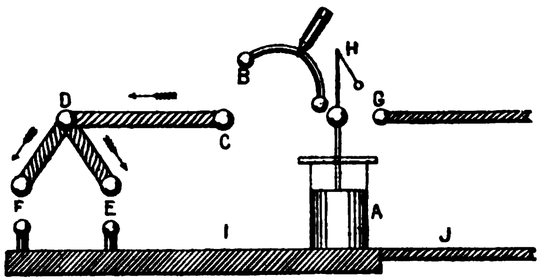
Thus he recognised the importance of studying the effects of an impulsive rush, the results of which led him to most important conclusions, both as to the best material and form of conductor for a lightning-rod—his conclusions being that copper tape had practical and theoretical advantages over rods of iron.

If subsequent experimenters failed to verify the conclusions of Harris, it was probably due to the employment by them of a too feeble electric charge where the impulsive rush was not sufficient to reproduce the effects that he had noticed. Professor Guillemin and myself not only verified and reproduced Harris' results in 1864, but, in addition, observed an extraordinary result due to an extremely high and sudden discharge through copper wires, and as the results were identical with those cited by Dr. Lodge in his chapter on "Lightning Protectors," and as I called his attention last year to these results, I do think that he ought to have mentioned our names and Report published in 1865, instead of (by his silence as regards our previous labours) allowing readers of this paper to infer that he was the first to observe these effects.

In the year 1864, Professor Guillemin and myself, as members of the Commission de Perfectionnement of the French Telegraph Administration, were charged with the mission of testing the comparative merits of the lightning protectors then used upon their lines for the protection of the telegraph instruments from lightning. These experiments were carried out at the laboratory of the Ecole de St. Cyr, of which Professor Guillemin was the Professeur de Physique.

Our idea was to employ an extremely powerful Leyden jar discharge, or spark, brought into action as suddenly as possible through an alternative path arrangement, one branch of which contained the electro-magnet, or a fine iron wire that we wished to protect, and the second branch as a shunt around the electro-magnet or fine iron wire, in which we could introduce the protector, whose duty was to allow a current of high tension, such as lightning, to choose this path and thus protect the electro-magnet of the telegraph instrument. Professor Hughes.

The general idea of our experimental arrangement was the following:—




In order to have a powerful source of electricity at high tension we made use of a large Leyden jar battery, A, of six large jars, having a total condensing surface of about 1 square metre, and as we found it took too long to charge these with the ordinary electrical machines, we used a powerful Rhumkorff coil giving a spark of 30 centimetres. The jars were charged to a fixed degree through the wires G and J, which were removed before discharging the jar. A fixed charge was given to the jars and measured by the electrometer, H, and could be discharged through the apparatus by means of the universal discharger, B. An insulated rod of brass, C D E F, served to conduct the charge to the experimental protector at E, and also to the electro-magnet or fine iron wire introduced at F to the return wire or plate I.

With this arrangement, and using our highest power, we at once found this curious result, viz., that we could not protect our electro-magnet or fine iron wire at F from being burnt, notwithstanding that we introduced the best known form of

Professor Hughes.

protector as a shunt at E. We then joined E metallically by a copper wire, and failing still to protect F we joined C direct to earth, or the return plate I, by a short thick copper wire of one centimètre diameter. Even in these conditions, where a short thick copper return wire was in advance of the electro-magnet, there would still be sufficient current pass through the shunted electro-magnet to entirely destroy it. Now, here we had evidently a more sudden charge or impulsive rush than occurs generally in practice. So there seems to be much truth in the remarks made by Mr. Preece and Professor Fitzgerald, that a discharge from a cloud is very different and much slower than that from a metallic condenser. On the other hand, there are no doubt sudden lightning discharges observed on our telegraph lines which not only equals but far surpasses anything we have yet seen in laboratory experiments.

Mr. Frank L. Pope, in a discussion upon my paper upon Self-induction, in 1886,* mentioned that it was a well-ascertained fact in the United States that their electro-magnets were often destroyed during lightning storms, notwithstanding that they had joined their wires direct to earth by a short wire in advance of the electro-magnet. Thus verifying in practice the results that we had experimentally obtained.

In order to test the comparative value of different protectors, we diminished the charge to a point where a marked difference in their behaviour was shown. The results were given in a report which I made as reporter, and published in the *Annales Telegraphique*, 1865 (tome viii., pp. 290 to 302), in which we recommended (in addition to the use of a shunt protector) the insertion of a short length of fine iron wire (*bobine preservatrice*) in the main line, the fusion of which would act as a cut-out (similar in function to those now used in electric lighting), and thus in all cases preserve the electro-magnet.

Having been called to Russia, I was unable to continue these experiments; but Professor Guillemin, having taken a great interest in them, he continued them, and published his results

* *Journal of the Society of Telegraph Engineers*, 1886, Vol. 15, p. 41.

in the *Comptes Rendus*, 1866. In this paper he demonstrated, by experiments, the advantage of a sheet conductor over that of a circular section, and thus verified the conclusion of Sir W. Snow Harris. And he also showed that Ohm's law for steady currents does not hold good for Leyden jar discharges during the variable period. Professor Hughes.

I believe that the effects observed by Sir W. Snow Harris, Professor Guillemin, myself, and, later, by Dr. Lodge, can be explained by self-induction; and it is curious how very nearly the earlier experimenters perceived this. Until lately I believed (through reading somes abstracts only of Harris's results) that he believed that the advantage of sheet-conductors was due to their greater electrostatic capacity. This is an error, for in his work on the "Nature of Thunderstorms," 1843, an abstract of which is given in the "Report of the Lightning-rod Conference," page 86, he said:—"The beneficial effect of superficial con-"ductors appears to depend on the removal of the electrical "particles further out of the sphere of each other's influence."

Professor Guillemin, 1866, said:—"This phenomenon arises "apparently from the inductive action of conductors upon each "other. Increasing their surface facilitates the discharge by "increasing the distance between mutually opposing forces."

In my paper upon Self-induction, 1886, I said:—"It is well "known that currents in separate portions of the same wire (as "in a coil) react upon each other, and I felt convinced from the "preceding experiment that self-induction is entirely due to "similar electro-magnetic reactions between contiguous portions "of the current in its own wire. Let us assume that an electric "current consists of a bundle or an almost infinite number of "parallel currents, the limit being a single line of consecutive "molecules; then each line of current should by its electro-"magnetic action react on each of the others similarly to wires "conveying separate portions of the current, and the self-"induction should be at its maximum when the lines are in the "closest possible proximity, as in a conductor of circular section, "and far less when separated, as in one of ribbon form."



Dr. Lodge says in his paper, paragraph *F*, page 412:—"The

Professor Hughes.

"electrical disturbance is conveyed to a conductor through the "ether or space surrounding it; expressed more simply—"lightning currents make use of the periphery of a conductor "only, and so the more surface it exposes the better. Better "than a single rod or tape is a number of separate lengths of "wire, each thick enough not to be easily melted, and well "separated, so as not to interfere with each other by mutual "induction."

I do not quite agree with Dr. Lodge's theoretical view, but I judge from the above that he advocates a tape form in preference to a single conductor of circular section; he does not, however, attribute much importance to this, as he says farther on, paragraph 56:—"The shape of cross-section is but little "matter. Flat ribbon has a slight advantage over round rod, "but not enough to override questions of convenience."

This does not fully agree with the views of Sir W. Snow Harris, Professor Guillemin, nor myself—the first two having demonstrated with Leyden jar discharges a very great advantage in the use of flat ribbon, and I demonstrated the same in 1886 for voltaic currents—the difference in copper being 50 per cent. in favour of the flat ribbon form; and in iron still greater, for here a flat strip of iron was 1,000 per cent. superior to a rod of the same ohmic resistance, but of circular section.

As our time is limited I will not enter into the question of copper *versus* iron.

The PRESIDENT: You have experimented so very much upon the impulsive current in copper and in iron conductors of various shapes, that I should much like to hear your remarks upon that part of the paper.

Professor D. E. HUGHES: As regards the choice of metal, it is a question of relative cost and convenience, for I regard both iron and copper as very suitable, provided we diminish the enormous self-induction of an iron rod by employing iron in the form of tape, or, better still, in numerous separated smaller wires whose total ohmic resistance is no greater than that of a solid rod.

Leaving aside the question of relative cost and convenience, I do not agree with Dr. Lodge where he says (paragraph 55) "iron

has advantages over every other metal." Nor can I agree with the theory of Oliver Heaviside, if it leads to the conclusion that iron for telephone wires is better than copper. Professor Hughes.

The PRESIDENT: He was very guarded in the statement, and said that an arrangement of iron was suggested which might be an improvement, but the ordinary iron wire was recognised as being very inferior in practical conductivity to copper wire in regard to magnetisation.

Professor D. E. HUGHES: I am very glad to hear this, for I believed that Oliver Heaviside's theory agreed with Dr. Lodge's statement as to the advantages of iron.

Franklin recommended iron as having a higher fusing point. I believe, however, that this is not important, for any lightning charge that is sufficient to fuse copper would be dangerously near the fusing point of iron; besides, iron when fused by a sudden electric current does so with explosive force, sending off pieces which burn with violence, thus increasing the danger from fire. With copper it is different, and Dr. Lodge himself quotes an example from the report of the Lightning-rod Conference (p. 195), where a small copper bell-wire protected a house in Monte Video, the flag-staff of which was shattered, the copper wire melted without any further harm.

Dr. Lodge has made some experiments, quoted in his lecture to the Society of Arts, in which he seemed to demonstrate that iron (when tested by his peculiar method) was better than copper. Now, as these results were opposed to all our previous experiments, we had to examine closely the conditions of the experiments. Many who have examined and repeated these experiments find that the conditions of the experiments are not those which occur in a lightning discharge.

I pointed out to Dr. Lodge that I did not believe that the B spark (upon which he depended for his proof) was simultaneous with that of the A spark. Mr. Acheson, in the United States, made a series of brilliant experiments on this subject, in which he proved that the B spark was not simultaneous with that of A, and that the B spark was due to an extra current. Mr. Preece has ably criticised Dr. Lodge's method, and proves that the charge

Professor Hughes.

of the Leyden jars were not the same in different trials. Mr. Wimshurst has shown this evening, in his very beautiful experiments, that the direction of the current influences the result. Therefore I do not admit that Dr. Lodge's experiments on this subject prove anything, except that by a very complicated arrangement of Leyden jars and circuits you can prove that iron is better than copper, and also a comparatively bad conductor delaying a current is better than a good one that allows the charge to be at once conducted to earth.

I have tried to obtain some direct proof of the superiority of copper with Leyden jar discharges, as I have already proved by the voltaic current; but as yet I have no method, except some equally open to objection as that of Dr. Lodge's; but I have found one result which seems to me to be hopeful.

We know that the comparative high self-induction in iron is due to circular magnetism, so I took a soft iron rod, which was previously free from circular magnetism, and discharged a Leyden jar through it, the rod lying horizontal east and west. After the discharge it was again tested and found to have strong traces of remaining circular magnetism; so I think this a hopeful proof that there is sufficient time during a discharge to produce circular magnetism in an iron wire.

Perhaps it may be of some interest for me to describe briefly my method of testing for circular magnetism, as it is extremely difficult to find if an iron wire has circular magnetism or not, as it shows no evidence of external magnetism, and our magnetometers fail to indicate the magnetic condition of a piece of iron in which there is a perfect closed circuit of internal magnetism. But if we seek for the Wiedemann effect, by putting the apparently neutral wire under elastic torsion, we may, if the circular magnetism is sufficiently strong, observe the effect on an ordinary magnetometer. For feeble effects I am obliged to use an apparatus invented by myself, and described in my paper to the Royal Society,* in which an insulated coil of wire surrounds the wire to be tested, the coil being connected with a telephone and

* *Proc. Roy. Soc.*, 1881, p. 526; 1883, p. 6.

rheotome. When elastic torsion is applied to the wire, induced currents are produced in the coil, which we can plainly hear in the telephone; the effect, however, does not last very long, as repeated torsions destroy the circular magnetism. The effect can be rendered continuous by passing a weak voltaic current through the wire, in which case we hear the induced current, produced by the reaction of the circular magnetism produced by the passage of the voltaic current. Professor Hughes.

If we look to the practical results of long-distance telephony and telegraphy, we find universal testimony in favour of copper in place of iron. They have proved in the United States that they are enabled to work the telephone at a distance of 1,000 miles through copper wire, when the same results cannot be obtained through more than 200 miles of iron wire. They find the same difference all over the Continent. Sir W. Thomson, in his Presidential Address this year, cites a letter from Mr. Bennet also proving the same fact. As regards telegraphy, Mr. W. H. Preece conclusively proved, in his remarkable paper,* that he could more than double the speed of the Wheatstone instrument by the use of copper wire.

Taking all these facts into consideration, I should certainly prefer copper, not only for a telegraph and telephone line but also as the best material for the conduction of lightning discharges.

If I had to choose a lightning conductor for my own house and wished to feel perfectly safe, independent of any theoretical views, I should feel that by the choice of a copper tape in place of a rod there could not be any theoretical or practical objection to its use; and as regards choice between copper and iron, as iron can never have less self-induction than copper, I should feel perfectly safe in choosing a copper tape in place of an iron rod, this being my ideal perfect lightning conductor.

Mr. G. J. SYMONS, F.R.S.: I do not rise, Sir, to speak on the electrical matters in this paper, as I should be hopelessly out of place in doing so, but simply as the—amanuensis, shall I put it, Mr. Symons

* British Association, September, 1885.

Mr. Symons. of the Conference which sat some ten years ago upon this question of lightning-rods; and the remarks I have to make are rather historical and critical than electrical. I hope, however, that you will bear with me a little, because I think that Dr. Lodge has not realised what I was going to express as the terrible mischief which he may be doing. That is a strong expression to use, but at the same time the amount invested in lightning conductors throughout the world must amount to several millions sterling; every public building, whether it is the Houses of Parliament, Westminster Abbey, or whatever it may be, is protected by lightning conductors, and we are told that all these lightning conductors are wrong in principle and are dangerous. That is a very alarming thing as regards both life and property, and I venture to think that a statement of that sort should not be allowed to pass without very severe criticism. Of course if there can be an improvement of any kind in such an essential and important apparatus, it would be wrong for any one to stand up against it, but on the one hand we have the records of the behaviour of conductors during more than a century; on the other hand, laboratory experiments.

I personally feel very doubtful whether laboratory experiments sufficiently represent real lightning; but, be that as it may, I am not going to say anything whatever about them, not being competent to do so.

I will now refer *seriatim* to some points in Dr. Lodge's paper.

In the early part of his paper he refers to the system which as carried out at the Hotel de Ville, Brussels, and speaks of that in these terms: "I may say, however, that I have seen no occasion "since to importantly modify any of them, and that my position "in respect to these practical hints is stronger now than it was then, "inasmuch as I find that the large experience of the Belgian "electricians, who have given more attention to the subject than "perhaps any other school, has led them to recommend, in recent "years, almost precisely the same methods of protection as those I "advocated; their views being acquired almost entirely by practical "experience, and scarcely at all by theory." In the first place there was, I believe, only one man (and he has unfortunately passed away) in Belgium, of the Belgian electricians who held these views

—that was M. Melsens, who was consulted with reference to the Town Hall at Brussels, and who put up the most wonderful bird-cage of conductors that has ever been put up. Dr. Lodge tells us that that building has been damaged by lightning! That does not say very much for the system which Dr. Lodge himself says is very nearly carrying out his own views. Mr. Symons.

Then with reference to the terminals. I am very much surprised to hear this question as between knobs, points, and balls coming up again, because it was all thrashed out about 120 years ago. There was a large powder magazine down at Purfleet which was damaged by lightning, and a committee of the Royal Society was appointed to investigate what had been done and why it happened, and they examined Priestley, Hindley, and a number of the first electricians of that day upon this very question of knobs and points. Of course, as is clearly stated in the Lightning-rod Report, the point is supposed to discharge two duties—and that is the reason why the Conference recommended a blunt terminal surrounded by a number of sharp points. The blunt terminal was intended to receive a disruptive shock, if such a thing should happen to fall upon the conductor, and the sharp points were supposed to dissipate the electricity from the earth and so reduce the potential in the vicinity of the stroke. But surely, with respect to a cloud thousands of yards in area, the difference between a point, say $\frac{1}{100}$ of an inch in area, and a ball a foot in area, vanishes.

Professor Lodge refers in the next place to protection with respect to chimneys, and he had a gas-flame to represent a a chimney. There is no doubt whatever that a hot-air current is a very excellent conductor, and for that reason the Lightning-rod Conference have always held (I am not quite sure whether it was mentioned in the Report) that we should look more to the protection of the kitchen chimney than of any other chimney in a house, because the kitchen chimney is the only one in summer which has a fire at the bottom of it and has any hot air coming from it. But when he speaks of the Continental plan of a cross-bar over the top of a factory chimney, with a spike rising up from that, being by far the best way of protection, I do not

Mr. Symons. agree with him, because I have myself seen on the Continent a considerable number of conductors of that sort which have got out of order in consequence of the hot fumes passing corroding the iron-work—they generally use iron—and of course the whole thing tumbles to pieces. What the Lightning-rod Conference recommended seems to me, with due respect to Professor Lodge, a preferable system; and they recommended the system proposed long ago by Faraday, that there should be a flat ring on the top of the chimney, entirely surrounding it, but, further, that that ring should be furnished with a number of points that should do the silent discharge business, while if a disruptive discharge came it would find the ring itself and have plenty of material upon which to act.

With respect to the question of size, I was rather sorry to hear what Mr. Preece said. I know that Mr. Preece's authority is infinitely beyond mine, and that he has always held strong views as to very small wires being sufficient for carrying off a flash of lightning. I think that possibly that is because the bulk of his experience has been with wires running down and protecting telegraph posts. I do not know at all, I speak quite subject to correction, but it seems to me as possible that, in the presence of the very large mass of wires carried by these posts, disruptive charges, if they fall upon posts, are considerably diminished in intensity. Certainly the Conference, when they were considering this subject, found that rods of iron, and platinum even, of considerable dimensions were melted. The one case which I quote is that of the New York packet struck by lightning in the Gulf Stream in April, 1827. I am now reading from Snow Harris's book,* and it says—"In this case the discharge "fell on a painted iron rod four feet long and half an inch in "diameter; some few inches of the rod near its point were melted; "the linked iron chain which descended from this rod to the water, "about a quarter of an inch in diameter, was knocked in pieces by "the expansive force of the shock, and some of the links fused. "The flash of lightning not only melted some of the links, but

* Snow Harris, "On the Nature of Thunderstorms," 1843, p. 109.

"caused them to burn like a taper. The melted iron fell in glow- Mr. Symons.
"ing drops on the deck. The results of a great natural experiment
"are here presented to us, and we see that an iron rod of half an
"inch in diameter effectually resisted a flash which fused a chain
"of about one half of its dimensions." In the Lightning-rod Conference Report, page 223, several instances are given, but perhaps the most striking of all was that at Carcassone, in the south of France, in which the conductor, seven-tenths of an inch in diameter, was fused. As such cases happen, I think that a No. 5 B.W.G. wire, which I think is what Professor Lodge recommends, would very quickly go out of the way on being struck, and then you would have the difficulty that the building would be left entirely without protection.

With respect to iron and copper, I think that those who will have the goodness to peruse our Report will see that we were by no means absolute as to the preference for copper. The Conference did prefer copper, and they gave their reasons for so doing, but they also gave iron—and of course iron is very largely used, especially in France, where they go in very freely for conductors with much longer upper terminals than are used in this country.

Then we come to the question of a copper rod, and whether a man may or may not grasp it with impunity, which Mr. Preece said he would do. I was delighted to hear Mr. Wimshurst say that he thought that there would be no danger; that is my own opinion, but, of course, as I said before, my opinion on a purely electrical matter is worth nothing.

At the end of the paper Dr. Lodge quotes a number of instances from our Report, but I confess I do not quite know why, because it is not obvious that they contradict any of the recommendations that we have made. Of course there is the question of protected area; that is a very doubtful subject, but still, there is the fact that hundreds of instances have been collected together of strokes falling near conductors, but we have never yet found a case in which any damage has been done within the protected area as defined by us, and, therefore, if that "protected area" be not absolutely true it seems to me that there is very strong

Mr. Symons. evidence in its favour. At the bottom of slip 13, Dr. Lodge quotes the instance of a church being slightly injured on May 18th, 1877, at Garding, in Germany. But it appears that they did not follow our rules; they took the conductor down the *north* side of the steeple, not down the *wet* side as we advised.

Again, on the same slip and under No. 43, Dr. Lodge raises a question on a statement by Professor Fitzgerald, at Bath. He says: "*Illustrating a matter important, if true, suggested by* "*Professor Fitzgerald, theoretically, at Bath, not yet verified by* "*me.* In two cases the stroke broke the conductor at points "where its direction was abruptly changed."

That is one of our rules—never bend a conductor at a sharp angle (*see* Report, p. 18). The above statement is an illustration of the result of not obeying that rule.

Then with respect to the brush discharges from the conductor and surging circuits in the neighbourhood, he says: "Two "electrical phenomena are to be noted as sometimes occurring "when a lightning-rod is struck . . . a peculiar noise is heard "like pouring water on a fire, and electric sparks are emitted from "bodies near."

That is a quotation from a paper by Duprez, but I think it would have been equally fair to the subject to have quoted two or three other words which are in the same abstract (Report, p. 96), viz., that M. Duprez "considers a properly constructed "lightning-rod to be a perfect safeguard." As one half of the story was quoted, the other half may as well be added.

I now come to the last slip, and there we have a series of what Dr. Lodge calls practical questions. They are from No. 51 onwards, and I have gone carefully through them as compared with our Report, and the result is that nearly all those rules—I suppose they might almost be called—are in our Report, and are in accordance with it, therefore I do not quite understand what is the use of reprinting them.

In conclusion, I hope that in some way or other this great Institution and the very distinguished men who are here present will express something in the way of a distinct opinion, so that it shall not go forth that all that has been done

up to the present time, in every country throughout the globe, has been wrong. It is certainly hard for the new system that at the only large building to which it has been applied—the Town Hall at Brussels—it has failed. Mr. Symons.

Lieut.-Col. R. Y. ARMSTRONG, R.E.: It will be readily understood that the War Office takes a keen interest in discussions on the efficiency and economy of lightning conductors, and, as I have had to do with the matter in question for many years, I wish to state a few facts connected with my experience which may or may not be held to be pertinent to the discussion, but which I will state as briefly as possible. Lieut.-Col. Armstrong.

The Fortification Branch of the War Office have mainly depended for the protection of ordinary magazines on (1) continuous conductors, without joints as far as possible; when joints are necessary they are of very low resistance to low voltaic currents; (2) very* low resistance earth; (3) sufficiency of points; (4) and other minor details which are in accordance almost entirely with the recommendations of the Lightning-rod Conference.

Whatever might have been supposed, from Professor Lodge's earlier lectures, to be his views on (1) and (2), he has stated in the lecture now under discussion that he considers the measures in question to be valuable, and he has even gone so far as to admit that an ordinary galvanometer may be usefully employed in testing the work. It will be satisfactory to some of us to know that we are supported by the weight of his opinion on these points. It may be interesting to the meeting, in connection with the facts I am about to lay before them, if I state that I think the War Department are probably the only body in this country, and possibly the only people in the world, who really do employ the method of testing the efficiency of their lightning conductors by voltaic currents and galvanometers periodically.

Without wishing for a moment to argue that absolute protection is guaranteed under the foregoing conditions by the precautions we adopt, I have to state that whether safety has been attained by low-resistance earth, jointless conductors, or by

* That is, a few ohms.

Lieut.-Col. Armstrong. other precautions taken, or merely by luck, to the best of my belief, in no case where the above principles have been carried out has a conductor failed. On the other hand, I know of cases where conductors, defective when judged by the foregoing standard, *have* failed.

At Slough Fort, on the Thames, about eight years ago a small tower, to which a conductor was attached, was struck and cracked by lightning. How the portion of the discharge which struck the tower got to earth was not clear. The conductor was not injured. Its earth resistance was about 200 ohms, otherwise it was in accord with the rules of the Lightning-rod Conference.

Again, about five years ago the contents of a small civil magazine or shifting-room in the Lake District were exploded by lightning. The earth of the conductor had been dug up before I had the opportunity of having it tested. The resistance of a similar earth on an adjacent magazine was, however, found to be between 100 and 200 ohms, and the point of the conductor of the magazine where the accident occurred did not test good continuity with low volts.

At the Chichester Cathedral the earth resistance was over 100 ohms, and part of the lightning left it for another earth of between 100 and 200 ohms in connection with a large metallic surface. In another case which I examined, where lightning went down the chimney of a dwelling-house, the earth resistance of an adjacent conductor was over 100 ohms.

No doubt many members of the Institution will be acquainted with cases where lightning conductors have been burnt up by lightning at riveted joints. Such a case occurred to a conductor on the citadel at Malta, about seven years ago, but the lightning did no other damage in this case.

Another instance of this sort occurred more recently, also abroad, the conductor being fused BELOW the point at which part of the current left it through a staple in the wall.

The portion of the current which left the conductor in this case ignited some small-arm cartridges, and these cartridges were in *metal cases*, so that apparently the current, or electrical action, was not entirely on the "differential outer skin."

I would further observe that, in the cases given by Professor Lodge, no instance occurs of failure of good earth. The Gardner Church Conductor, which he quotes as damaged in 1887, was fractured in 15 places. This does not, therefore, appear to be a case of failure of earth, but of inefficiency of conductor, probably owing to defective joints, which would have been revealed by testing. The statement, moreover, that the earth was faultless, is vague. Lieut.-Col. Armstrong.

I would wish to submit, with all deference, that although Professor Lodge's experiments are magnificent, they are not lightning; and that we must be very cautious in drawing definite conclusions concerning lightning discharges from laboratory experiments or mathematical calculations until further accurate experiments have been made with the real thing. It would be a big jump to attempt to say, with any confidence, that lightning coming from a cloud, square miles in area, through hundreds of feet of air, with energy capable of destroying substantial structures, will follow, even approximately, the laws governing discharges between glass jars connected to a few feet of tin, and points or flames a few inches off, the dimensions of the flames being considerable in comparison with their distance from the sheet tin.

In olden times the experiments were made with *real* lightning. No doubt some of the experimenters were expended; but there are plenty of electricians who could deal with lightning and take care of themselves at the same time.

Mr. A. J. S. Adams: The present occasion is opportune for submitting some features relative to an apparent effect of stress in conductors. We know that a current in passing along a conductor has to contend with the resistance proper to the metals and to its self-induction; but it would seem that such a current has also a property of self-relief by a radiation of energy. The idea of self-relief is roughly drawn from the fact that increased energy in the conductor results in increased radiation. Observe the simple proposition that a conductor A under momentary stress, such as that produced by an induction coil, exerts an influence upon an adjacent conductor B. Now replace B, by a Mr. Adams.

Mr. Adams. microphone, and we obtain from it a similar indication of A's effect, together with the suggestion that in this case, at least, a portion of the energy radiated by A presents the form of mechanical force.

We remove the microphonic circuit and replace it with a single telephone held to the ear; the same feature of intercepted energy is apparent, and the fact that the telephone coil is open, again indicates that the energy intercepted is one of mechanical motion. These instrumental aids are not necessary, however, for the radiated energy may be made perceptible to the unaided ear.

It will be understood that the lines of force from a circular conductor, such as A, ever widen out, and, for a given space, lose in effect; but that by using a conductor having a flat surface the force lines will be rendered parallel, with corresponding accentuation of effect. I attach a piece of flat metal to one end of the secondary wire of this induction coil, and the flat surface, acting as a kind of electric lens, focusses the force into parallel lines, and the tone of the coil-vibrator becomes audibly reproduced upon its surface. You will also observe that by placing this piece of mica upon the metal plate, and then laying the ear close to the mica, the sound becomes full and crisp. [Handing the metal disc to the President.]

The PRESIDENT: I can hear it very well with this mica.

Mr. A. J. S. ADAMS: Yes, I prefer mica between the ear and the metal to prevent chance of sparking. We may arrange a

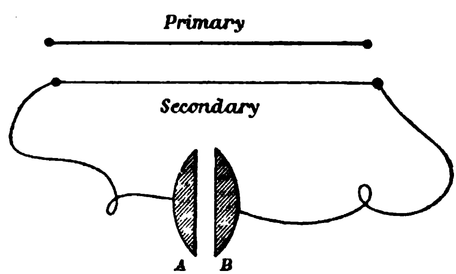



FIG. 1.

lens of still greater power by having two surfaces, as shown in Fig. 1, where the plates A and B are shown in section. I have

here two such plates, between which I insert from one to half-a-dozen pieces of ¼-inch plate glass. The result is that, whilst the sound reproduced upon the inner surfaces of A and B is clear and audible to all around me when only one slip of glass is used, the effect decreases as the remainder of the glass slips are inserted, widening, as they do, the distance between A and B. Mr. Adams.

This mechanical effect of *sound* was brought under the notice of this Society, and an explanation offered, by myself in a short paper read in December, 1877.

I regret that time does not now permit me to enter more fully into the subject generally, in order that I might demonstrate how that, if two or more pieces of plate glass be inserted between the metal surfaces A and B, Fig. 2 (where, although the spaces are shown large, they should in practice be mere films by the pressure of all the surfaces one upon the other), whilst sound waves are radiated from the surfaces of A and B; *light*, similar to that produced in ordinary vacuua, is emitted between the glass plates S S.

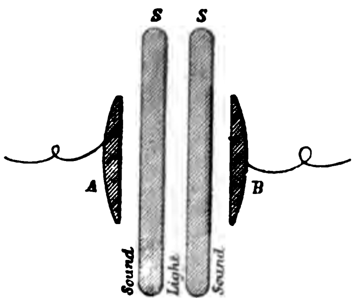



Fig. 2.

And also how that, by unequally magnetising the atmosphere upon the one side of a looped or loosely-hung conductor as against the other side, the conductor itself may be caused to bodily vibrate with each pulsation of force. What I do wish now to suggest is the far-reaching effect of the invisible radiation referred to, and the apparent focussing power of flat surfaces—of a probably highly dangerous character in the case of lightning.

In order to illustrate this, and as pointing to an explanation

Mr. Adams. of the incident mentioned in Professor Lodge's paper anent the isolated iron bar sparking and causing combustion, I quote the following experiment. In it the lens A is connected to one end

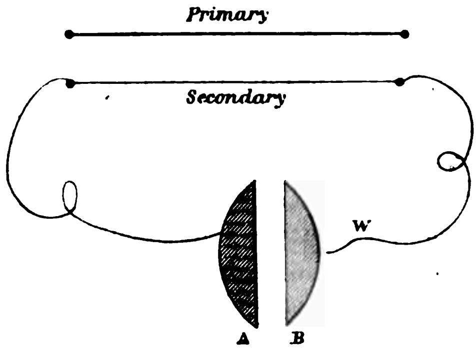



FIG. 3.

of the secondary coil. B is placed facing A, but is isolated. Nevertheless, the energy radiated from A upon B will spark off from B to the wire W, or to the earth—a result possessing serious significance in the case of lightning.

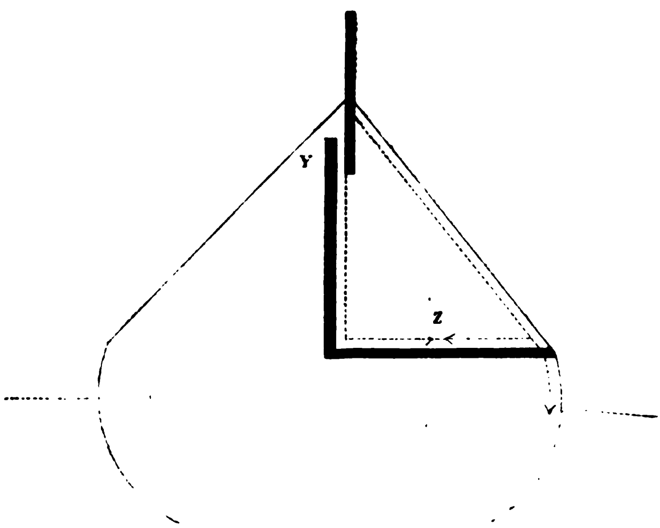



FIG. 4

So also this radiative aspect of the question seems to offer an explanation of the marine incident referred to at our last meeting, as at Fig. 4. Here we have the metalled topmast insulated from

the lower parts by a coat of paint, at Y. The topmast is struck, Mr. Adams. and, during the fierce radiation from surface to surface at Y, the flash itself will have traversed the shrouds to the vessel's side, where one part will have gone overboard and another will have fallen back upon the comparatively free metal Z, at which point a great concussion of the radiated and the conducted forces would seem to have occurred.

It is not improbable that this radiation of energy will be strongly and dangerously developed in the flash itself during its passage through the air, and afterwards in the conductor under stress, and this feature may perhaps go far to explain many of the effects at present mainly accounted for by side-flashes and surgings.

Another point, as giving emphasis to the possible effects of fierce electrical radiation, suggests itself in connection with so-called "lightning photography." Here is an instance. "A seaman was sitting at the foot of a mast to which had been nailed a horse-shoe. The man was killed by a flash which struck the mast, and upon his body was found a mark, apparently produced by the flash, corresponding with the horse-shoe.

The PRESIDENT: Was it knocked off the mast?

Mr. A. J. S. ADAMS: I understand not. The incident is mentioned in one of the Journals of this Institution, and I refer to it because it seems to emphasise this idea of radiated energy, and because the facts I have demonstrated in respect of radiation and focussing would appear to render such an occurrence at least a possibility; and as to the general question of lightning marks, which have borne resemblance to some adjacent body, I venture to think that the recorded instances are too numerous and circumstantial for the subject to be easily brushed aside as an assured absurdity.*

I have already referred to the phenomenon of light emission in the air film lying between the glass slides at Fig. 2, a feature I have not seen noticed elsewhere, although it is one not a little remarkable from the fact that the periphery of the air film is



* *Nature*, May 6th, 1875.

Mr. Adams. open to the surrounding atmosphere. Now, what we can do artificially is surely within the capacity of Nature, and hence it seems to me that we have in this phenomenon the approach to an explanation of globular lightning, in that whilst the clouds and earth may constitute the inciting discs, and the atmosphere the insulating plates, a patch of comparatively rarified air may become the nucleus of such an electric cloud or globe.

In respect to this feature of radiation, there is a marked difference in the radiative capacity of some metals, as, for example, in the case of samples of No. 11 hand copper, hand brass, and hand-drawn iron. I find that, *size for size* and *stress for stress*, iron shows 25 per cent. greater radiative capacity than copper, and brass slightly more than iron. So that, supposing the idea of self-relief to possess any value, copper would appear to be, size for size, the most suitable of the three for lightning conduction.

Mr. Spagnoletti. Mr. C. E. Spagnoletti: I might mention a case or two bearing on this subject. Having to maintain very many thousands of miles of telegraph wires, I have a good opportunity of learning the effects of lightning upon them. The wires are struck very frequently, but are seldom fused. The instruments attached to them, on the other hand, suffer to some extent. An instance occurred in North Wales, where the wire attached to a bell was struck and the coil of the bell was split like a cross, in four, and the whole of the wooden case was lined like a wire brush, by the small pieces of wire, showing that, although the flash must have been a very strong one, the line-wire was not at all affected, which was a galvanised wire of No. 8 or 11 gauge. These line-wires are capable of taking large currents. Another case occurred at Shrewsbury, where a man was up among the wires on a pole one afternoon and was struck by lightning severely. The lightning struck him, and apparently entered his body underneath one arm and passed out of his leg. I inquired why a man of his experience should have been up among the wires during a thunderstorm, and the reply was that there was no storm at Shrewsbury, it being a mild and calm evening, but I learned that there was a heavy thunderstorm at Hereford at the time, and that the current, which was ultimately the cause

the man's death, was carried from Hereford to Shrewsbury, a distance of 50 miles, by the line-wire—No. 8 galvanised iron. On another occasion, at Reading, a No. 4 gauge galvanised iron wire was fused (and that is the only case where I have known of a No. 4 wire having been fused), and the No. 16 copper wires, covered gutta percha, attached to it were melted, and the gutta percha ran over the ends and sealed them. Mr. Spagnoletti.

I should like to ask whether a flash of lightning has been proved to be oscillatory. From what Professor Hughes stated to-night it does not appear to be so. Mr. Preece gave several examples, one showing that the action of a flash upon a polarised relay was a continuous line on the printing instrument, therefore it does not look as if it were oscillatory. I have several times tried with hand magneto machines to test the currents they give with a galvanometer, but could get scarcely any motion of the needle; and the faster I turned the less was the motion. Therefore, I think, if lightning is oscillatory the polarised relay would have either caused a dotted line, or, if the vibrations were so rapid as to prevent the tongue not touching the contact pins, no mark would have been made.

The PRESIDENT: The time is rather advanced, but I am sure we would all sit till midnight rather than lose some of the statements of fact that we have just heard; and I would invite any gentleman present who can contribute some facts that would help to throw light on this subject to do so. Sir William Thomson.

Mr. SYDNEY EVERSHED: I came here this evening intending to make some rather deprecatory remarks on the futile nature of the discussion at the last meeting, but Mr. Wimshurst's experiments have put those remarks aside, because I feel certain we must have all learned a good deal from the beautiful experiments showing that side sparks from a conductor will not charge a Leyden jar. Mr. Evershed.

The first thing that I noted for remark was a most important question which has been left entirely untouched by every speaker, namely, do B sparks, which occur in laboratory experiments, occur in nature? and is there any evidence of such sparks striking lightning conductors?

Mr. Evershed.

Mr. Preece occupied some time at the last meeting in trying to prove that there are no such cases, but I do not think he brought any very good argument to bear on the point. During 1877 I carried out a great many observations on atmospheric electricity by means of a quadrant electrometer, using a gold-leaf electroscope when the electrometer proved too sensitive; and, with your permission, I will occupy the few remaining minutes in describing what seems to me an important observation that bears on this question of B sparks. One of my earliest observations was with a gold-leaf electroscope during a thunderstorm taking place some miles away from where I was. I was on the side of a hill (marked B in the sketch), and a thunderstorm was taking


place some five miles away at the other end of a long cloud, which was apparently continuous between my point of observation and the thunderstorm at A. About six feet from the ground I held a gold-leaf electroscope, and, before a discharge took place at A, the leaves would be from $\frac{1}{8}$ to $\frac{1}{4}$ inch divergent; the instant a spark took place the leaves would jump out three or four times their former divergence. Now there you have a case of sudden increase of potential which, if great enough, will cause a B spark. This was a very striking observation, and one which I have repeated since in similar cases, but never with such a great length of cloud. Afterwards, when I got my quadrant electrometer to work, I was often troubled by thunderclouds causing the light spot to disappear very suddenly from the scale.

A great point made by Mr. Preece at the last meeting was as to the oscillatory character of lightning, and he produced a record to convince us that lightning in telegraph instruments could not be oscillatory. Professor Lodge, in his paper, does not say that it

is so; he describes the conditions for an oscillatory discharge, but, so far as one can see, these conditions do not obtain in a telegraph instrument, and, even if they did, we should get these records on the Bain or any other telegraph instrument, because it would not be an oscillation of equal currents in opposite directions, but one in which each impulse is less than the last—in other words, a gradual dying away. This being so, the experiments described by Mr. Preece have no bearing on the question at issue. Mr. Evershed.

Mr. Wimshurst's experiments this evening with the ball and point, or small ball and large ball, remind me of an interesting result obtained by H. Yeates, in 1876. He had a large induction coil connected with two small Leyden jars, each jar having a pair of terminals, a point and a ball, the ball of one jar being opposite the point of the other. The path chosen by the discharges from the jars then depended on the direction of the primary current, and the moment this was reversed the sparks would change from one pair of terminals to the other.

That is a more striking illustration than the one Mr. Wimshurst showed us, because points are used instead of small balls. Of course, it does not bear so much on the question raised by Mr. Wimshurst, where the difference was whether the *cloud* was positive or negative.

Before I sit down I should like to thank Dr. Lodge, on behalf of the younger members of this Institution, for his work during the last two or three years. In these days, when we are inundated with papers and books in which a modicum of fact is buried under a mountain of mathematics, his admirable way of putting ideas into our heads is very refreshing.

Mr. W. H. PREECE: Before you close this discussion, Sir, perhaps you will allow me to submit, for the inspection of the members, three or four examples indicating fusion by lightning. There is one in particular—it happened only on May 11th last, near Leeds—which gives unmistakable evidence of fusion in the interior of the copper wire. The outer section has evidently been cooler, and the hot metal inside has burst itself through the skin. There are also two or three specimens of iron wire and gutta percha-covered copper wire that give much the same indication. Mr. Preece.

Mr. Preece. I should like just to correct Mr. Evershed, who referred to my liking for ancient history, and to inform him that the cloud experiments which he made only twelve years ago were also made by Lord Mahon in 1789—one hundred years ago!

Sir William Thomson. The PRESIDENT: I think we must now consider the discussion as closed. I am quite sure we all agree that Dr. Lodge has done exceedingly good service in having raised the question in the manner in which he has raised it, and in having brought into the discussion of the theory and practice of lightning conductors some very important scientific principles that had not been fully taken into account by those who preceded him in the subject. I think we all admit that the principle of self-induction had not been sufficiently taken into account in connection with the theory of lightning conductors and practical rules for safety in their use. I do not know whether Franklin had any consciousness whatever that there was such a question as the mutual influence of currents in neighbouring conductors, or in different parts of one conductor, in respect to the facility afforded for carrying away the electricity by the conductors. It is quite clear that Snow Harris *had* some correct views on the subject: we must not accept all his views of electricity as correct; but many of us must now feel that in some respects in which we thought him wrong—in which, forty years ago, I, among many others, thought him wrong—he was quite right. There is one thing in Dr. Lodge's summary of results (article 56) that I confess I cannot understand at all: "the shape "of the cross-section is not of much importance." This seems to be altogether at variance with his own teaching on the subject. Snow Harris thought a great deal of surface and shape of cross-section. In speaking on the subject at the British Association at Bath, I referred to the lightning conductor set up fifty years ago on the tower of the old Glasgow University buildings, under the recommendation of Snow Harris. It was a large *tube* of copper, and I well remember being taught to consider that that had been a mistake, and that the same quantity of copper in a solid rod or a wire rope would have been cheaper and just equally effective. We then thought Snow Harris wrong, and I believe that Faraday himself did not perceive that Snow Harris was right in that

matter. We now know that he *was* right, and that spreading the copper over a wide area is even better than rolling it up the same breadth in the form of a tube. A sheet of copper, we now know, constitutes a conductive path for the discharge from a lightning stroke much less impeded by self-induction than the same quantity of copper in a more condensed form, whether tubular or solid. Sir William Thomson.

Now, as to the "practical questions" put forth by Dr. Lodge, I think there are some valuable suggestions. No. 72 seems to me important: "The cheapest way of protecting an ordinary "house is to run common galvanised iron telegraph wire up all "the corners, along all the ridges and eaves, and over all the "chimneys, taking them down to the earth in several places, "and at each place burying a load of coke." The burying of the load of coke is the heaviest part of the business, but the multiplying the mains by connecting a large number of comparatively small wires instead of one close conductor does seem to me an important practical suggestion. On the other hand, he says it is no use connecting them to water pipes. That I cannot agree with at all. On the contrary, I would take these galvanised iron wires described by Dr. Lodge, and the more of them the better, down all the corners and wherever you can get them, and connect every one of them to a water pipe. I would far rather do that than to a load of coke, it is more easily done; and I think that that is the best way of doing it for the protection of an ordinary dwelling-house having water supplied to it in many-branched metal pipes. An ordinary house can, I believe, be made exceedingly safe by the water pipes.

"Connecting a lead roof or other such expanse with a light-"ning conductor is not an unmixed good, for it virtually increases "the dangerous proximity of the lightning conductor." Well, I would say connect all pieces of metal to each other, and to the earth if you can, but if you cannot connect each of them to an earth, connect them to the lightning conductor, and give *it* a good earth. I think, on the whole, that the spark coming from a lightning conductor is not one of the main sources of danger, although there is no doubt that Dr. Lodge is perfectly right in saying that

Sir William Thomson. there is a liability that it may light gas or other combustible substance. There is no doubt whatever but that the more completely the house can be caged in the better; and for powder magazines I believe that it is perfectly true what Dr. Lodge says (and what I have said myself), that the way to make a powder magazine perfectly safe is to completely enclose it in iron. Make a complete iron house of a powder magazine: line the floors with wood or soft material to prevent ignition of stray powder by persons walking on the floor; but let a powder magazine be an iron building with an iron floor and then you do not need an earth. The powder should be kept well in, far enough from iron walls, floor, and roof, that no etheric spark can ignite it. Whether on a granite rock or in a swamp it would be equally safe: the need for the earth is absolutely done away with if the magazine is completely enclosed by metal. In that case I suppose iron would be the best metal, although it would be rash to say, seeing how very difficult is the subject of the impulsive current in iron. Remembering Professor Hughes' experiments and illustrations, and the mathematical theory worked out so magnificently by Heaviside, we are not allowed to overlook the impedance due to the magnetisation of the iron itself under the influence of a sudden current. I may be wrong in this, but my impression is that this very impedance would help to make the interior of an iron shell freer from electric disturbance than it would be with a mass of equal conductivity of copper, or other metal having equal conductivity.

The subject is so tremendously interesting that I do hope this is only the beginning of it, and that we shall have a great deal more of it. Colonel Armstrong spoke of the ignition of ammunition completely encased in metal. I hope he will experiment in that direction. The metal was not soldered all round I presume.

Lieut.-Col. ARMSTRONG: I think it was. The ammunition is made damp proof, and therefore the case must be complete all round.

The PRESIDENT: I hope that Colonel Armstrong will be able to take up the matter experimentally as a scientific question; to see, for instance, if thin steel instead of copper would make any difference. Besides that, I think that on a larger scale something

Sir William Thomson.

should be done. We all know how Faraday made himself a cage, six feet in diameter, hung it up in mid-air in the theatre of the Royal Institution, went into it, and, as he said, lived in it and made experiments. It was a cage with tinfoil hanging all round it; it was not a complete metallic enclosing shell. Faraday had a powerful machine working in the neighbourhood, giving all varieties of gradual working up and discharges by "impulsive rush;" and whether it was a sudden discharge of ordinary insulated conductors, or of Leyden jars in the neighbourhood outside the cage, or electrification and discharge of the cage itself, he saw no effects on his most delicate gold leaf electroscopes in the interior. His attention was not directed to look for Hertz sparks, or probably he might have found them in the interior. Edison seems to have noticed something of the kind in what he called the etheric force. His name "etheric" may, thirteen years ago, have seemed to many people absurd. But now we are all beginning to call these inductive phenomena "etheric."

I cannot sit down without expressing in the name of the Institution our most cordial thanks to Dr. Lodge for having taken all the trouble he took to bring this subject before us, with the beautiful experiments he has shown, and for having stimulated so many minds, whether to defend or oppose his views. I am sure you must all feel grateful to Mr. Wimshurst also for the potent assistance he gave to Dr. Lodge to prove his case, and for the potent application of his splendid apparatus this evening to further illustrate and to criticise some parts of Dr. Lodge's case. The discussion has been sometimes warm, and has been carried on with a considerable degree of humour; but I am perfectly sure that we all feel exceedingly obliged, not only to Dr. Lodge, but to all who have spoken on the subject, whether they have attacked Dr. Lodge wholly, or agreed with him wholly or in part. He has pointed out some flaws in the Lightning Rod Conference Report but I do think that this book continues practically to hold the field, by its practical rules and recommendations for the rendering of buildings and telegraphic apparatus safe against lightning. We may admit the validity of some, or perhaps even of all his criticisms of the orthodox dogma. We must admire the vigour

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of his attack; and in the brilliancy of his own exposition we cannot but see much that is instructive and suggestive.

But, after all, the conclusions adopted by the Lightning Rod Conference do afford us very strong reason to feel that there is a very comfortable degree of security, if not of absolute safety, given to us by lightning conductors made according to the present and ORTHODOX rules as actually laid down in this book. I am quite sure that the authors of this book will be exceedingly glad to modify their views in any practical way whatever, when cause is shown and proof given that such modification will improve the practical result.

Major Cardew.

Major P. CARDEW, R.E. [*communicated*]: This paper and the discussion on it recalls to mind the old fable of the wars of the Gods and Titans. There has been plenty of lightning playing about too to help the analogy, while there was certainly a Jove-like serenity and dignity of utterance about my friend Mr. Preece.

I do not think it necessary to declare on one side or the other, but should like to make a few general observations after the manner of the Greek chorus. I will follow the numbering of the paper itself.

2.—This can only happen, if at all, when rain is falling heavily from the intermediate cloud. A good deal of the stored-up energy must be released in the primary flash. The thoroughly wet surface of any object struck under these conditions will tend at least to protect it. I should not therefore expect these secondary discharges to be very dangerous, although they may produce startling effects. Professor Hughes did not appear to have quite grasped the meaning of the "impulsive rush" discharge in his remarks.

4.—The essential difference between a tinplate and a cloud has been already pointed out. I will come to this again later.

5.—This is very instructive.

6.— . . . ("The sparking distance under these circumstances "is surprisingly great by reason of the impetus with which the "electricity rushes into the top plate.") . . . I cannot understand this, unless Dr. Lodge believes that electricity is material.

8.—Here, again, the conditions of the experiment are widely

different from those of what it is intended to represent. The flame issues absolutely from a metallic pipe in metallic connection with a metallic representation of earth. The case of the real chimney is much more nearly that of 9 with an imperfect conductor (the soot deposit) in circuit, which is consolatory. Major Cardew.

10, &c.—All these experiments were very beautiful, and certainly the effects produced closely resembled those often noticeable in the centre of a storm at sea, with heavy rain, especially the side and multiple flashes and the violet-coloured discharges.

16, &c.—It appears to me that Dr. Lodge utterly fails to prove the oscillatory character of a lightning flash. The weakest point in his theory, to my mind, is that he considers the path, in air, taken by the discharge to have all the properties of a metallic conductor, including a determinate resistance per unit length.

Now I do not think that much real knowledge has been attained on the subject of the process known as a disruptive discharge, and the molecular movements and changes by which it is propagated.

Taking the dissipation of energy in heat, which is visibly there, Is this due to resistance of the same kind as in a metallic conductor? Is it constant; or, is it not widely different the instant before, the instant of, and the instant after the flash? Is there not also, probably, a conversion of energy chemically or by electrolysis?

We have very good reason for thinking that the counter E.M.F. of the voltaic arc is something considerable compared with the E.M.F. producing it. We have also very good reason for thinking that powerful chemical effects are produced in the atmosphere by lightning.

Then as regards the self-induction, how can we estimate this? We know that, when constrained in a metallic wire, the discharge will so distribute itself as to reduce this—will try to minimise P, the impedance, by decrease of $p$ L, even at the cost of increase of R. How much more freely can it do this where there is no constraint, where one line of particles is practically as good as another over, perhaps, yards of section?

Major Cardew.

25, &c.—Iron *versus* copper.—Now Dr. Lodge is very dogmatic about this. His meaning is, apparently, that iron certainly offers more impedance to rapidly varying currents than copper, this could not, I take it, be disputed; but that this total impedance is made up differently—the true resistance factor being of more importance in the case of iron. The reasoning is hardly conclusive I think, but the conclusion is altogether wrong I feel sure. The proper function of a lightning conductor is not to exhaust the superfluous energy of a discharge, but to ensure that that energy shall be all expended inocuously in the air, and the less the total impedance, and especially the resistance, of the conductor the better.

At the same time I think that, under practical conditions, iron of a moderate size—say No. 2 gauge wire—is quite good enough.

28. I doubt this statement. Of course it would be true if the discharge occupied the whole cross section, but it does not; it makes $\sqrt{p \, L^2 + R^2}$ a minimum by traversing the outer annuli alone, thus increasing R, at any rate until it is comparable to $p$ L. Obviously by this selection of route the value of R for a rapid discharge may be indefinitely great, even in the case of a thick copper rod. I do not understand the limitation in brackets [not many miles]; $p$ L varies rather more rapidly than directly as the length, while R varies directly as the length.

29. This is mere assertion. I do not believe a lightning current ever exceeds a few hundred amperes; but I do not assert this.

30. We all know there are millions of volts; the question is, how produced?

33. *Lightning Protectors.*—Certainly the experiments described by Professor Hughes, and those indicated by Dr. Lodge, demonstrate clearly that there are conditions of discharge under which any ordinary protector would fail. On the other hand, the very extended experience of our own Postal Telegraphs clearly points to the practical value of lightning protectors. The most logical conclusion would appear to be, not that the statistics of many years are misleading, but that the conditions are not the same.

Everyone knows that protectors are not perfect, and would hail any real improvement; but the improvement must be proved by experience, and not by reasoning based on false premises. Major Cardew.

39. *A.*—The impedance offered by a conductor to a rapidly varying current is no doubt greater than to a steady current, but this does not prove that it amounts to hundreds or thousands of ohms in a properly constructed conductor conveying a lightning discharge, or that it is more than a mere trifle compared with that offered in the air-path of the flash, where, I consider, the energy is and should be expended.

As regards the statements quoted from the Lightning-rod Conference. The first statement does not, I think, imply that the man could not be hurt, but that he might not be; and as nothing is premised as to what he is to stand on, I should not say it was untrue, though it is possibly injudicious.

I am not prepared to defend the second statement, as gunpowder is an imperfect conductor; but it seems to me that some interesting experiments might be made in this direction. As a rule, the discharge of a Leyden jar will not ignite, but will scatter, gunpowder; but it can be made to ignite it.

*B.*—This statement is worded rather loosely. It is not quite apparent whether it is the jar or the charge on the jar that is likened to a spring. I do not see what is gained by the comparison. No one probably would deny that the discharge of a Leyden jar under certain conditions is vibratory, but many people would deny that the vibrations are analogous to those of a spring.

*C.*—This statement begs the whole question. If whenever the spark ceases to be oscillatory it degenerates of necessity into a fizz or rapid leak, then it follows that every proper flash is oscillatory. But I dispute the premise.

*D.*—This also I deny. We don't want much common or uncommon resistance in a lightning conductor; and we don't get it in a properly constructed one.

*E.*—I admit there is something in this; but I contend that pieces of metal near the conductor are much better connected to it.

Major Cardew.

*F.*—The first statement is revolutionary, and seems to be applicable to space of four dimensions. The explanatory statement is contradictory to former statements in this paper. If true, what becomes of the reasons given for preferring iron to copper. Certainly there are reasons for dividing up the conductor. There are also practical reasons, and I think more weighty ones, against it.

*G.*—I leave this to Mr. Wimshurst. Nothing appears to be suggested as an improvement on a set of points.

*H, I.*—These statements are simply unsettling. There is certainly some truth in them. There is also truth in the statement that a good lightning conductor is a safeguard, although perhaps not an absolute one.

I now come to the practical outcome of all this as given by Dr. Lodge; and after all this trumpeting forth of a new creed—these terrible fulminations against the prophets of the old superstition—What is the new revelation? It is apparently contained in a number of statements constituting, it is to be presumed, a guide to complete protection. Of these, the majority bear a suspicious resemblance to the tenets of the old superstition, so much so that one is tempted to believe that, after all, our present powder magazines, &c., may escape destruction from the coming case *b;* but in some, what Mr. Preece might call, the cloven hoof is disclosed. It is impossible for me to discuss them all, but I will take 75 as perhaps the most dangerous. I cannot understand how Dr. Lodge can reconcile this statement even with his own theories.

Would not a well-earthed piece of metal in proximity to an elevated portion of the lightning conductor, but not attached to it, be likely to give rise to a spark across, establishing a derived circuit, and thus perhaps causing the fusion of the conductor at the point of derivation, and great danger of fire? One instance at least of this happening is within my knowledge. The conductor of Chichester Cathedral passed within a few inches of a lead flat roof without being connected to it. The roof was connected to earth by the downpipes. The conductor was struck and the flash leapt across this space, the result being that 50 or

60 feet of the conductor was fused from this point up, while every downpipe was broken across at one joint or another, where the discharge encountered local resistance. Major Cardew.

Now a few words upon Mr. Preece's observations. I do not think he quite takes in Dr. Lodge's meaning in talking of the oscillatory character of lightning. However many oscillations there may be there must be a preponderance of transfer in the true direction in order to discharge the quantity. This will account for the fact of its producing chemical traces and magnetism.

The whole energy which is expended must be $\frac{1}{2}$ Q V, where Q is the quantity stored up and V the potential difference, and however it oscillates it cannot do more than this amount of work. Even if lightning be unidirectional, there must be one great wave of current, giving full play to the impedance caused by self-induction, although this impedánce will not amount to anything like what Dr. Lodge considers it to be, since the rate of variation is not nearly so rapid.

The evidence of currents in telegraph lines originating in a lightning discharge is not valid against the oscillatory character of lightning itself. They traverse long lengths of thin wire with considerable resistance, self-induction, and mutual induction with other wires, and I should say would in any case be unidirectional and comparatively long drawn out. But I fully sympathise with Mr. Preece in his defence of the older electricians. How does it befit us, to whom they have left so splendid a heritage, to render them back mocking? Superstition! Well, if you come to that, what are all our beliefs and theories but superstitions erected upon old observations and reasonings?

What do we know of the agency by which gravitation acts—how the sun holds the planets, as it were, in the hollow of his hand? But what we do know of it is not simply a brilliant theory evolved by Newton from his inner consciousness and the fall of an apple, but the result of centuries of observation and observed accordance of events with calculated prediction. Thus we know its laws although ignorant of its real nature. But in the case of lightning, we know very little indeed. Who can tell of the

Major Cardew.

generation of the thundercloud, or the meaning of the rush of wind to it? Why, the lower animals seem to have more insight into it than we have.

Mr. Preece spoke of the solemnity of the operations of nature, and was, I believe, laughed at for applying this epithet to lightning. I adopt his meaning entirely. What can be more solemn than a thunderstorm, especially at sea. "They that go down to "the sea in ships, that do business in great waters; these see "the works of the Lord, and His wonders in the deep." And is all this to be brought down to a tinplate cloud and a storm in a tea cup? By all means let us experiment and reason on our small experience, but let us avoid dogmatising, at any rate until nature has amply justified us.

We are, I trust, all grateful to Dr. Lodge for stirring up the question, and for his able exposition of his original views; and originality, even when in error, helps forward the attainment of truth. And without attempting to decide whether he is right or wrong in his conclusions, let me recapitulate the directions in which I think his experiments might be brought more nearly in accordance with nature and where his theory appears weak:—

*First.*—The cloud should certainly not be metallic. The great difference between a metallic and a partial conductor is well shown in Dr. Lodge's own experiments. We cannot make a real cloud; but at least we can get nearer to it with a wet sponge or handkerchief, or some such device.

*Second.*—Earth should always be real. The mere completion of the circuit in the way we attribute to earth often leads to fallacious results, especially with high tensions and small quantities. It is easily seen that the addition to a portion of a circuit of an infinite capacity is likely to modify results—still it is often overlooked. One side of the generator and everything representing a conductor or protector should be thoroughly connected to good earth.

*Third.*—The assumption that the air-path traversed by a disruptive discharge offers the same kinds of impedance as a solid conductor, and those only, cannot, I think, be justified.

*Fourth.*—The doctrine that a lightning conductor is perfect

which offers a considerable impedance to lightning, and is the receptacle for a very appreciable portion of the total energy of the flash, is, I think, pernicious, and the true thing to aim at is a conductor offering a minimum of any kind of impedance, and especially of resistance proper. This being of course fulfilled when the impedance of the conductor is negligible compared with that of the air-path—a condition probably obtained with a conductor erected and earthed in accordance with the recommendations of the Lightning Rod Conference. Major Cardew.

There is one remark of Dr. Lodge's I find I have overlooked, and that is 84, where he talks of an earth resistance of a thousandth of an ohm or less. Has Dr. Lodge ever tested a real lightning conductor?

I fear these remarks have been tedious, and I am fully conscious of my inability to throw any light on the subject, but, as it is one of deep and abiding importance and interest to the human race, I hope the intention of this protest against hasty conclusions may atone for its feebleness.

Lieut.-Col. J. T. BUCKNILL, late R.E. [*communicated*]: This is the most interesting and suggestive paper I have ever read on the subject of lightning and lightning-rods. Lieut.-Col. Bucknill.

Section 5 is very important, as it seems to indicate that "the violence of the spark is lessened" by an increase of ohmic resistance in the conductor, but that the conductor gathers the stroke as effectively, so far as striking or sparking distance is concerned, as with a much lower ohmic resistance.

That present practice gives conductors a much higher conductivity than is absolutely necessary has been held before. Thus, Mr. R. S. Brough, in a communication to the Asiatic Society of Bengal, February, 1877, gave scientific reasons in harmony with the more convincing arguments and mathematics now published by Professor Lodge; and I myself suggested in 1881, to a War Office authority, that a large telegraph wire will always carry off a stroke of lightning inocuously.

The cloud to cloud, or condensers in series action, has been ably examined by the lecturer, but the possibility of subterraneous condensers in series acting similarly is not suggested, and this

Lieut.-Col. Bucknill.

appears to me to be a more probable explanation of the phenomena noted in the Tanfield Moor Colliery than the one given on p. 237 Lightning Rod Conference, and adopted by the lecturer (see section 46).

The coal strata separated by strata of very low conducting power and connected by the galleries, shafts, and winding-gear, and tramways of the mine, would spark from one to the other through these imperfect connections.

And this leads to a very important matter, which I think has not received sufficient attention from Dr. Lodge, viz., the *position* of the main induced terrestrial charge. Where water and gas pipes exist, I believe that they become highly charged by induction before the flash, and that the flash follows the route of minimum impedance that exists between the charged cloud and the earth system of conductors which is inductively charged. It is therefore useless to provide "a good earth independent of the "water and gas pipes," as proposed in section 59; on the contrary, it would evidently be preferable to connect the highest portions of the water and gas supply pipes to the conductors, and thus get to the induced charge by the *shortest* route.

I am convinced that this word *shortest* is one that should never be lost sight of in lightning-rod practice. For similar reasons I would add the words, *but where they cannot be avoided they should be connected*, to section 63, as *disruptive* is far more dangerous than *conductive* discharge; and I am utterly sceptical as to a flash melting even a small gas-pipe, or igniting the gas except by disruptive discharge. Hence, large cast-iron gas-pipes with oakum packing at the sockets are more dangerous conductors than small gas-pipes with threaded connections.

I should like to ask the Professor how he would deal with the great mass of metal now frequently stored in magazines—(*a*) by metal powder cases, which have replaced powder barrels; (*b*) by live shell in the expense magazines.

Would he connect them? I say No.

With reference to section 65. There are notable exceptions—tall rods being absolutely necessary over powder mills, petroleum oil wells, etc.

Mr. LEONARD JOSEPH [*communicated*]: Availing myself of the President's permission to send in writing the few remarks which time did not permit of my making at the meeting, I beg to observe, firstly—as regards Dr. Lodge's analogy of the Leyden jar—one objection against his theory, not already mentioned, is that his dielectric *air* is the reservoir, so to speak, of electricity, whereas glass is not. Next, as regards oscillations: Their character, as was shown by one speaker, was something like Fig. 1. This is easily to be accounted for. Let me give you an illustration. Let Fig. 2 (*a*) represent Mr. Joseph.

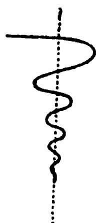

FIG. 1.

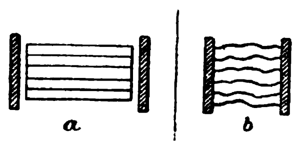


FIG. 2.

several pieces of linen piled on top of each other, and laying between two upright boards. Compress the boards, and the linen sheets will take up a wavy form similar to that shown in *b*.

Now, instead of boards for our lightning, we have the pressure of discharge on the one side and the earth's resistance on the other. The result is a number of waves all along the course taken. The reason the waves get smaller as they near the earth is that the earth's attraction draws the current in a straight line. Energy of discharge, distance from cloud to earth and earth's resistance at that point, being known, it then becomes comparatively easy for a mathematician (which I am not) to calculate the size of these waves or oscillations at any point.

Now for the practical side of the question. Iron *v.* copper has been fought out by abler heads than mine, so I shall content myself with a summary of what seems to me to be the advantages of the latter. (1) Copper is a better conductor; (2) iron is easily affected by weather, and corrodes even when

Mr. Joseph. galvanised; (3) magnetic defects in iron; (4) it is true that the conductivity of heat in copper is greater than in iron, but continual contraction and expansion affect iron—as a conductor of electricity—sooner than copper, as the latter, being more elastic, does not so soon lose the original construction of its particles.

As to shape, I propose tubing, *not rod*, for several reasons. (1) Tubing gives the metal a greater cooling surface; (2) a greater conducting surface; (3) a lesser chance of the metal being destroyed by expansion and contraction, as more "play-room" is given.

Mr. Symons spoke of the advantages of a hot-air current. As an amendment to this, I should propose putting the conductor along that side of the house which is the warmest—*i.e.*, near fires, etc.—and for this reason: copper when heated is negative, and vapour is positively charged. Unlike electrics attract each other, so we should have a moister air surrounding the conductor, drawing the electricity towards it, vapour being the better conductor in the air.

It was proposed to me the other day (whether rightly or wrongly I submit to you) that all metal work within the building should be joined together, and these again to the conductor, both at the top and at the bottom, the latter connections being made with a metal of high resistance; the entire thus forming an ante-induction circuit. The high resistance of the connections is that the current should give preference to the conductor.

Before I conclude, I should like to ask a couple of questions about which I feel doubtful. Firstly: Last year, at the Royal Meteorological Society, I saw a number of beautifully-taken photographs of lightning. In nearly all of them I noticed *two parallel flashes*, one seemingly going up, the other downwards. In fact it looked as if the current had wanted to go to one part of the earth from another, but, finding the resistance high, had taken a route through the air, via clouds, back to earth. I should like to know if this ever occurs.



Again, something might have been said about fire-balls or

ball lightning—their origin, and the cause of their explosion. Mr. Joseph.
A probable way to account for them might be the following: Fire-balls are seen, as a rule, only during severe storms. Now some hurricanes, like cyclones, are but vast whirlwinds, blowing round an axis in sinuous spirals towards that axis. This was first elaborated by Mr. Redfield and Col. Reid, and verified by Professor Maury of the Weather Bureau of the United States. Now then, imagine an instantaneous flash of lightning descending this spiral and suddenly encountering the earth's resistance. The whirling would for a second or so continue above ground, thus forming the fire-ball. The bursting of these balls might easily be caused by the sudden expansion of the vapour inside, on the pressure from without (caused by the whirling) being diminished. Another view as to the cause of the report (not, however, held by me) is, that in the centre is a vacuum to which the outer air can have no access so long as the stronger spiral current of air is blowing. The moment, however, that the electric current passes through and into the earth, the vacuum caused by the flash—in addition to the other already made—is too great, and the air rushes in from all sides, making the noise heard.

Dr. Oliver Lodge, in further reply [*communicated*]: To a Dr. Lodge.
person unaccustomed to controversy, like myself, I find there is an absurd tendency to accept any position which may happen to be suggested as appropriate to me, and to reply to objections raised against a number of shady views as if they were my own, when they are really not mine at all; being, indeed, often only supposed to be mine because they are repudiated by my critic.

Another tendency, more strenuously to be guarded against, is a stupid feeling of semi-irritation when one finds that statements carefully recorded as the result of much thinking and experimenting, combined with some acquaintance with the fundamental theory of the masters of the science, are called dogmatic and lightly set aside, not on account of any real objection or valid argument, but solely and conspicuously because they do not happen to fit in with the speaker's preconceived opinions or hastily interpreted experience.

Dr. Lodge. In replying to the criticisms with which my paper has been thus far honoured, I shall endeavour to resist both these tendencies; and especially shall I try to pick out of the criticisms those points which seem to me wholesome and salutary, and which I can more or less completely accept.

First of all, I must clear away some misapprehensions which have arisen in connection with questions of history and priority. They have arisen partly through misapprehension of what I said or intended to say, partly on account of a distinct oversight on my part in not acknowledging some prior work of Professor Hughes and M. Guillemin in the same direction.

To take the last first. I well knew that experiments in connection with lightning protectors had been made by M. Guillemin and Professor Hughes, because after my Society of Arts lectures Professor Hughes was good enough to write me an account of these experiments, and to consent to my request that his letters should be published in the *Electrician*. There they are for all to see, and it was a mere oversight that I did not refer to them in the corresponding portion of my paper. I suppose it was because I was trying to explain the *facts* quickly, and was not thinking about history or priority. I can assure Professor Hughes that nothing was further from my intention than to claim, or by silence to suggest a claim to, any priority in connection with such experiments.

These experiments showed clearly, in 1865, that no simple shunt arrangement could entirely protect a telegraph instrument from a Leyden jar discharge; and though they hesitated to press their conclusion to the case of lightning in all its completeness, yet I venture to think their experiments demonstrated this also. Demonstrated—not, of course, that lightning protectors can never protect, nor that they may not always *partially* protect, but that in a number of cases they may partially, yet very seriously, fail.

That is one point where I hope I have now set myself right; the other is due, not to an oversight of mine, but to a misapprehension by my critics of what I intended to say. In emphasising the importance of the "impulsive rush" case of lightning discharge (the only one very liable to occur, perhaps, during a heavy

shower) with reference to the absence of protective virtue from points, and the ease with which they, as well as everything else, could be struck by a noisy and destructive flash, I said: "The steady "strain case was the only one ever contemplated by the older "electricians; in fact, so far as I know, it was my experiments "last year which first called attention to the other case." Dr Lodge.

These words I used, and these words I still repeat. But the reason I used them was not, I need hardly say, to claim a miserable figment of priority in so trivial a matter, but to emphasise the fact—for I believe it to be a fact—that the impulsive rush case of lightning discharge had not previously been attended to, and that hence many vitally important features had been overlooked.

In the "steady strain" case, points fizz off quietly and are not easily struck by a flash. In the impulsive rush case (*pace* Mr. Wimshurst for the present) they do nothing of the kind.

The important thing, therefore, in the words I have quoted is not the words "my experiments," but the words "experiments "last year." It is not to claim the authorship, but to emphasise the recentness, of the observations of this hasty and unprepared-for discharge.

It may be that the calling attention to the possibility of this case of lightning discharge is after all not recent—I only said it was recent "*so far as I know*"; but, if noticed before, it has not been emphasised or even mentioned in any books or papers on the subject that I have seen. Nor, indeed, do any of my critics really assert that it is an old idea.

Professor Hughes seems to think that I mean, by "the im-"pulsive rush," a Leyden jar discharge in general, and refers me back to the year 1752, when a Prof. Muschenbroeck, or his pupil Cuneus (*vide* Ganot or Deschanel *passim*), appears to have anticipated me by something over a century. But the gentleman who first noticed an electric spark—Dr. Gilbert, I presume—must be credited with a still earlier knowledge of an "impulsive rush" in that sense. That, however, is hardly the sense I intended.

Mr. Wimshurst, in his remarks at the first evening of the discussion, seemed to think I meant to claim the mode of obtain-

Dr. Lodge. ing impulsive rushes from the outside coatings of two Leyden jars; and pointed out that his machines were so arranged that the outside coats of jars could at any time be conveniently connected to the outer circuit.

Fortunately, I did not mean or imply this either; though, forgetfully, I might accidentally have implied something of the sort, because I cannot help perceiving how much more neatly many Leyden jar experiments, recorded in books or shown at lectures, could be performed, if this mode of connecting to the outside circuit were more generally employed, instead of using a discharger worked by hand or by pulling a string. The advantage of having all the variable part of the circuit at zero potential up to the very instant of discharge, is so marked, and I have used this arrangement so frequently, that I might have been tempted to forget that it was not really a perfectly well-known, though too frequently forgotten, method. I am sure, however, that when attending Professor Carey Foster's classes, long ago, I saw Leyden jars thus connected up to magnetising spirals or other such things; and I seem to remember also having first seen it with something of the same sort of puzzlement as to the way it acted, which I detect, or fancy I detect, in some few of the speakers, especially during the first evening, with regard to case *b*.

Many remarks have been made—and some weighty ones—on the subject of the probable absence of conducting power in clouds, and the consequent difficulty in satisfying the conditions of the B flash (*i.e.*, the impulsive rush, or spark between bodies initially at the same potential). I admit at once that obtaining B flashes from metal sheets proves nothing whatever concerning the possibility of obtaining them from clouds. But my argument is rather converse to this. I argue (whether rightly or wrongly) that flashes often occur from clouds under circumstances which, under the *a*, or steady strain, or high potential condition, scarcely seem natural. Thus points are sometimes struck and melted by flashes; and it is not easy for them to be struck in case *a*. Moreover, from a cloud violently raining flashes occur; whereas one would expect rain to lower the potential gradually.

Again, from a cloud resting on a hill-top flashes occur; and sparks from a badly insulated body are at once suggestive of impulsive rush conditions—that is, sparks from a body of zero potential. Similarly, I understand that a cloud perforated by the Eiffel Tower has carried on a thunderstorm to people below. I doubt if any of these things could easily happen under the conditions of case *a*. Dr. Lodge.

But, it will be said, surely some of the facts you adduce establish the bad conducting power of clouds. To a great extent the Eiffel Tower case (if a fact) does. The hill-top case merely proves that the *hill* was a poor conductor. I can easily get long sparks from metals roughly uninsulated, as by wood, water, or soil.

Suppose, however, it admitted (not as proved, but as probable) that clouds are poor conductors, what then? All that we can assert is that the whole of a cloud, or even a large portion of a cloud, is unlikely to discharge at once. I quite think that that is so. A calculation of energy shows that a violent flash need only discharge a very small portion—a few square metres—of a charged cloud, and that the same cloud may therefore go on sparking for a long while, as, indeed, it appears to do. Now no great conducting power is needed for a discharge from a small area at a great elevation: the lateral component of rush is in that case negligible.

Of course it *may* be asserted that clouds conduct so badly that no "flash" in the proper sense can ever occur. An answer to that assertion is the existence of lightning.

All that poor conductivity in clouds has to say concerning the impulsive rush seems to me this: that when any part of the cloud receives a violent disturbance from some A flash in its neighbourhood, that same part which receives it is most likely to spit off the consequent B flash. Whereas, with a perfect conductor, any other portion would be almost equally liable. Poor conductivity goes, indeed, to *help* the violence of the impulsive rush; for the essence of it is that a conductor of small capacity at zero potential shall be suddenly overloaded. Now, if a charge suddenly communicated to a portion of a large cloud could instantaneously be shared with the whole, the potential would be reduced, and nothing very violent need occur.

Dr. Lodge. This may sound like special pleading, but it is only recording the circumstances that have to be attended to. What the *facts* are must be determined by direct observation on flashes and clouds.

But let me here beseech meteorologists to remember that establishing the condition for some one flash or class of flashes does not establish the impossibility or improbability of very different conditions obtaining elsewhere or at other times. There are varieties of thunderstorms, varieties of clouds, and varieties of flashes. Every good and accurate observation will be a help to fuller knowledge, but it will take years of enlightened experience and observation before all possible varieties and circumstances of discharge can be supposed exhausted.

Before leaving this subject I should like to remind the Institution that I have never hesitated to contemplate the imperfect conductivity of clouds, whatever the consequences of that imperfect conductivity might be; and, in proof of this, I enclose an extract from the report of my remarks at Bath, as published in the scientific journals at the time:—

*Extract from British Association Discussion.*—" There was " one point where Mr. Preece might have attacked him, but " where he did not think Mr. Preece had made out the full " strength of his case, namely, the question—What are the " conditions of a flash? He (Professor Lodge) had assumed " that a flash behaves, or may behave, like the discharge of " condensers in a laboratory; but it was a question whether a " cloud discharge was of this kind. A cloud is not a good con- " ductor; it consists of globules of water separated from one " another by inter-spaces of air; it may be compared, therefore, to " a kind of spangled jar; when a spangled jar discharges there is " no guarantee that the whole of it discharges, it may discharge " out in a slowish manner; it may be that you have first a bit of " discharge, then another bit, and so on, so that you may have a " kind of dribbling of the charge out of it, and you may thus " fail to get these oscillatory and sudden rushes. At the same " time he did not think that they could always guarantee doing " this with cloud discharges; and it would not be safe in arranging

"protectors to protect for only one case, and that the easiest; Dr. Lodge.
"they must provide for the possibility of a sudden and violent "discharge. Still, the conditions of actual lightning were to be "ascertained by observing lightning, and not by experiments in "the laboratory."

Proceeding now to the remarks of the first speaker at the second evening of the discussion, I must draw a clear distinction between Mr. Wimshurst's experiments and Mr. Wimshurst's comments thereupon.

With respect to the experiments, I feel obliged to him for exhibiting and emphasising several points which, for want of time or otherwise, I had rather slurred over; also for recalling to my memory a little point which I had forgotten, though it is in my assistant's note book; and, lastly, for detecting an interesting matter which had escaped me.

To take these successively. 1. He seems to have exhibited side flashes, from badly earthed conductors and to well earthed bodies, more satisfactorily than in my hurry during the paper I managed to do. It is not likely that he exhibited them so strongly as I have obtained them in the laboratory, because the violence of many of these side flashes is a thing that strikes observers with astonishment; and with a *long* conductor, as stout as you please, it makes very little difference whether its far end is "to earth" or not. It does undoubtedly make some. Professor Threlfall, and Mr. J. Brown of Belfast, have both seen these effects at Liverpool, and neither, I fancy, would contemplate with equanimity the idea of bringing their knuckles near a conductor when struck, however well earthed it was.

At the same time it is perfectly true, and I must have often recorded the fact, that from a well earthed conductor the sparks will not charge a Leyden jar. They jump in and out again. Sometimes, indeed, there is a residue of charge, but it is accidental what sign it is, and it is always merely the tail end of a series of oscillations cut off by resistance at some arbitrary point.

A more striking experiment is to connect a gold-leaf electro-

Dr. Lodge. scope to the conductor. If the connection is metallically perfect the gold leaves are nearly quiescent. With imperfect connection they may diverge; they always slightly kick. The experiment is a little rough on the electroscope, for it strains the leaves downwards, and blows fragments off sometimes; but it is a striking thing to be able to take a half-inch or even a one-inch spark, of considerable power and noise, from the cap of an electroscope whose leaves hang stiffly down all the time and barely twinkle.

These side flashes are not very painful, they look worse than they feel: the charge hops in and out of you without going through you much or disturbing the nerves seriously. I by no means assert that a man would *necessarily* be killed if touching a conductor struck by lightning; but it would surely be a position of considerable danger.

2. The fact which Mr. Wimshurst has observed, but which I had missed, is this: that when side flashes are tried for at different points in the length of a wire joining the outer coats of two equally-insulated symmetrical jars, they are obtained more strongly towards either end of the wire, and are not obtainable at all at the middle. The middle is, in fact, a node. There are stationary waves set up in the wire, whose ends are now at high potential and now at low potential alternately, just like a long bath which has been tipped and set down sharply. To and fro the water splashes, and the ends are now at high and now at low level alternately, but the middle is a node and remains of average level all the time: it is at zero potential, and no spark is obtainable from it. It is quite an interesting fact, and one that it would have been a pity to miss. I am sure we are obliged to Mr. Wimshurst for discovering it.

3. Lastly. The little point Mr. Wimshurst has recalled to my memory is this: that when a cloud or top-plate is negative, a small terminal or point gets struck rather more easily—*i.e.*, at a lower elevation—than a big terminal or dome, even under the circumstance of the impulsive rush.

The fact is often so; but Mr. Wimshurst's account of it may lead persons who have not tried the experiment to over-estimate

the magnitude of the difference, which is frequently quite inappreciable: being, indeed, often non-existent. Dr. Lodge.

If the impulsive rush is violent—*i.e.*, if it proceeds from a pair of large jars, highly charged, into a plate of moderate size—the difference is non-existent. Careful measurement fails to show that the things equally struck are not all the same height; and this whether the "cloud" be negative or positive. This was the case I exhibited at the meeting, and there was no need to notice of what sign the top-plate was. But if the rush be made less violent, either by using small jars or by charging them feebly, a difference is observable. A point then gets struck, as Mr. Wimshurst showed, at a distinctly lower elevation than a knob does, whenever the overhead flash is negative—not when it is positive.*

Those are the facts, and we are indebted to Mr. Wimshurst for calling attention to them, but, as to what the moral and practical bearing of them is, opinions may differ.

Certainly it in no wise upholds the statement that points always discharge silently and cannot get struck, which is what has always been meant by their "protective virtue." Rather it would seem that they get struck in an impulsive rush always as easily as anything else, and sometimes, as Mr. Wimshurst shows, still more easily. For remember that the "striking" is not a fizz or leak of a gentle and protective kind, but is a violent and destructive flash.

The other experiments exhibited by Mr. Wimshurst are less important. One has to do with the experiment of the alternative path, or, as Mr. Wimshurst prefers to call it, a "bye-pass."

I did not make any reference to this class of experiments in the paper, because I consider I have got to the bottom of them and thoroughly understand their conditions, which I have published; so that, except in a very serious memoir, they are not really discussable.

This may sound a desperately arrogant thing to say, but if so, I

* For measurements see Appendix IV.

Dr. Lodge. cannot help it. One would make no progress at all if one could never get to the bottom of a thing and feel one's ground secure. And I am so profoundly conscious of ignorance with respect to a vast number of phenomena that I have no care to waste time by pretending ignorance where it does not exist.

May I therefore save time by saying that all the experiments of Mr. Wimshurst on this particular matter are completely in accord with my theory;* and also that the remark of Professor Hughes concerning the probable delay of the B spark behind the A spark is completely borne out by theory. The lag is, in fact, a quarter period of the oscillation.

Referring to another remark, I may also say that at Liverpool we have recently obtained excellent photographs of a slowly oscillating spark on a rotating sensitive disc, and that the constituent oscillations are not only conspicuous, but well spread out and accurately measurable, and in agreement with theory within one half per cent.

Finally Mr. Wimshurst seems to have exhibited an ordinary spangled jar, in which a charge electrostatically induced in its outer coat exhibits sparks between the spangles in a simple and well-known way. He thinks this is analogous to the sparkings on the wall-gilding excited when an oscillating current is sent through a wire circuit lying on the floor; but, without laying unnecessary stress on the distinction between electrostatic and electromagnetic induction, I think I am justified in saying that the two experiments are not analogous.

Some remarks which he made in apparent detraction of the Voss machine I do not completely follow. I am very ready to admit that a Wimshurst machine is an excellent instrument, but it is not necessary to condemn the Voss machine, or to suggest that it is worse than an old frictional machine, in order to believe that. The Voss machine I have found most excellent too, and for small size machines I personally prefer it; but that is perhaps an idiosyncracy based upon finding it easier to turn, easier to take to pieces, thoroughly under control, and mechanically simpler.

* See Appendix I.

There is not the slightest difficulty in telling which terminal is positive, the brush appearance to the points tells us that whenever we choose to look. Certainly it easily reverses. That is its one objection; and occasionally it is an annoying peculiarity, though I have got pretty used to it. Dr. Lodge.

I should like to remark here, what after all is fairly obvious, how great service has been done by the developers of the modern influence machine, from Nicholson, Varley, and Thomson, to Holtz, who did so much, and on to Mr. Wimshurst himself.

Many of the remarks of Professor Hughes I have already incidently answered or referred to. He says, quite rightly, that I maintain flat ribbon and plenty of surface is an improvement on round rod, but as to whether it is a matter on which very great stress need be laid, I will defer discussion till I come to the observations of the President.

Professor Hughes' experiments on iron versus copper no doubt agree with mine whenever he uses alternating currents of the same frequency, and disagree when he uses alternating currents of much lower frequency. For telephonic frequencies iron has much greater impedance than copper. Oliver Heaviside does not for an instant deny this, but he says that there may be circumstances in which this extra inertia is an *advantage*, in that it helps to preserve the character, or quality, or shape, of electric waves, although it admittedly transmits more slowly and weakens them. If Mr. Preece and observers in America find in practice that copper wire is better for telephony, as they apparently do, then that means that the character of the vibrations is *sufficiently* preserved in copper, and the comparative absence of retardation is all to the good. It may, however, still happen that in submarine cables iron will shew an advantage. It is a curious paradox no doubt at first sight, and whether the advantages outweigh the disadvantages in any particular case is a quantitative question of no extreme simplicity; though, nevertheless, it has in general terms been worked out.

But the question for ordinary Leyden jar frequencies is much simpler. For them the *impedance* of iron and copper of the same

Dr. Lodge. diameter is practically the same, unless the wire is very long or very thin. The *resistance* of the iron is much greater than that of the copper. All this can be expressed, and has been expressed quantitatively, with, in my opinion, complete certainty, and beyond the reach of any but revolutionary doubt to which all scientific doctrines are liable.

And on the practical side one may say this: The circumstance of a telephone wire and of a lightning rod are not only different, they are, in some respects, opposite. The object of a telephone wire is to convey electric waves, unaltered and unweakened, to a distance. One object of a lightning conductor is to wipe them out and dissipate their energy as soon as possible. The very properties which are detrimental in one case may be desirable in the other. It is no doubt a quantitative question how far it is wise to wipe out energy in the lightning conductor itself, and Major Cardew thinks it is unwise to do so at all. Possibly; but at present I hold that so long as total impedance is not appreciably increased, and so long as the margin of melting is not too closely approached, so long it is desirable to dissipate energy wherever you can, and to check the violence of the oscillations as rapidly as possible; and hence I hold that a moderate amount of true *resistance* is no defect in a lightning conductor.

Everyone must admire the beautiful method by which Professor Hughes tests his wires for circular or cylindrical magnetisation.

In Mr. Symons' objection to laboratory experiments being regarded as at all analogous to lightning, and still more clearly in Captain Cardew's solemn protest in favour of the dignity of a thunderstorm and the absence of dignity from experiments conducted with tinplate, I seem to hear echoes of some fine old crusted objections which were current in the time of Franklin, and which were, perhaps, somewhat more in harmony with that time than with the present. Now that the subject has been mooted, however, I may be permitted to assert my conviction that the intrinsic dignity and solemnity of nature is as present in a spark one inch, as in a spark one mile, long; that, looked at with

insight, a drop of ink hanging from a funnel* may be as inspiring Dr. Lodge. an object of contemplation as a cataract; and that to explicitly claim special dignity for the one is implicitly to reject it from the other. True, one's subjective feelings of awe are not aroused in the one case as in the other, but that has to do with the relative size of the human body; and so far as an observer is overwhelmed or liable to have his nerves shattered out of existence by the phenomenon he is witnessing, just so far he is not in a perfectly collected and scientific frame of mind. Moreover, experiment under modifiable circumstances has enormous advantages over mere observation, especially observation which is only occasionally possible. Hence experiments in a laboratory, and a thorough understanding of what occurs on a small scale, are a very good introduction to the enlightened study of atmospheric electricity, though they are by no means to be regarded as a substitute for that direct study. So let me here emphatically admit and insist, in full agreement with what I suppose was the intention of these speakers, and with the more direct assertion of Colonel Armstrong, that experiments on actual lightning are highly desirable. Such experiments, as a sort of practical outcome of the Mann lectures, are, I hope, in course of establishment, by means of the bright idea of the editor of the *Electrician*, and by the enlightened co-operation of the Eastern Telegraph Company and Sir James Anderson. At foreign stations storms are frequent, and, with suitable appliances, I trust a record of valuable observations may be forthcoming in, say, five or ten years—without, let us hope, the "expenditure of any observers." The large number of photographic records of lightning which are now being obtained all over the country are likewise very valuable aids to progress.

Mr. Symons is a determined and consistent advocate of large cross-section for conductors, maintaining that they are liable to be fused; and as this is a question of observation and experience of damage, I should be disposed to allow much weight to his opinion. Unfortunately, however, the two instances

* Sir W. Thomson's "Popular Lectures and Addresses," page 48.

Dr. Lodge. he adduces in support of his contention are not such as will bear serious examination ;* since in one it is the links of a chain which are melted, and in the other the conductor is not fused, but merely "burnt by use."

Mr. Symons beautifully illustrates my remark, that whenever a building is damaged, it is always because the infallible rules of the Lightning Rod Conference had not been followed. The church at Garding was damaged because "they took the conductor down "the *North* side of the steeple, not down the *wet* side, as we "advised"!

With reference to his depreciatory observations on the Hotel de Ville, Brussels, and the Belgian School of Electricians, I shall leave them to speak for themselves. Mr. Symons will find that the death of M. Melsens has by no means removed from that country every man skilled in lightning conductor appliances.

I do not know who it is that has told Mr. Symons that all the lightning conductors "on the Houses of Parliament, Westminster "Abbey, or wherever it may be, are wrong in principle and are "dangerous." Certainly I have not: I have never either said or implied that a well-erected lightning conductor is other than a source of safety as far as it goes. I said, in my first Mann lecture, that the neighbourhood of a factory chimney is "a source "of mild danger." And so I believe it is, even when possessing a good conductor. But most distinctly without a conductor it would be a source of danger very much other than "mild." I never contemplated such a case, nor supposed that anyone would endeavour to increase their safety by pulling down lightning conductors!

Colonel Armstrong quotes several cases of damage, wherein the earth resistance was found to be from 100 to 200 ohms. With all deference to his experience, I feel very doubtful if this amount of resistance is sufficient to account for the damage. I have admitted all along that the better the "earth" the better for everybody; but I have also pointed out numerous other reasons for failure and damage beside a bad earth.

* See Appendix III.

In his most interesting observation of the cartridges exploded in a sealed metal case, we ought to be sure that the flash did not pierce, or melt, or ignite to redness, the case. Any violence of that sort might explode things in a very commonplace and unelectrical fashion. So also the violent shock due to expansion of air, or what Sir W. Thomson at Bath called the sound-wave, may be expected to have an effect on detonators. Dr. Lodge.

Mr. Adams calls attention to the radiation of energy from conductors. It is most true that radiation goes on from all alternating circuits; and from linear oscillators like those of Hertz, or, what is essentially the same thing, from cloud and earth joined by a lightning conductor, the intensity of radiation is prodigious. It is a most important way of getting rid of energy; it is expended on the ether at the rate of many horse-power. See a paper of mine in the forthcoming July *Philosophical Magazine.* I am not sure, however, that the experiments of Mr. Adams illustrate this radiation very exactly. Nor do I quite understand his last sentence. If the facts be as he states, do they not place iron above copper rather than below it?

Mr. Spagnoletti's statements are most interesting. Mr. W. Groves, of Bolsover Street, tells me he has seen the alphabetical step-by-step machine worked two or three letters forward by atmospheric electricity of some kind on a wire between his place and Sir Charles Wheatstone's.

Mr. Evershed's observations on clouds go to support the conclusion that the well-known "return stroke," and such like observations, prove the conducting nature of clouds—of some clouds at any rate.

He is quite right in pointing out that all oscillatory character is liable to be wiped out of a discharge which has had to travel a great length of thin wire, and that the finding of a quiet tail of current leaking away in some obscure corner of a telegraph instrument is no criterion as to the vigour or character of the main flash whence it arose.

Passing to the criticisms communicated since the meeting, I have no special remark to make on the statements of Major

Dr. Lodge. Cardew except to say that several seem to be mere statements of personal opinion, rather lightly and casually made.

With respect to No. 30: "We all know there are millions of volts," my point is misapprehended. It is familiar that there are millions of volts between cloud and earth; it is not familiar that there may be millions of volts between the top of a well earthed and stout copper lightning conductor and the earth. When Major Cardew says, as he does towards the end of his remarks, under head "*Fourth*," that a conductor of small impedance is desirable, everyone must agree with him; but when he goes on to say that such a conductor is obtained by following the rules of the Lightning Rod Conference (or any other rules for that matter), it is necessary to disagree with him. The impedance could not be considered in any sense "small," even if a column of pure copper, a foot in diameter, was employed. The impedance of such a column, 100 metres high, to a current of frequency one million per second, is nearly 900 ohms.

Major Cardew twits me with having supposed that an earth resistance can be got as low as one-thousandth of an ohm, but if he chooses to refer to my Section 84 again he will find that I have supposed nothing of the kind. If I had said the billionth of an ohm it would have expressed precisely the same meaning.

The experience which Colonel Bucknill has had in connection with the War Office conductors, and the attention he has for many years given to the protection of powder magazines, render his practical remarks very weighty. I regret they are at present so brief.

And now I come to the remarks of the President himself. For such of them as are personal, I may be permitted to express to him my thanks. There is one point—that with reference to article 56—where I wish to explain my meaning more fully.

My statement runs, "Flat ribbon has a slight advantage over "round rod, but not enough to override questions of convenience." Now it is of course perfectly true that extent of surface diminishes impedance, that Snow Harris's hollow tubes were

better than Faraday's solid rods, and that if only one single stout conductor is to be used, then tape is distinctly its best, as indeed it is then also its most convenient, form. But I wished to obtain small self-induction by splitting up the conductor into detached portions, making each portion fairly thin. For these small conductors also, no doubt ribbon is electrically better than wire. But will it last as long? Is iron ribbon easy to obtain? So long as common galvanised-iron telegraph wire is so easy to procure, it seemed a pity to insist on any other shape of cross-section, especially since a ribbon of corresponding cross-section would have to be so thin as to be very liable to rust away. All this I had in my mind in writing section 56. I had so frequently insisted on the advantage of large surface in my theoretical papers, that I thought it permissible to throw it over in the practical portion for solely practical reasons, *i.e.*, because to insist on it to the bitter end seemed to entail trouble and expense. Dr. Lodge.

But, it may be objected, why then did I say that tape had only a *slight* advantage over rod? Well, it is a matter of arithmetic to reckon how much better a given tape is than a given rod. If I make no mistake this is the result.

The self-induction of a rod of sectional radius, $r$, is to that of a strip of breadth, $b$, both being of same length, $l$, very nearly in the ratio

$$\frac{\log 2\,l - \log r - 1}{\log 2\,l - \log \frac{1}{4}\,b - 1},$$

the currents in each case being of such rapid frequency as keep to the outer surface.

Now, unless the rods are very short, or unless the breadth of the tape is enormous—its thinness being likewise excessive, if it is to consist of the same amount of metal as the rod—this ratio is not much greater than unity; and the same will be the ratio of their impedances.

Similarly the difference between hollow tube and solid rod is not of any *great* practical moment in lightning-rod circumstances.

With far lower frequencies, such as 100 per second, when frictional or dissipation resistance is the important part of total impedance, and when currents penetrate a certain depth into the

Dr. Lodge. substance of a conductor, it is an altogether different matter, and the advantage of tube or plate over rod is then enormous; as Sir William Thomson has so thoroughly brought home to everybody.

Suppose, as rather an extreme case, the ratio of self-inductions for tape and rod were as great as 2, then the tape would have half the impedance of the rod for currents of the same frequency. Such a case I have experimented on; but I should not like to insist even then on the use of the tape in preference to the rod, if there were serious practical objections on the score of cost, unsightliness, want of durability, &c., to be made against it.

If there are no such objections, then tape by all means, and the thinner and broader the better.*

It may be just borne in mind that decreasing the self-induction goes to increase the frequency, and hence that if ever the conductor forms a large portion of the entire path of discharge, the advantage of reducing its inertia is still less marked, for the impedance depends only on the square root of L in that case.

The President misunderstands me in one place, where he thinks I have said that it is no use connecting conductors to water mains. I do not know whence this misunderstanding can have arisen; possibly from section 59, where I say, "A good "and deep earth should in general be provided, independent of "water and gas mains." This may not be perfectly clear, but my meaning was as follows:—

Have at least one independent earth, made by a well or other suitable means, in addition to water main connections. In other words, do not depend *solely* on water main connections.

Probably this is a counsel of perfection for the case of ordinary dwelling-houses, but for an important building I think it may be wise, for these reasons. Mains are near the surface, and in some weathers the soil near them may have become dry. Also they ramify into the house and into other people's houses, and will therefore conduct any violent charge communicated to them partly into these places, where, by a branch flash to a gas pipe, damage may be done and gas ignited.

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* See Appendix II.

I have shewn that well earthed mains can thus give off unexpected sparks at a fair distance, even when only a Leyden jar discharge is run into them; hence I feel sure that some cases of damage result from lightning being thus brought underground into houses. Dr. Lodge.

Having a good independent earth in addition to water mains is not indeed a *security* against this source of danger, but it is a step towards it. I do not propose to avoid the mains altogether, because in so many places it is not practicable. Whether you connect to them or not, the lightning will go to them if it chooses, unless they are far away; and it is better to give it an easy path rather than let it fly through air or soil, and knock, or melt, or burn a hole in them. It may sound absurd to talk of lightning knocking a hole; but the concussion of air is so great as to produce all the effects of an explosion. I entirely agree with Colonel Bucknill, that damage is most usually done wherever an air gap is jumped. I think compo-pipes are mostly melted where a flash jumps to or from them than where it simply passes along them.

With reference to the load of coke, I was under the impression that it was cheap and easy. It is not novel, and there are dozens of other well-known plans, if any are handier.

Lastly, I come to the most interesting topic of all—the cartridges exploded in metal cases, mentioned by Colonel Armstrong (always provided that they were not merely ignited by heat), and the President's remarks thereupon.

Experiments on the effect of screens have gone on at intervals for some time in my laboratory. We can suspend a little electrometer-like needle, charged positive at one end and negative at the other, inside a tinfoil-coated glass box, and can deflect it by moving towards it a charged ebonite rod. But in order to succeed, the lid of the box must be so put on that a Léclanché cell shall not be able to ring a bell by conduction along the box. In other words, there must be a breach of continuity, or at least a very high resistance in the circuit. So soon as a Léclanché current can pass, no practicable motion of the ebonite rod can disturb the needle in the slightest degree. But there must be

Dr. Lodge. some limit to this. A stronger charge moved more quickly might do something, so we have taken to firing charged bullets out of a miniature cannon towards the box. The bullets are no joke. They go slick through three inches of wood, and it takes a considerable thickness of earth to stop them. They can be charged and discharged, or reversed at appropriate stations, by passing through charged metal; or they can be made to propel suddenly a permanently charged disc. But not a wink does the needle show. That only means that the tinfoil coating is too thick. We are going on to gold leaf, or a silver film, and so gradually thinning down till an effect is obtained. An effect *must* be forthcoming with a thin enough conductor, because one can go by gradual degrees to none at all. Liquid screens can, of course, also be employed, and probably quite a decent thickness of these will be fairly transparent. I would suggest, principally by way of query, that the action will be as follows:—

Let the resistance of a metal box to a current along it be R, then when a steady current (C) flows, a difference of potential (R C) will exist between its ends, whence electrostatic lines of force will radiate both inside and outside, and an electrometer needle inside will feel them. Now, instead of passing a current through the box, move an electrostatic charge, Q, with velocity, $v$, towards it. An electric displacement occurs which results in a momentary current, proportional to Q $v$, in the metal walls of the box, and to a slope of potential some specifiable fraction of R Q $v$ which the needle may feel.

When a spark strikes the box, a momentary current similarly exists in its coating.

Now, if the momentary current has no time to penetrate the entire thickness of the metal so as to flow in its innermost layers, then none of the slope of potential due to it can be felt inside the box, though outside it would be mixed up with the much greater direct action of the electrostatic charge. But if the covering is thin enough for some portion of the current to travel by its innermost layer, then an electrostatic disturbance will occur inside, which the needle, or a frog's leg, or a vacuum tube, or a microscopic spark gap, may be competent to feel. I may say,

however, that frog's legs do not appear very sensitive to this class of effects. A zinc copper contact disturbs them vastly more. Dr. Lodge.

Now, if the metal be iron, the depth to which the transient currents penetrate is very much less than it is in the case of non-magnetic metals; hence a superficial layer, thick enough to make an effective screen if made of iron, might be a very imperfect screen if made of any non-magnetic metal. On the other hand, the resistance of iron is so immensely greater than that of non-magnetic metals to these transient currents, that if the layer were thin enough to permit an effect to be appreciated at all, the slope of potential to be felt might be greater than with copper, or even with tin or lead.

May I ask the President to be good enough to say if any or all of this is nonsense? and, if it is not (being perhaps only what he instinctively had in his mind when he gave a hint about iron cages at Bath—the hint which he repeated in reply to Colonel Armstrong), then he will probably be able to calculate off-hand how much effect is to be expected in any given case, and to lay down rules whereby the disturbance liable to be felt through a given thickness of a definite metal can be at once calculated.

And now with respect to the "Practical Suggestions," which I provisionally made at the end of my paper in order that they might receive the benefit of criticism; and between which and the main body of the paper I have always drawn a clear distinction. Several have been criticised, and some have been shaken. May I quickly run over the list, indicating those which I still strongly uphold and those which I regard as doubtful?

Nos. 51, 52, and 53, I suppose Mr. Symons would say, are "reprinted from the Lightning Rod Conference." They have, certainly, a fine ancient flavour of orthodoxy about them. But he would not have me throw over everything, both bad and good! They seem to me good.

Nos. 54 and 55 I strongly uphold.

No. 56 I have indicated my reasons for provisionally maintaining. If they are bad reasons I trust the President will at once crush them out of existence.

Dr. Lodge. No. 57 I regard as very important, especially its latter sentence. It is just one of the points wherein the rules of the future will differ from the rules of the past.

Nos. 59, 60, 61, 62 are very much open to discussion.

Nos. 63 and 64, I think, are sound. But very likely Colonel Bucknill's addition to 63 is an improvement.

No. 65 is very doubtful. There are, as Colonel Bucknill points out, very serious exceptions to it, even if it can ever be regarded as a rule.

Nos. 66 and 67 are sound, I think.

No. 68 is a fact. (This, Major Cardew will say, is "dogmatic." That also is a fact.)

No. 69 is a counsel of perfection: intended for powder magazines, not for dwelling-houses. Sir W. Thomson said it, or something like it, at Bath. It must be remembered, however, that "gasometers" are damaged when struck, according to reports in newspapers.

Nos. 70 and 71 are very doubtful. I throw them out as suggestions which experience must settle.

No. 72 is, I think, all right, but after the words "a load of "coke" one may add, *or any of the well-known earth contact arrangements.*

No. 73 has been wholesomely criticised. I think I am safe in still saying "it is not an unmixed good." But very likely the gain outweighs the loss. In fact, I have in the Mann lectures advocated the proceeding as good on the whole.

No. 74 I should be glad to be able to omit, but see no present chance of it.

Nos. 75 and 76 have been well criticised. I quite feel the force of the criticisms, and am glad to take refuge in No. 74. At the same time a righteous substitute for No. 76, if it be wrong, is very desirable. The middle part of No. 76 (a chimney with inside metal shaft) is a frequent and very difficult case. It embodies the advice which at present, for want of better, I give. Boiler firemen, engine tenders, and dynamos, would be apt to be damaged, I fear, if contrary advice were followed.

No. 77 is, I think, generally true, for such things as rain-

water conduits under eaves, for picture rods, &c.; not, of course, for a miscellaneous collection of metal objects. Dr. Lodge

No. 78 is, I think, right, if not too troublesome in practice. A crown of long points leaning well over into the smoke may do as well.

No. 79 probably belongs to Mr. Symons and the Lightning Rod Conference.

Nos. 80, 81, 82, 83 are intended to apply only to desperately important places: dynamite factories, petroleum tanks, and such like. They are of course perfectly open to criticism.

No. 84 is correct.

Nos. 85, 86, 87 are hints towards more elaborate methods of testing than the out-of-date plan at present in use. I call it out-of-date because it is based upon the untruth of No. 57, and upon entire ignorance (very natural a few years back) of the great obstruction offered by good conductors. It is better than no testing at all, but it is extremely inadequate, in that it detects only one, and that a comparatively unimportant, kind of flaw.

Nos. 88, 89, 90, 91 have to do with lightning "protectors," and, I suppose, are orthodox and indubitable.

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## APPENDIX I.

### Theory of B Circuits, of "Alternative Path" Experiments, and of Side Flash.

Consider a couple of jars connected to the terminals of a machine by their inner coats and to a wire circuit by their outer coats (Fig. 1).

They form an ordinary circuit with a capacity inserted equal to the semi-harmonic mean of the two jars separately, and an air gap of adjustable width at A; and the maximum difference of potential producible in it is determined by the distance of the A knobs.

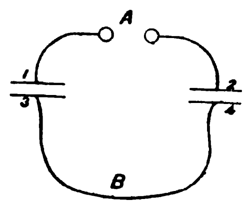

Fig. 1.

When the discharge occurs, a current flows of course equally round the whole circuit, but the peculiarity is that up to the instant of

Dr. Lodge. discharge the B portion of the circuit is at a uniform potential. If a gap exists in B also, as it well may, the terminals of the gap may likewise be at the same potential up to the instant when the rush occurs. The discharge will, as usual, be oscillatory unless the resistance of the whole circuit be too great; and the period of oscillation will be approximately $2\pi\sqrt{(L S)}$, where S is the capacity of the two jars in series.

Now number the coatings of the two jars as shewn in the diagram, and write down their electrical condition before and during the discharge spark at A:—

| | Plate 1. | | Plate 2. | | Plate 3. | | Plate 4. | |
|---|---|---|---|---|---|---|---|---|
| | Charge. | Potential. | Charge. | Potential. | Charge. | Potential. | Charge. | Potential. |
| Before discharge... ... | $+Q$ | $+V$ | $-Q$ | $-V$ | $-Q$ | 0 | $+Q$ | 0 |
| After $\frac{1}{4}$ period ... ... | 0 | 0 | 0 | 0 | 0 | $-V$ | 0 | $+V$ |
| After $\frac{1}{2}$ period ... ... | $-Q$ | $-V$ | $+Q$ | $+V$ | $+Q$ | 0 | $-Q$ | 0 |
| After $\frac{3}{4}$ period ... ... | 0 | 0 | 0 | 0 | 0 | $+V$ | 0 | $-V$ |
| After a whole period... | $+Q$ | $+V$ | $-Q$ | $-V$ | $-Q$ | 0 | $+Q$ | 0 |

and so on, with gradual damping (the damping being omitted in the table for simplicity).

Thus, then, between the ends of the B wire exists at regular intervals almost the whole difference of potential which is able to jump the air gap at A. Strictly speaking, the difference of potential is rather less than that corresponding to the A gap, thus:—

The equation to the current at any instant is accurately

$$C = \frac{V_o}{p L} \; e^{-\frac{R}{2L}t} \sin p t,$$

where $V_o$ is the initial difference of potential corresponding to the A spark, and where

$$p = \sqrt{\left(\frac{1}{L S} - \frac{R^2}{4 L^2}\right)}.$$

Now if $L_1$ is the portion of the whole self-induction which

corresponds to the B length of wire (*i.e.*, subtracting from the whole L the part belonging to the A wire), and if $R_1$ is the resistance of the B wire, its impedance is $\sqrt{((p\,L_1)^2 + R_1^2)}$; and while a current, C, is flowing through it, the difference of potential between its ends is therefore $\sqrt{((p\,L_1)^2 + R_1^2)}\,C$. Dr. Lodge.

Now the current flowing through attains its maximum value one-quarter period after the A spark has commenced, *i.e.*, in a time

$$\frac{\pi}{2\,p}; \left(\text{more exactly, in a time } \frac{1}{p} \tan^{-1} \frac{2\,p\,L}{R}\right);$$

and inserting this in C we get the maximum possible strength of current, viz.:—

$$C_1 = \frac{V_o}{p\,L}\, e^{-\frac{\pi R}{4\,p\,L}}.$$

Hence the maximum possible difference of potential between the ends of the B wire is

$$V = V_o \frac{\sqrt{\left\{L_1^2 + \left(\frac{R_1}{p}\right)^2\right\}}}{L}\, e^{-\frac{\pi R}{4\,p\,L}};$$

that is, a certain fraction of $V_o$, the fraction being

$$\frac{\text{total impedance of B wire}}{\text{inertia impedance of whole circuit}} \times \text{damping during } \tfrac{1}{4} \text{ period.}$$

Very often a sufficient approximation to this is

$$\frac{L_1}{L}\, e^{-\frac{\pi R}{4\,p\,L}};$$

and if the wires are thick and short, or non-magnetic, and the capacity big, the damping during the first quarter of a period is often so small that merely the fraction $\frac{L_1}{L}$ will do sufficiently well.

So then, if a supplementary pair of tapping knobs be connected to the ends of the B wire, as shown in Fig. 2, and if their distance be adjusted to be $\frac{L_1}{L}$ ths of the A distance, a spark is liable to pass at these knobs.

This is what I call a B spark, and the spark gap affords an alternative path to the B wire, or *vice versa*.

Dr. Lodge.

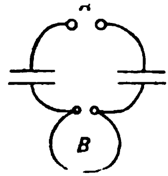


Fig. 2.

There is no need to tap off the *whole* of the B wire. Any portion however small will serve, provided the appropriate value of $L_1$ is used. The length of the B spark measures the difference of potential needed to propel the current through the portion of wire which is thus tapped. Of course, if a B spark actually *occurs*, it introduces disturbance; the knobs should be set so that it just fails.

There is one thing not here explicitly stated, but which has to be taken into account in calculating the value of R, and that is the loss of energy by radiation. With small jars and circuits this loss is very great, and it increases the value of R enormously. See a paper of mine in the forthcoming July *Philosophical Magazine.* With big jars and circuits it may be safely omitted; the experimentally observed B spark will agree with calculation. But with small jars, if it be omitted, the observed B spark will be always less than the calculated.*

In this way a measure of the damping due to radiative dissipation of energy can immediately be made.

The observation of Mr. Wimshurst about the neutral point, indicates at once that this theory also gives the length of side flash obtainable from the wire. Let any part of the B wire be put to earth, or let its natural neutral point be found, then the V calculated as above for any other point gives the length of side flash obtainable from that point to earth.

Side flash is in fact a special case of the alternative-path experiment. With a symmetrical wire like this, everything insulated and the jars equally charged, the neutral point is naturally the middle. But with a lightning conductor the lower end is to earth more or less completely, hence from the actual

* See *The Electrician* for 21st June, 1889.

bottom of the wire no side flash should be obtainable. One Dr. Lodge. always will be obtainable, however, owing to the impossibility of making a non-resisting earth of infinite capacity. Higher up, the length of side flash obtainable must be its length at the bottom plus the V corresponding to height of point tried. The maximum side flash is obtainable from the top of the wire. The strength or energy of the spark depends, of course, on the capacity of the body receiving it (if insulated); being $\frac{1}{2}$ S $V^2$, when V is calculated as already said. If it be an earthed body, then the whole discharge divides itself between the two paths, according to the laws of divided current appropriate to these conditions.

In testing a conductor, a spark should be given to the top, and the length of side spark obtainable at the bottom should be observed. All else can be calculated, except in so far as there may be defects in the visible portion of the rod.

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## APPENDIX II.

### Resistance and Impedance for Frequencies comparable to a Million per Second.

If $\frac{p}{2\pi}$ is the frequency of current conveyed by a wire of length $l$, and of ordinary resistance $r$, made of a substance of permeability $\mu$; then its resistance to currents of excessively high frequency is

$$R = \sqrt{(\tfrac{1}{2} p l \mu r)},$$

hence the resistance of soft iron is immensely higher than that of any non-magnetic metal.

The self-induction under the same circumstances is

$$L_0 + \frac{R}{p},$$

where $L_0$ refers solely to the space surrounding the conductor.

The inertia portion of the impedance is*

$$p L_0 + R,$$

* At first sight it may seem as if I were making a mistake in having an R term in the purely inertia part of the obstruction, but it is quite right. This term R happens to represent exactly the magnetisation of the substance of the wire, so far as its outer skin is magnetised.

Dr. Lodge. of which the first term is far the bigger at high frequencies, even for iron, unless the wire is very thin.

The total impedance is

$$\sqrt{(p^2 L_o^2 + 2 p L_o R + 2 R^2)},$$

of which, again, the first term usually far eclipses the others.

*Numerical Examples.*—1. Let the length, $l$, of conducting rod be 10 metres, its diameter 1 centimetre, and let it be bent into the form of a circle (if it be straight, there will be but little difference); take $\mu = 1$ for copper, or 900 for iron; specific resistance, 1600 square centimetres per second for copper, or 7 times this for iron; and let $p = 2 \pi \times 10^6$ per second.

Then, whatever the substance of the conductor,

$$L_o = 12{,}000 \text{ centimetres};$$

while the common resistance,

$$r = \begin{cases} \cdot 002 \text{ ohm for copper.} \\ \cdot 014 \text{ ohm for iron.} \end{cases}$$

Hence the effective resistance is

$$R = \begin{cases} \cdot 08 \text{ ohm for copper.} \\ 6\cdot 3 \text{ ohms for iron.} \end{cases}$$

The inertia portion of the impedance is,

$$p L_o + R = \begin{cases} 75\cdot 4 + \cdot 08 = 75\cdot 5 \text{ ohms for copper.} \\ 75\cdot 4 + 6\cdot 3 = 82\cdot 0 \text{ ohms for iron.} \end{cases}$$

The total impedances are practically the same—viz.,

$$\begin{cases} 75\cdot 5 \text{ ohms for copper.} \\ 82\cdot 0 \text{ ohms for iron.} \end{cases}$$

2. If, instead of taking a rod 10 metres long, we consider a length 100 metres long, of the same thickness, these quantities become :—

$$L_o = 162{,}000 \text{ centimetres.}$$

$$r = \begin{cases} \cdot 02 \text{ ohm for copper.} \\ \cdot 14 \text{ ohm for iron.} \end{cases}$$

$$R = \begin{cases} \cdot 8 \text{ ohm for copper.} \\ 63\cdot 0 \text{ ohms for iron.} \end{cases}$$

$$\text{Inertia part of impedance} \begin{cases} 1{,}003 \text{ ohms for copper.} \\ 1{,}066 \text{ ohms for iron.} \end{cases}$$

$$\text{Total impedance} \begin{cases} 1{,}003 \text{ ohms for copper.} \\ 1{,}067 \text{ ohms for iron.} \end{cases}$$

3. Lastly, for a wire 100 metres long, but 1 *millimetre* in diameter, the values would be Dr. Lodge.

$$L_o = 208{,}000.$$

$$r = \begin{cases} 2 \text{ ohms for copper.} \\ 14 \text{ ohms for iron.} \end{cases}$$

$$R = \begin{cases} 8 \text{ ohms for copper.} \\ 630 \text{ ohms for iron.} \end{cases}$$

Inertia part of impedance,

$$p\,L_o + R = \begin{cases} 1310 + 8 = 1{,}318 \text{ ohms for copper.} \\ 1310 + 630 = 1{,}940 \text{ ohms for iron.} \end{cases}$$

$$\text{Total impedance} \begin{cases} 1{,}318 \text{ ohms for copper.} \\ 2{,}040 \text{ ohms for iron.} \end{cases}$$

All this supposes the frequency to be determined independently of the conductor considered, and to remain the same; but as the conductor increases in length it has a tendency to decrease the frequency; and that is the meaning of my sentence in section 28, to which Major Cardew objects, "and of any "moderate length, such as 100 yards or less (not many miles)."

I ought to say that the here calculated values for R do not take into account at all the loss of energy by radiation. This will always go to increase R, often very perceptibly, sometimes enormously. I will go into this further in some other place.

These examples illustrate sufficiently well the comparative behaviour of iron and copper under well-marked and frequently occurring conditions. I have chosen the frequency of a million a second, because I have shewn reason for believing that it is not at all unlikely to apply to the circumstances of lightning; the capacity discharged per flash, and the self-induction of its path, being neither of them very big.

But while we are about it, it is instructive and quite easy to write down the values for some considerably lower frequencies: not for slow frequencies such as alternating machines give, the theory for them is more complicated, but the simple theory will do for, say, 10,000 complete periods per second. The result will be distinctly different. No longer does inertia constitute the whole of the obstruction for iron, though it still does for copper; and for iron it constitutes the largest part.

Dr. Lodge.

| Frequency, 10,000 per second. | | Resistance, R. | Inertia part of Impedance ($p L_0 + R$). | Total Impedance. |
|---|---|---|---|---|
| | | Ohms. | Ohms. | Ohms. |
| 10-metre rod 1 cm. thick | Copper ... | ·008 | ·762 | ·762 |
| | Iron ... | ·63 | 1·384 | 1·52 |
| 100-metre rod 1 cm. thick | Copper ... | ·08 | 10·11 | 10·11 |
| | Iron ... | 6·3 | 16·96 | 18·1 |
| 100-metre wire 1 mm. thick | Copper ... | ·8 | 13·9 | 13·9 |
| | Iron ... | 63· | 76·1 | 98·8 |

The depth penetrated by the current into the substance of the wires, is definite at a given frequency—unless the wire is too thin to leave a central margin, and is independent of the diameter of the wire; at least for these high frequencies. It is easily calculated with fair approximation, thus, the sectional radius of the wire being $a$:—

$$\frac{2\,\pi\,a\,d\,a}{\pi\,a^2} = \frac{r}{R},$$

whence $d\,a$, the depth effectively penetrated by the current, or the thickness of conductor practically made use of, is—

For the million per second frequency $\begin{cases} \text{in copper } \frac{1}{16} \text{ millimetre;} \\ \text{in iron } \frac{1}{180} \quad \text{"} \end{cases}$

For the ten thousand per second frequency $\begin{cases} \text{in copper } \frac{1}{1.6} \text{ mm.;} \\ \text{in iron } \frac{1}{18} \quad \text{"} \end{cases}$

It may be after all, therefore, that I am wrong in saying that rod is anything approaching as good as tape for conductors. It is nearly as good in respect of mere impedance, but whenever there is any chance of the wire being melted, then tape is far better. Rod ought to be apt to have its skin burnt off it, unless the central core has time to exert any cooling action by sharing the heat.* But it is because I doubt whether decently substantial

* The specimens exhibited by Mr. Preece, of copper wire incipiently fused by lightning internally, are interesting. They may have been fused by the dead-beat tail of a current; the outside cooling most rapidly. They *look* as if they had been hottest inside, and if so an explanation is needed; but they are not likely to upheave the foundations of electro-magnetism.

conductors are in any real danger from heat that I have asserted the advantage of greater surface to be but small. Dr. Lodge.

I wait, however, for an expression of authoritative opinion from the President on these points.

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## APPENDIX III.

### On the Melting of Conductors.

The list of fused conductors at the end of the Lightning Rod Conference Report, Appendix J, is very short, but short as it is it includes things not quite free from serious misleading. Over and over again it has been truly asserted that wherever there is an arc or a flash to a conductor damage is likely to be done. Terminals which have to receive the flash should always be thicker than the wire which has only to conduct it. This must be regarded as very ancient and orthodox, as well as very true. I now run through the short list of damage, and analyse it. The table is headed, "List of Metals Melted."

1. "Copper rod, ·35 inch diameter." This was an upper terminal, tapering from one-third of an inch diameter at the *base* to a point, and only 9½ inches long altogether. This terminal was "nearly all melted."
2. "Copper rope, ·31 inch diameter, at Nantes." Callaud, "Traité," page 89.
3. Rope, *said* to be ·7 inch diameter, at Carcassone. Callaud, "Traité," page 89.

These I will refer to directly. They were not fused, but broken, or eaten into, or otherwise "burnt by use."

4. "Iron rod, ·2 inch diameter." This was a few inches melted from the *point* of an upper terminal, and some of the links of a *chain*.
5. "Brass rod, ·2 inch diameter." This was a tapering terminal, 10 inches long, of the given diameter at the *base*, and it was only melted for one-fourth of its length.

The implied *statement* in the report is, therefore, that a brass rod ⅕ of an inch in diameter was melted. The *fact* is, that

Dr. Lodge. 2½ inches was melted off a sharp brass point! Fortunately in this case, and in case 1 also, the body of the report itself contains the material capable of overthrowing this misrepresentation.

6. "Copper rod, *perhaps* ·13 inch diameter." This was a common bell-wire, and it was legitimately destroyed, but still it protected.

That is the whole list, and it amounts to nothing more than a bell-wire, and to cases 2 and 3, the account of which I now proceed to translate from the treatise of M. Callaud. The Carcassone case is one of the two Mr. Symons quotes in his remarks (the other is case No. 4, above). It is the only one that sounds improbable, and the evidence for it seems to me weak; but I leave readers to judge. The evidence for the Nantes cases, such as they are, is perfectly good.

*Extract from "Traité des Paratonnerres," par A. Callaud, p.* 89:—"The conductor of the Church Sainte-Croix at Nantes "was a cable of red copper, a centimetre in diameter; it was "formed of seven strands, each consisting of seven wires, the "wires being one millimetre thick. I was witness of a storm and "of violent flashes which traversed it, and it showed no trace of "deterioration. This size can therefore be permitted, though it "seems to me slight. The cable which existed before that of "which I speak, also of red copper, was found broken by a flash "and damaged over a part of its length; il avait 8 millimètres. "I know of conducting bars, 5 millimetres, which a single storm "has deteriorated and eaten into in a way that ten years of rust "would hardly accomplish.

"M. Viollet-le-Duc, whose words I have had the honour of "quoting, has seen at Carcassone some cables of lightning "conductors burnt by use. Ils avaient 18 millimètres. 'In this "'town,' he tells me, 'storms are frequent—daily, in certain "'seasons.'" In such a case the size of 18 millimetres will be then "insufficient."

This last is a most vague account. The material is not specified, nor is it perfectly certain whether the 18 millimetres refer to the diameter, or whether it means that it consisted of 18 wires, each a millimetre thick. Evidently, however, M. Callaud supposes

it to mean the diameter, and most likely it does. But why in the plural? And does "burnt by use" mean anything more than that some of the thin wires were burnt or fused together, or that the cable was oxidised superficially? Dr. Lodge

Considering the exceptional character of the testimony, if understood in the Lightning Rod Conference sense, it is a pity it is second-hand.

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## APPENDIX IV.

### On Conditions under which Points can be Preferentially Struck in Case B.

Referring to Mr. Wimshurst's observation of the effect of the sign of top-plate, the following is an extract from an April note book kept by my assistant:—

"Large sphere (or dome), knobs, and point, arranged between two plates so as to be equally struck by a B spark. The plates are connected to the outer coats of the two small or pint jars, whose inner coats are connected to the machine, between whose terminals occurs a moderate A spark.

1st. *With the top-plate positive.*

| | | | |
|---|---|---|---|
| Distances of | Dome ... ... ... | 2·5 | centimetres. |
| | Large knob ... ... | 3·6 | „ |
| | Small knob ... ... | 3.8 | „ |
| | Point ... ... ... | 3·8 | „ |

2nd. *Top-plate negative.*

| | | | |
|---|---|---|---|
| Distances | Dome ... ... ... | 2·5 | centimetres. |
| | Large knob ... ... | 3·0 | „ |
| | Small knob ... ... | 3·7 | „ |
| | Point ... ... ... | 8·0 | „ |

Lengthening the A spark makes the distance at which the point is struck less."

The following measurements have been made quite recently, large jars being used, but the vigour of the rush being diminished in some cases by making the A spark (*i.e.*, the distance between the machine terminals) quite short.

Dr. Lodge. Two gallon jars similarly connected, instead of the pint jars. Objects arranged between plates to be easily and about equally struck, as before. First, with the A spark 1 centimetre long.

*Top-plate negative.*

| | | | |
|---|---|---|---|
| Distances from top-plate of | Large knob | 1·2 | centimetres. |
| | Small knob | 1·4 | „ |
| | Point ... | 2·4 | „ |

*Top-plate positive.*

| | | | |
|---|---|---|---|
| Distances of | Large knob ... | 1·5 | centimetres. |
| | Small knob... ... | 2·2 | „ |
| | Point ... ... ... | 2·0 | „ |

Lengthen A spark to 5 centimetres—

*Top-plate negative.*

| | | | |
|---|---|---|---|
| Distances of | Large knob ... | 3·4 | centimetres. |
| | Small knob... ... | 3·5 | „ |
| | Point ... ... ... | 3·9 | „ |

*Top-plate positive.*

| | | |
|---|---|---|
| Large knob ... | 4·0 | centimetres. |
| Small knob... ... | 4·2 | „ |
| Point ... ... ... | 3·9 | „ |

Repeat with A spark *about* 5 centimetres, but the B distances made greater.

*Top-plate positive.*

| | | |
|---|---|---|
| Large knob ... | 5·4 | centimetres. |
| Small knob... ... | 4·7 | „ |
| Point ... ... ... | 4·7 | „ |

*Top-plate negative.*

Distances unaltered, and all are struck occasionally as before, but the small knob gets struck rather more often than the others, and the large knob rather less often.

Hence it is clear that, under circumstances when the rush is really *impulsive*, the difference between positive and negative top-plate, which Mr. Wimshurst called attention to, does not exist. It only exists in so far as the rush is gradual.

A ballot for new members took place, at which the following were elected :—

*Foreign Member:*

Carlos Monteiro e Souza.

*Associates:*

Edward W. Cowan.
W. P. James Fawcus.
Richard O. G. Drummond.
Jesse Kemsley.
Ernest B. Vignoles.
Henry Bloomfield Vorley.
George Wilkinson.
Cecil Wray.

*Students:*

Alexander J. Protheroe.
Herbert Edward Starr.

The meeting then adjourned.

The One Hundred and Ninety-fifth Ordinary General Meeting of the Institution was held at the Institution of Civil Engineers, 25, Great George Street, Westminster, on Thursday, May 23rd, 1889—Sir WILLIAM THOMSON, F.R.S., President, in the Chair.

The minutes of the Ordinary General Meeting held on May 16th were read and confirmed.

The names of candidates for admission into the Institution were announced and ordered to be suspended.

Donations to the Library since the last statement were announced as having been received from Major Francis J. Day, R.E.; Major P. Cardew, R.E., Member; C. S. James, Member; to whom the thanks of the meeting were duly accorded.

The PRESIDENT: Gentlemen, the order of the papers has been altered from that which appears in the notice. Mr. Mordey has specially requested that my short communication be read first and the discussion on it taken before the reading of his own paper. It is a very kind suggestion on Mr. Mordey's part, and as he has expressed the desire that it should be so, I accept the proposal, and beg to thank him for it, although I should be exceedingly sorry if any such change in the order should in the slightest degree interfere with the communication of his paper to the Institution. It must not, in fact, be allowed to do so. Mr. Mordey's paper is a most important one, on a subject of vital interest to electrical engineers, and it is necessary that it should be thoroughly and carefully read and listened to, and that there should be as much time allowed for its discussion as circumstances permit.

Whatever discussion there may be on my communication must therefore naturally be very short, and I hope we may so arrange it that we may not occupy more than half an hour of the time of the evening.

I shall now, without further preface, read the following paper:—

## ON THE SECURITY AGAINST DISTURBANCE OF SHIPS' COMPASSES BY ELECTRIC LIGHTING APPLIANCES.

By Sir William Thomson, D.C.L., F.R.S.S. (L. & E.), President.

The danger to be avoided is sufficiently explained in the following short statement by Mr. William Bottomley, which appeared in the *Nautical Magazine* for December, 1885:—

"The following example of a case which might occur in any "large ship, will show the amount of error which may be pro-"duced on the compass" [by the electric lighting apparatus] "unless precautions are taken to guard against it.

"Suppose a main lead from the engine-room to the fore part "of the ship, to light up 100 lamps, is brought along the centre "of the ship. It may be at a distance of 10 metres, or 33 feet, "from the standard compass, and will run almost underneath it. "If we suppose that each lamp takes one ampere of current "there will be a current of 100 amperes altogether in this lead. "Now, the effect" [of an infinitely long straight current] "on the "compass" [above it] "at a distance D in centimetres is given "by the formula

$$F = \frac{2 \times \frac{1}{10} C}{H D}$$

"where C is the current in amperes and H is the horizontal "magnetic force. In this case we have $C = 100$ amperes and "$D = 1{,}000$ centimetres. Therefore

$$F = \frac{20}{1{,}000\ H} = \frac{0{\cdot}02}{H}.$$

"At Glasgow the horizontal force may be taken as 0·15 in c.g.s. "units, therefore the effect on the compass will be $\frac{\cdot 02}{\cdot 15} = \frac{1}{7{\cdot}5}$. "This will be expressed in degrees by multiplying by 57·3, the "number of degrees in the radian, or angle subtended at the "centre of a circle by an arc equal in length to the radius.

" Therefore, the amount of error produced by such a current on " the compass will be $\frac{57 \cdot 3}{7 \cdot 5} = 7 \cdot 6$ degrees.

" The foregoing refers to a single wire and a continuous " current machine, but if an alternate current machine is " employed no effect will be produced on the compass even when " the ship's side is used for the return. When a continuous " current machine is used, the danger of producing an error on " the compass can be avoided by using two wires close to one " another, but these wires should be well insulated from the " ship's side. If in any way one of the wires is brought in " contact at two points of its length with the iron of the ship " there may be no change observable in the lighting, but the " current may produce as much error on the compass as it " would if there was only a single wire.

" The following points should therefore be attended to in all " cases of lighting ships by electricity:—

" First.—With continuous current machines two wires, well " insulated, should always be employed.

" Second.—The insulation of the wires should be tested " periodically; if any connection with the iron of the ship is " found, the fault should at once be corrected.

" Third.—When an alternate current machine is used, a " single wire may be employed and the iron of the ship used " to complete the current without producing any effect on the " compass.

" What makes this question of the greatest importance is that " the error may be produced without ever being detected by the " officers of the ship. On board ship the errors of the compass " are usually determined during the day, in the morning and " afternoon, but the electric light is only used at night. The " captain may therefore carefully determine his errors every day, " and set his course quite correctly; but at night, when the " electric light is turned on, the ship may be going several " degrees off her proper course, although she is being correctly " steered by the compass.

" In connection with the lighting of ships with electricity,

"there is another point which should also be attended to—that "is, the position of the dynamo. If it is placed near an iron "bulkhead, the upper end of which is near the compass, the "bulkhead may become magnetised by induction so powerfully "that it will produce a considerable error on the compass."

The subject was also referred to in Mr. Bottomley's paper on "The Magnetism of Ships and the Mariner's Compass," read before the Society of Arts, January 28th, 1886, and published in the Journal of the Society for February 5, 1886. In the discussion which followed, and in which Captain Creak, of the Admiralty Compass Department, Mr. Alexander Siemens, and Dr. Hopkinson took part, it appeared that in three ships, lighted on the single-wire system with direct currents, small but not unimportant errors in the compass, due to the lighting currents, had been actually observed. Since that time several cases have been reported to me of large passenger ships, lighted with direct currents on the one-wire system, in which as much as 4° or 5° of error on the compass has been produced by the electric lighting. In the latest of these cases, a few weeks ago, an error of 4° on the North course was found when the light was put on. The light was put on and off several times with the ship's head North, and every time the same error was produced.

The precautions for security which I have to suggest are—

1. The use of the two-wire method exclusively (unless, which is now rarely the case, alternate currents are used).

2. The most simple and convenient test for faults of insulation capable of disturbing any of the compasses on board is a lamp set up in the neighbourhood of the dynamo, with one end permanently connected with the ship's iron, and a switch for readily putting its other terminal in connection with either of the dynamo mains at any time. The switch should occasionally be moved each way by the engineer in charge, and if either motion lights the lamp to any visible degree, a defect of insulation on the corresponding main is proved, and ought to be immediately corrected. But unless the lamp is lighted to full brilliance, the fault is certainly not so great as to sensibly disturb a compass. Full brilliance proves only one fault; and there must be more

than one such fault before any error of practical importance can be produced in any of the compasses.

3. Care that there is not magnetic "leakage" from the dynamo (as practical men, guided by Faraday's ideas, theory, and language, have now taught the scientific world to call it) enough to produce any compass-disturbance of practical moment. Capt. Creak, speaking at the beginning of 1886, in the Society of Arts discussion previously referred to, said that in one ship the direct compass-disturbance produced by the generating machine was "felt through a distance of 55 ft.," and across iron bulkheads, and that it was perceptible also in other ships of the Royal Navy electrically lighted on the two-wire system.

My impression is that the improved dynamos now made have much less of magnetic leakage than those made prior to 1886, but we still want information as to their disturbing magnetic effect at such distances as have to be considered in connection with the compass question.

4. To ascertain that there is no perceptible compass-disturbance, or if there is any to test its amount, the compass should be observed while the current through the dynamo is started and stopped, either by starting and stopping the dynamo itself, or by making and breaking the circuit of the field magnets. This should always be done before the electric light installation is taken over from the contractors. It is best and most easily done when the ship is in dock, or lying steadily at anchor. On no account ought it to be delayed, in a new ship, till she goes out for compass adjustment. A determination of the amounts of the disturbance, if any, for all courses of the ship can be made by aid of my deflector without moving the ship. But a sufficient practical test may be made by first observing the effect of starting and stopping the current, on the compass as it stands; then adjusting a small magnet placed on, or supported a little above, the glass of the bowl, to deflect the compass about 45° first on one side and then on the other side of its undisturbed position, and in each case observing the effect of starting and stopping the electric current. This effect ought not to be as much as 2° in any of the three cases.

5. A small electric lamp, with its two electrodes insulated and twisted together in the usual manner, may safely (and with very great advantage in most cases of electrically lighted ships*) be used to light the compass. The effect, if any perceptible, of its current on the compass ought to be tested in the manner described in No. 4.

Staff-Commander CREAK, R.N., F.R.S.: I am very glad to hear what Sir William Thomson has told us on the subject of the effects of single-wire leads and dynamos on compasses in electrical installations on board ship. I can only speak from experience as regards our men-of-war, where the electric light has been adopted for some years past. We have only one ship—the "Polyphemus"—fitted with single wires; in all other vessels the wires are duplicated, the lead and return wires being close together and led along the ship's side with double wire branches to the lamps. There has never been any trouble caused by the wires—our troubles have been caused by the dynamos. As an instance, I may mention the "Northampton," where three dynamos of large external field were fixed about 37 feet from the standard compass on the bridge. Near this latter were two of Sir William Thomson's compasses, and on the deck below two steering compasses.. I made experiments for horizontal and vertical force, as well as notations of the angular deflection of the compasses. With all three machines running, a deflection of 3° to 5° was observed at the most distant positions on the bridge, and as much as 11° was observed at the steering compass. These experiments distinctly pointed to the dynamos as the cause. These machines were unfortunately placed with their red poles uppermost. The effects of each machine in deflecting the several compasses was next tried. First, No. 1 on and off, then No. 2, and lastly No. 3 on and off. The greatest deflection produced by any one machine was 6°—with all three on, 11°. Besides the "Northampton," two other ships have given trouble—the "Curlew" and "Landrail,"—all distinctly traceable to the dynamos, and either from their being too near or their poles Staff-Com. Creak.

* Those who know the trouble of keeping the best of ordinary compass lamps alight, for the steering compass on the bridge of some of our cross-Channel steamers in a winter gale, will appreciate this advantage.

Staff-Com. Creak.

being placed in wrong relation to the compasses, or from the machines being of unsuitable type for ships. There are machines the external field of which is so small that it may be disregarded. In the "Northampton," the officer of the watch could never tell whether the engineers below were running one, two, or three machines; whilst he had three deviation tables to consult, according to the number of dynamos at work. This state of things would be intolerable in any weather, much less in fogs, and the machines are to be removed from the vessel in consequence. There is nothing to prevent an installation of the electric light being made on board perfectly harmless as regards the compass, if suitable dynamos and double wires are used.

It is a great pleasure to me to find that I have the authority of Sir William Thomson to confirm much that I have said, knowing that we must all gladly defer to his opinion as one of our highest authorities in these matters.

Mr. Siemens

Mr. ALEXANDER SIEMENS: Mr. President, I am very glad that you have brought this subject forward so that we can discuss it here, because I need not tell you I am a strong advocate of the single-wire system, and the experience of my firm has been that hardly any trouble has ever been experienced. This is principally due to our having carried out paragraph 4 of your paper. It has been our habit always to try the effect of the lamps and the dynamos on the compasses, and in most cases there has been no effect at all. In fact, in all our practice, which extends over a number of years, and includes a great number of ships, we have really had only one single case in which any effect was produced on the compasses, and I have brought with me a copy of the letters which passed between my firm and the shipping company at the time, because I thought it would be best to submit the matter as it was discussed at that time.

The shipping company wrote to us the following:—"With "reference to the visit of your Mr. Siemens to the steamer to-"day, and to the conversation with him on the subject of the "effect produced on the standard compass of the ship by the "electric light currents in the companion below it, we shall be "glad if you will be good enough to write us a full letter

Mr. Siemens.

"detailing the cause of this occurrence (the only one which has "yet come to our notice), and the re-arrangement of the wires "which you propose to make in order to do away with the "disturbing effect complained of. We shall be glad at the same "time to have an expression of your views with reference to "Mr. Bottomley's letter to the *Nautical Magazine* for the "current month, on this subject." That is to say, the letter which you, sir, refer to in your paper.

We answered as follows:—"We beg to say that on Tuesday "last we made experiments on this ship, and were unable to "find any deflection of the compass needle due to the electric "light currents, as the ship was then lying—*i.e.*, with her head "pointing 63° to the west of north; but it is possible that when "the ship is otherwise positioned there may be some disturbance, "although our experience in fitting some hundreds of ships on "the single-wire system has not brought any such instance to "our notice.

"We believe the cause of the observed deflection to be the "position selected for the compass with respect to the wires, and "we have offered to re-arrange the wires so as to neutralise their "effect;"—[I should say that the compass was put up after the wires had been fixed, and we had no knowledge that the compass was to be there]—"but the captain wished us not to do so before "the ship sailed this time, in view of the disturbance to the "ship's woodwork our work would involve, assuring us that he "anticipates no inconvenience from this effect on the compass if "it be found to be permanent and a constant quantity. We "have, therefore, taken no further steps in the matter."—[I may say from that day to this—that is, during four years—we have not heard any further complaints, although we make it a practice to send one of our assistants on board each ship which has been fitted by us with electric lights, as soon as she returns to harbour, to ask of the engineers, or those who have had charge of the electric lighting apparatus, what complaints they have to make. And in this way any effect on the compasses would have been brought to our notice at once.]—"We have carefully perused "Mr. Bottomley's letter in the *Nautical Magazine* for this

Mr. Siemens.

"month, and whilst we are not disposed to question the correctness of his calculations, we must point out that the example he gives is one not likely to occur in practice, for he takes no account of the fact that a wire carrying as much as 100 amperes on board ship would be laid between decks, and the compasses would therefore be shielded from its influence by the iron deck, bulkheads, and other adjacent masses of iron, which do not enter into Mr. Bottomley's calculations. Moreover, the neutralising effect of other wires (branches and duplicate mains) has been neglected by him.

"We shall be interested in any reports you may be pleased to lay before us on the effects on the compass which may be observed during the ship's first voyage, that are traceable to the electric lights"—and, as I said, we have had no further reports about this particular ship at all. This has been the only case where an effect on the compass was observed in a ship fitted by us; and if we had known that the compass was to be there, we should simply have put up a return wire in the vicinity of the compass, so as to neutralise the effect of the current.

The second case of a compass disturbance which was brought to our knowledge was by the same shipping company, who wished us to inspect a ship which was fitted by another firm, and I made a personal report upon it as follows:—

"As requested verbally, I went yesterday on board your steamship to investigate the effect of the electric light current on the compasses.

"1. I observed the positions of the compass in the steering house, and of the standard compass before the electric light was started, and took further observations—

"2. With all lights on;

"3. After three lights in the music saloon, immediately under the steering compass, had been turned out;

"4. After three lights in the companion way had been turned out;

"5. After two lights in the saloon had been turned out;

"6. After putting on the two lights in saloon;

"7. After putting on the three lights in the music saloon;

Mr. Siemens.

"8. After putting on the three lights in the companion way;
"9. After turning out all lights except those on night circuit;
"10. After turning out all lights;
"11. After turning on again the upper lights;
"12. After turning all lights on again.

"The effect of all these changes on the standard compass was "hardly to be noticed, and was certainly less than half a degree; "a pretty strong wind was blowing at the time, and this may "have had as much to do with the changes as the electric "current.

"The effect of turning on all lights was to cause a deviation "of about $1\frac{1}{2}°$ in the steering compass, but after all lights had "been turned out again the compass did not quite return to the "first position.

"After these observations had been taken, the ship's head was "swung out 20° and a similar set of observations taken.

"The effect on the standard compass was again hardly "noticeable, and the steering compass showed not more than "half a degree variation.

"The turning on and off the lights under 4 seemed to have "some effect; but as all the variations were excedingly small, and "not clearly traceable to the electric light current in any par- "ticular circuit, I am hardly in a position to suggest an alteration "in the electric light installation.

"The captain was good enough to show me some deviation "cards obtained during the run from Greenock, which seemed to "indicate that the greatest error was caused when the ship was "running on a southerly course.

"The difference in the positions 1 and 10 seems to indicate "that the 'permanent' magnetism of the ship has not yet "assumed its permanent value; and, under the circumstances, "observations during day time with the lights on and off will be "an efficient safeguard against errors of the compass, especially "as the effect on the standard compass is really insignificant."

I may say that as long ago as 1879 we made an experiment by putting a dynamo on the deck of our cable-ship and running it as a motor, and we found that if it was close to the bridge it had

Mr. Siemens. a strong influence on the compass; then we moved it more and more away, and at a distance of 50 feet from the compass we found there was no effect whatever.

I wanted to call your particular attention to the statement made by Captain Creak, that with the "Polyphemus," which is the only ship in the English navy fitted on the single-wire system, there has been hardly any complaint about trouble with the wires, whereas there have been several cases of disturbances—apart from compass disturbances I mean—on the other ships which are fitted on the double-wire system.

Professor Forbes. Professor GEORGE FORBES: I desire to draw attention to the fact, that the compasses were partially screened from the dynamos by a $2\frac{1}{2}$ inch iron deck; that the dynamos were, in fact, actually in an ironclad chamber. On examination of the dynamos, the polarity which caused trouble seemed to come from imperfect joints in the yokes of the magnets, and I consider that the same type of machine could now be made more satisfactorily, so far as deviation of the compass is concerned.

Professor Jamieson. Professor A. JAMIESON: Mr. President and gentlemen, shortly after the appearance of Mr. Bottomley's letter in the *Nautical Magazine*, I spoke to him of the case of the s.s. "Bombay," which was being fitted with the electric light on "the single-"wire system," under my inspection. He cautioned me against the probability of the current affecting the compasses, and kindly arranged that, when the ship was being swung for the adjustment of compasses, I should have every opportunity of observing the results produced by turning on and off the lights, as described by Sir William Thomson in his paper. The results were that the steering compass deviated about 9°, and the standard compass (Sir William Thomson's) about $1\frac{1}{2}$°, in the position most affected by the electric light current.

The PRESIDENT: The engine running all the time?—the circuit through the field magnets being continued all the time?

Professor A. JAMIESON: Yes; and when the lights were switched off, the compasses came back to their natural positions.

The PRESIDENT: Was that due to the field or to the electric light currents?

Professor A. JAMIESON: It was due to the electric light current alone.

Professor Jamieson.

The PRESIDENT: The field magnets having been kept on all the time?

Professor A. JAMIESON: Yes. By-the-bye, the steering compass was only about 4 or 5 feet away from the current-carrying wire, which, if I remember rightly, had some 25 amperes passing through it when all the lamps were on that circuit, whilst the Thomson compass was about 12 or 14 feet away from it. If I remember correctly, double wires (+ and −) were run close together for that part of the circuit close to the compasses, and the evil effect was almost nullified, at least it was imperceptible on the standard compass, and so little on the steering one that the captain said it was not worth minding.

I have not met with any serious compass errors due to the magnetic field of dynamos alone, the fact being that the dynamos which I have had to do with were always placed low down in the engine-room, and consequently in a position far removed from the compasses.

Mr. J. S. RAWORTH: I should like to corroborate what Mr. Alexander Siemens said as to the practical result of wiring ships on the single-wire system, having been associated with it from the commencement. I may say that in the early days, when the first ships were fitted up, captains and owners were very much more particular than they are now. They are becoming quite used to the electric light, and perhaps they are not so much on the look-out for defects as they were in the first electric-lighted ships; and, except in the one instance which Mr. Siemens has mentioned, I have never heard of a single case where any ship's compasses were affected. The one which he has mentioned is one that I was also connected with. It was a very remarkable case, and I think I could throw a little more light upon that question than even Mr. Siemens did, because I happen to know exactly how all the wires in that ship were run. It so happened, which is a very common thing on board ship, there were two side-passages, and these, when they came to the fore part of the ship, converged in the form of the letter V, and formed one central

Mr. Raworth

Mr. Haworth. passage, and the main wires on each side of the ship came along these two side passages, and then turned towards each other, towards the apex of the V; and I was very much puzzled for a long time to find out what was the cause, which wire it was that affected the compass, and I found at last that only one wire was affecting the compass in the position in which the ship was then lying, and that turned out to be the wire which was lying very nearly parallel with the compass needle. The compass needle was about half-way across the ship, and parallel with one side of the V: when you turned on the current on the one side of the ship the compass needle went over about $1\frac{1}{2}°$, and when you turned the current to the other side of the ship you scarcely got any effect whatever. Then we put some biassing magnets to throw the needle over on the other side, when the conditions were exactly reversed: the starboard wire affected the needle, and the other had no effect whatever. It so happens that in this ship there was a concurrence of structural peculiarities, which is scarcely ever met with: a huge gap was cut out of the two iron decks, and there was no intervening screen of iron between the conductors and the upper deck where the compass was placed. It was so peculiar and so abnormal that one never expected to find it, and it was only after close examination that I discovered that such was the case. The steps I took to rectify the mischief were these: I took a 3-inch wrought-iron pipe and laid it on each side of the ship along this V-piece, and for a short distance beyond the V both fore and aft; and the effect of these pieces of wrought-iron tube was to so far cure the evil that the ship-owners said that there was no error left which was worth taking any notice whatever of. That simple expedient was sufficient, not quite to cure it,—for I am quite willing to confess that there was just a trace left,—but it was so far cured as to certainly leave only one-half a degree deflection, and the owners said there was no necessity to give any further attention to it. That case and the one mentioned by Mr. Siemens are, so far as I know, the only instances we have had in fitting up passenger steam-ships on the single-wire system.

Major Cardew. Major P. CARDEW, R.E.: I should like to refer to the question

of using an electric lamp to light the binnacle. I have had experience of trying that, and perhaps it might be worth just mentioning. One night I had put a few accumulators on board a little yacht: my friends were very anxious to try an electric lamp in the binnacle, because in a small yacht the binnacle-lamps are a great nuisance, and so we rigged up a little lamp, and I carefully twisted the wires together, for of course I thought of the compass, and the lamp was placed just above it. I never thought of the filament of the lamp, it quite escaped my notice—I mean the effect of the current in the filament of the lamp itself. It was my first watch below. I took them out of Dover Harbour and then turned in; they were working away with a head wind, and were tacking about in the Gull passage and taking observations of the electric lamp, more with the view of testing how the compass worked than anything else. I was not asleep, and could hear much talking as to why, when a tack was made, there was a terrible difference with the compass. It flashed across my mind what it was; I went on deck, turned the lamp out, and they had to resort to oil again. Major Cardew.

The PRESIDENT: With reference to Major Cardew's very important remark, that precaution must be used in attempting to light compasses by an electric lamp, I may say that it is easy to shape the filament so that its magnetic moment, with the current through it, shall be insufficient to produce any sensible disturbance on the compass, however the lamp is placed outside the bowl, for convenience of lighting; and this with a lamp amply powerful enough to light the compass. I do not know what was the candle-power of the lamp referred to by Major Cardew. Sir William Thomson.

Major CARDEW, R.E.: It was a low candle-power lamp, with the filament just above the compass, and on the course we were steering the straight portion of the loop certainly affected the magnet.

The PRESIDENT: Was it a 16 c.p. lamp?

Major CARDEW, R.E.: Oh no; it was about a 5-volt 5-candle lamp. It was a very small binnacle, and we had it pretty near, so as to give a good light, but it distinctly did affect the compass.

Sir William Thomson.

The PRESIDENT: The proper arrangement of filament for a binnacle-lamp is to shape it like a hair-pin, with its two sides not more than half a centimetre or a centimetre apart. Suppose, for example, a 50-volt 6-candle lamp (which is a more than amply sufficient light to steer by), the filament would be about 6 centimetres long, giving area 3 square centimetres, and magnetic moment, when excited by $\frac{2}{3}$ of an ampere through it, $\cdot 2$ c. g. s. The maximum magnetic force of this at a distance of 10 cm. (and it could hardly be placed nearer, even for the smallest yacht compass!) is $2 \times \cdot 2 \times 10^{-3}$, or 1/2500, which could not disturb the compass by more than about a tenth of a degree in these latitudes.

Professor G. FORBES: Might I ask, Sir William, whether it is quite certain that an alternating current may not demagnetise a compass to a partial extent?

The PRESIDENT: It is, I think, quite certain that, with any practical arrangement of the wiring, it cannot. Regarding the magnitude of the disturbance produced by the one-wire system with direct current, I may say that, although Mr. Bottomley's illustration was a rough and ready example of an extreme case, you have only to vary the figures. Take 150 amperes instead of 100 amperes, or take 60 amperes instead of 100 amperes, and take 20 feet instead of 30 feet; vary it about, and instead of an infinitely long wire, which is convenient for calculation, take any actual length of wire concerned in any particular case, and the well-known formulas will show you that the effect on the compass is practically very considerable. But I must say that theoretical calculations of this kind are mere examples of what are possibilities. If the calculation of such an example as that shown in Mr. Bottomley's calculation gave only 2° or 1° for the greatest possible disturbance, then we might rest contented that in no practical circumstance would it be very serious. All we can do by theoretical examples of that kind is to let us know before we go into iron and steel and compasses and ships, before we go out of the laboratory or the workshop—to let us know what can be expected as a possible disturbance. If we know that the greatest possible disturbance is insensible, we may be satisfied; but if we know that the disturbance can be considerable, then

experience alone can tell us whether we may neglect the thing in any particular case, or agree to neglect it in general, or not. Now I must say, as a practical matter, that in the first place it is better to avoid a disease altogether than to let a disease be produced and then to find a remedy for it; and in the next place I would say, with reference to proposed remedies, the doctor's bill for curing the disease is liable to be much more expensive than adopting the arrangement in the beginning by which the disease can be prevented, and *the cure is essentially imperfect at best, after all that can be done short of almost complete re-wiring.* Sir William Thomson.

By a troublesome and expensive shunting or doubling of wires in a part of the ship, you may annul the disturbance on one particular compass, but then there is another compass and *another* compass, all three incessantly used in the navigation of the ship. It is practically not possible to arrange the mains on the 1-wire system so that there is no sensible error on one or other of the compasses. Mr. Siemens referred to an error of $1\frac{1}{2}°$. I do not wish to complain too much of $1\frac{1}{2}°$, but still I think every sailor will agree that I am right in saying that you had better not have $1\frac{1}{2}°$ error if you can avoid it; and any arrangement of wiring that could produce an error of 3° or more is not to be tolerated.

Mr. ALEXANDER SIEMENS: I think you ought to take into full consideration that that is the only case, out of hundreds of ships, where any compass in any position of the ship was affected.

Captain CREAK: May I ask Mr. Siemens if he remembers the P. and O. ship "Oceana"? Her steering compass was affected to the extent of something like 8°.

Mr. ALEXANDER SIEMENS: You can easily arrange the wires badly, of course.

The PRESIDENT: I am perfectly aware that there are many cases in ships at present at sea, such as the last referred to by Captain Creak. I did not care to mention the names of ships or Companies, but I know many cases in which there are errors of 3°, 4°, and 5°, undoubtedly due to the electric lighting. Now I would remark that it is not at all satisfactory to have a changing error in the steering compass, although the standard compass

Sir William Thomson. may be unaffected. The ship is steered by the steering compass. An officer in another part of the ship looks frequently at the standard compass, and if he finds the course of the ship is wrong he passes an order or a caution to the steersman; but it is exceedingly inconvenient if the officer in command, having known that his steering compass *was* all right at a certain time, should at some uncertain time—when the saloon is lighted up for No. 2 passengers' dinner, for example!—find the ship off her course 2°. He does not know whether it is careless steering or an error in the compass, and it may take ten minutes to find out which it is that has caused the ship to go off 2°. That is an intolerable state of things. Anything that introduces errors at all adds to the complication, great enough and perplexing enough as it is, that already exists, whether with the officers, the watch, or the men steering, and should if possible be avoided; and if the electric lighting of the ship could not be done otherwise than by methods which introduce errors of from 2° to 5° at uncertain times in different parts of the ship, it would be a serious question whether the electric lighting should not be given up altogether, or the captain and officers of the ship should make up their minds to pay careful attention to it and look out for the changes. To depend upon the steward sending a message that he is going to light up a cabin or part of the ship would be a very inconvenient state of things.

It seems to me that if the one-wire system is to be used at all, it ought to be obligatory to use only alternate current with it. If direct currents are used, the two-wire system alone ought to be admitted on board ship. Electricians must not suppose that if sailors do not complain there is nothing to complain of. In the first place, sailors do not always know that they have suffered from the error. Many a man has been steering for several hours, and has never imagined that his compass had been disturbed owing to the lighting of the ship. Even a thoroughly careful man may not have discovered that it was the lighting that had caused some disturbance which he may have noticed in his reckoning.

I have occupied your time too long, but I would just, in

conclusion, beg the Institution to consider, as far as the influence of its members is concerned (and I hope my friends will forgive me for being so urgent), whether it would not be better to adopt the two-wire system universally in ship lighting. Sir William Thomson.

On the motion of Mr. SPAGNOLETTI, seconded by Mr. PREECE, a hearty vote of thanks was unanimously accorded to the President for his communication.

Mr. W. M. MORDEY: As I am afraid, Sir William, that I may not get through the whole of my paper, I should like, in the first place, to express my thanks to the Brush Corporation for very kindly sending over the apparatus and the diagrams that are before you. I should also like to say how much obliged I am to my brother officials at the Brush Corporation for all the assistance they have given me.

I propose, with your permission, to read the first portion of the paper, and then to condense the remainder, and miss several sections out altogether. I would therefore ask those gentlemen who do me the honour of discussing the paper, that they should read the sections that are missed out, before making their remarks.

The following paper was then read:—

## ALTERNATE CURRENT WORKING.

### BY W. M. MORDEY.

I wish to state at the commencement that this paper is not intended as a contribution to the comparison of the relative merits of alternate-current working and of any system of direct-current supply. The discussion of this question has already taken place, and those who took the A.C. side have no reason to be dissatisfied with the result. It may, however, be pointed out that exclusive advocacy of any one particular method of working, as being the best for all purposes, is not a position that is likely to be taken by any electrical engineer who has any extensive acquaintance with the various requirements and conditions that are met with in practice.

## Working Alternators Parallel, and Best Principles of Construction for Alternators.

This is a matter of very great scientific interest, and of still greater practical importance. It is not too much to say that the complete success of the transformer system of supply depends to a great extent upon whether alternators can be quite easily and successfully worked parallel. This has been strongly insisted upon, especially by the opponents of that system. The importance attached to it arises partly from the fact that the most economical method of supply is that of using always the smallest plant that will do the work, and increasing or decreasing the number of generating units in operation, according to the fluctuations of the demand. To do this most conveniently, the alternators should be worked parallel, and ought to be capable of being put in and out of circuit easily, and without causing even a momentary flicker or interruption of the light. The use of a large machine and engine for the small day supply is especially to be avoided. In almost all cases it is desirable to have a comparatively small set of plant for this work, even where large machines are used for the heavy evening work.

Another argument in favour of parallel working, as against the use of very large machines, is that it reduces the cost of the spare plant. Thus, if a station is provided with one or two machines and engines for the full load, the spare plant is equal in cost to the working plant, or perhaps to one half as much. If, however, smaller generating units are employed, one set in four may be considered a safe allowance.

The history of parallel working of alternators may be briefly sketched.

In 1868 Wilde described parallel working and synchronous action of generators, and so nearly obtained synchronous motor action that it is extraordinary he should have missed it.*

In 1882-3 Dr. Hopkinson,† not knowing of Wilde's work,

* "On a Property of the Magneto-electric Current to control and render Synchronous the Rotations of the Armatures of a number of Electro-magnetic Induction Machines."—H. Wilde, *Phil. Mag.*, Jan. 1869, pp. 54-62.

† *Proc. Inst. C.E.*, 1882-3.

arrived theoretically at the conclusion that it was possible to work alternators parallel, but not in series, and subsequently demonstrated the fact by trials made, in conjunction with Professor Adams, on the De Meritens machines at the South Foreland Lighthouse, which were run parallel and as motors. This was all laid before this Society at the time, and was very fully discussed.*

In the course of this discussion in 1884, Mr. Alexander Siemens described some experiments showing that the Siemens alternate-current machine ran as a motor.† This is interesting as being, so far as I know, the only recorded instance of an alternator without iron in the armature being run as a motor. It will be remembered that the Wilde and De Meritens machines had iron cores. These experiments of Mr. Siemens were not very successful, the motor frequently stopping suddenly, even when doing very little work; but they were of value for comparative purposes.

Passing over the next few years, we find that the use of alternators had become of great importance on account of the growth and development of the transformer method of distribution, and that there was and is a certain amount of doubt and hesitation about working parallel. It is recognised that it has been and can be accomplished, but that the arrangement is not one to be thoroughly and completely depended upon in every case. And there is sufficient justification for this doubtfulness. The present opinion may be fairly stated as follows:—Alternators may be successfully run in parallel if they have a good deal of self-induction, and to secure this it is better that they should have iron-cored armatures.

In his recent paper on "Alternate Current Machinery," before the Institute of Civil Engineers,‡ Mr. Kapp dealt at considerable length with this part of the subject, and his views, I need not say, may be accepted as quite correctly representing the current state of knowledge and opinion. Referring to alternators that have self-induction negligible, Mr. Kapp says:—" Machines of

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* *Journal*, xiii. (1884), pp. 496-559.

† *Journal*, xiii., p. 525.

‡ *Proc. Inst. C.E.*, Feb. 1889.

"this type can only be run in parallel if the strength of their "field is adjusted with almost mathematical precision, and as this "would require more skill and attention than is available with the "ordinary staff of a central station, such machines are practically "unfit for parallel working. To make them fit for this method of "working, either the armature resistance or the self-induction "must be increased. An increase of resistance in order to be "effective, would have to be so considerable as to seriously "prejudice the electrical efficiency of the machine, and this "expedient may therefore be dismissed as impracticable. The "other plan of increasing the self-induction is not open to the "same objection. It has the effect of lowering the plant effi- "ciency, but its influence upon the electrical efficiency is only "indirect, and so small that it may be neglected. From the fore- "going, it will be readily seen that the only and sufficient condi- "tion for successful parallel working is a sensible amount of self- "induction in the armature circuit. If the armature itself does "not possess the quality in a sufficient degree, a choking coil of "suitable self-induction must be inserted into the circuit of each "machine. The results here arrived at, by a mere theoretical "investigation, are entirely borne out in practice. It is well "known that alternators having no iron in their armatures cannot "be run in parallel, except by the adoption of some such "expedient as choking coils; also that parallel running is feasible "with those alternators which have iron-cored armatures, and "then with different degrees of security."

Now this is a very serious state of affairs, for it really means, if true, that the only machines suitable for central station supply are those that, on account of their high self-induction and resistance, are bad regulators that will not bear any considerable change of load without varying a good deal in E.M.F., and that are wasteful on open circuit.

For my part, I have seen enough of the use of iron in armatures to wish to do without it if I can, and I have repeatedly pointed out that, if iron is really necessary, the armature is the very worst place to put it. For the purpose of increasing the self-induction it can be much better used outside of the machine,

and put in some place where it is not subject to all the losses, restrictions, and disadvantages that necessarily accompany its employment in the core of an armature. If it is not in the armature it can be entirely removed when not required for the purpose for which it is thought to be necessary—that is, when only one machine is in use. To put iron in an armature merely to increase self-induction appears to show a want of common ingenuity and an absence of the sense of proportion.

But let us examine the evidence on this matter, in order to see what basis of fact there is for the present views.

In the first place, it will be found that the use of iron armatures is not in all cases to be relied on to give successful parallel working. In the discussion last year, Mr. Gordon, who has had very considerable experience, and who used iron largely in his machine, warned us in strong terms against working in parallel. He said:* "We know that experiments have been "made by coupling a number of small alternate-current machines "together, and at the South Foreland they were successful, "but that was because they were working on arc lamps." A little elucidation, perhaps, is necessary here—"Many of us have "tried them, and they will, on trial, work together, no doubt, "but they do not work together till they have run for three "or four minutes; they will in that time jump, and that jump-"ing will take months of life out of the 40,000 lamps. That "alone is rather a serious difficulty in coupling machines "together, and I think we may take it in practice—I am not "speaking about the laboratory or experiments—we do not "a couple machines."

The Zipernowski alternator will, however, work parallel, but apparently not very well; to get it to do so the periodicity has had to be reduced to 42, as we now know.†

Further, I have recently seen a communication from the highly experienced makers of a well-known alternator in which iron is used, stating that, if it is desired to run parallel, the machines, engines, and arrangements must be in every way

* *Journal*, xvii., pp. 195-6, Feb. 1888.
† See under "Periodicity," p. 599.

identical: the impression conveyed being that with this machine the greatest care is necessary to ensure a successful result.

Then, again, the large experience of the Westinghouse Company in the United States has shown that their iron-cored alternators will work in parallel under some conditions. We are told that, with these machines, "parallel coupling always succeeds "when they are loaded to about half their maximum output or "over, but that machines working under a less load cannot with "certainty be so coupled."

But the other day Professor Forbes described* successful parallel working with the iron-cored Lowrie-Parker alternators at West Brompton, even with very light loads.

Thus it will be seen that we have evidence ranging from complete failure to complete success with iron-cored machines.

It is proved, then, that the presence of iron is not alone sufficient to ensure success, for the use of iron is associated equally with failure and with success.

That the success attained is complete only as regards the fact of synchronism, is shown by the following extract from Professor Forbes' remarks in the discussion on Mr. Kapp's paper, which I quote at some length, because Professor Forbes has taken every opportunity of ascertaining, by personal observation, what is being done, and because he gives us what is alone of much service at the present stage, viz., the independent record of actual facts, and tells us clearly what the real difficulties are.

The passage is†:—

"The question of parallel working was one which had been very much dis-"cussed as to whether it was desirable or possible. . . . It was always possible, as "Mr. Kapp said, with machines which had a very high self-induction—machines in "which the armature had a great mass of iron in it. At the same time, the introduction "of that self-induction into a machine reduced what Mr. Kapp called the plant-"efficiency of the dynamo-machine; and if they could work out, as he believed "engineers would work out, some better means of making machines work in parallel, "it would be very desirable. Another reason why the heavy self-induction was in-"jurious, was that it required such continued attention on the part of those in the "station to see that the pressure was maintained constant. The electrical pressure

* Board of Trade Enquiry. April, 1889.
† *Proc. Inst. C.E.*, Feb. 19, 1889.

"varied so much, that the quantity of current being developed, as shown by the table "of curves, fig. 11, was seriously affected, and constant attention was required to keep "such a machine regulated to the right pressure; whereas a machine which had no "self-induction, or very little, gave off the same electric pressure, or nearly so, what- "ever the current might be, so long as the speed was maintained constant. As to the "possibility of working conveniently in parallel with those machines, he might say "that the experience of America had been completely against it. It was there found "that it was possible to work in parallel, but that it enormously increased the amount "of skilled attention required in a central station."

This is a very strong condemnation of the present system of obtaining synchronism, and at the end of his "Central Stations" paper* Professor Forbes returns to the subject in these words:—

"I venture to think, however, that the plan now universally proposed for "making machines work in parallel will not long be tolerated. This is to introduce "into the machine a large amount of injurious self-induction, thus diminishing the "plant efficiency, and rendering the equalising of pressure with various loads very "difficult."

Turning now from alternators with, to those without iron-cored armatures, we are brought face to face with the serious fact that, in spite of the many inconveniences connected with the use of very large machines, Mr. Ferranti is laying down the Deptford station to work with engines and alternators, each of several thousands of horse-power, and is depending upon the employment of spare engines and machines, of corresponding size, to prevent or to minimise the risk of wholesale extinction of the lights. It is understood that one of the principal reasons for doing this is that parallel working is not to be relied on.

The next question is, Is it clear that, in order to be able to run them parallel, or as synchronising motors (for I need not say that the two qualities are inseparable and are in fact identical), alternators should be bad regulators, should have large self-induction, or high resistance, or both? I think not. I have mentioned Mr. Gordon's machines, which certainly had the first of those qualities, but they would not run parallel. Other machines that I have referred to, and whose parallel working is not to be depended upon, are at any rate not remarkable for absence of self-induction.

* *Journal* xviii., p. 195.

Again, is the absence of iron from the armature cores alone sufficient to deprive a machine of self-induction, and to give it a straight characteristic? I think not; for I have tested machines that had no iron, but that had a good deal of resistance, very considerable self-induction, that had very crooked characteristics, and yet that would not run parallel under any circumstances whatever.

So we see that neither a bent characteristic, nor self-induction, nor resistance, nor the use of iron cores, nor even the simultaneous possession of the whole of these admirable features and qualities—that none of these things is the secret of successful parallel working.

Perhaps—and this is a dreadful reflection—it is the exact opposite of all these!

Now I am in the unenviable position of being out of accord with the theories, the practice, the principles, and the explanations of the very able men who have lately written, spoken, and worked on this subject. I have no doubt that all they have said is perfectly correct, so far as their difficulties are concerned,—that they have experienced considerable trouble in working parallel, and have even in some cases met with actual failure. But this was partly because they have been unfortunate or unwise in the apparatus they have used, and partly because the principles that have been relied on to ensure success have not been in all respects suited to the case.

Although the conditions of the two problems are not in all points similar, I very respectfully submit that we are in danger of repeating the old mistake that was made regarding direct-current motors. We were taught that self-induction in the armatures was good, and that a special form and special proportions should be given to such motors; and I was very unorthodox when, in 1886,* I asserted that self-induction was not a virtue to be cultivated in motors any more than in dynamos, and, generally, that a good motor was a good dynamo, and *vice versâ*. However, it is some satisfaction to know that the views I then expressed

* *Phil. Mag.*, Jan., 1886, "The Dynamo as a Generator and as a Motor."

have received the sanction of general practice, although I am afraid that in the eyes of Professors Ayrton and Perry those views remain as unorthodox as ever.

Now, I am prepared to take a precisely similar stand with regard to alternators, and to submit for your criticism the view that a perfect alternator for any and every purpose should have no resistance and no self-induction.

I prefer to regard the question of alternate-current parallel working as a question very largely depending upon what are the best principles of construction for synchronising alternate-current motors. It is very much better and simpler to look at it from this point of view than from any other, and I venture to assert, in spite of all that has been said to the contrary, that if alternators are made, amongst other things, with the least possible self-induction and the lowest practicable resistance, they will not only be the best alternators, but they will best run parallel, and will do so because they will then be the best synchronising motors.

I think that even the advocates of large self-induction will admit that that quality does not improve an alternator *per se*, they only introduce and tolerate it on account of its supposed bearing on parallel working (I could quote a number of such admissions), but it appears to me that, properly regarded, self-induction is not even a necessary evil, it is an unmitigated evil. It is an evil even for this narrow and restricted purpose.

What does it do? When machines are getting out of phase, it steps in and prevents that ready transfer of current which is required in order to check the leading machine, and to accelerate the lagging machine. It prevents the latter from immediately and unhesitatingly developing those motor properties which lie at the root of successful synchronism. The foundation of parallel working should be that the prime motors are under the control of the generators. The question turns, therefore, partly on the qualities of the prime motor, but much more largely on the motor qualities of the alternators.

I am prepared to admit that if the tendency of the prime motor to lead or to lag is so great that under no circumstances can it be controlled,—and this may arise either from excessive

power on the part of a prime motor, or from defect of motor power in an alternator,—then large self-induction or resistance may be of use in order to prevent one or more of the machines being burnt up, not, however, as useful and not as effective as a safety-fuse ; but self-induction under such circumstances is not an assistance to parallel working—quite the contrary. If it were absent, probably the machines would run parallel all right. They would be much less likely to be burnt up without self-induction than with it. Self-induction is useful because it prevents the machine which ought not to have it from being burnt up because it has it.

Now I will briefly describe some experiments with two of my alternators, each made for 2,000 volts and each capable of working continuously at 35,000 to 40,000 watts, or, say, 50 E.H.P. output.

*Arrangement.*—Each machine was driven by a 75 I.H.P. Fowler engine. These engines were similar, their normal speed of working being 120 revolutions per minute. Each of the engines, which were not coupled or connected in any way, was provided with a heavy fly-wheel, and drove, besides an alternator, a heavy and wasteful double (and in one case treble) set of countershafts provided with a large number of belts, fitted to an arrangement of fast-and-loose pulleys for convenience of testing all sorts and conditions of dynamos. I mention this as showing that the momentum in each case was very considerable.

In order to make the test as onerous as possible, the pulleys used were such that one engine had to run at 130 revolutions, while the other ran at 90 revolutions, when the alternators were at their normal speed of 650 revolutions per minute.

(1.) The alternators were run up to full speed, and each excited to give 2,000 volts. When in phase they were switched parallel without any external load, and without any impedance coils or resistance between them.

They ran parallel perfectly.

(2.) A considerable inductionless load was then put on, varied, and taken off. They ran equally well under all circumstances.

(3.) They were uncoupled, and then, the load being connected to the mains, they were suddenly and simultaneously switched parallel and on to the mains with perfect success.

(4.) One alternator was excited to give 1,000 volts, the other giving 2,000 volts. They were then switched parallel, and went into step perfectly, giving a terminal P.D. of about 1,500 volts. No impedance or resistance was used in this or in any other case. A load was then put on without affecting their behaviour.

(5.) With one machine at 1,000 volts and the other at 2,000 volts they were switched parallel *when out of phase*, and instantly went into step. A large current appeared to pass between them for a fraction of a second, but not nearly long enough to enable it to be measured, or to do any harm.

(6.) They were then left running parallel while one was disconnected from the engine, by its belt being shifted from the fast to the loose pulley. It continued to run as a motor synchronously. A load of lamps was at the same time on the circuit

(7.) The two machines were then uncoupled and excited up to 2,000 volts. They were then switched parallel when out of phase and without any external load, and went into step instantly.

(8.) Whilst running as in (7), steam was suddenly and entirely shut off one engine. The alternators kept in step perfectly, one acting as a motor and driving the large engine and all the heavy counter-shafting and belts. It was impossible to tell, except by the top of the belt becoming tight instead of the bottom, which machine was the motor.

To find the power exerted by the alternator acting as a motor (in 8), a direct current motor was put in its place, and the power required to drive the engine and shafting was found to be 20 H.P.

It may be pointed out that these tests were made under the most exacting and onerous conditions that could possibly be imposed, and particularly I would point out that on account of the very great momentum of the revolving masses, nothing but the strongest and most instantaneous motor action could have kept the machines in phase. There never was a single case when they got out of step, even momentarily, or when subjected

to sudden and violent variations of load. When it is considered that, in order to secure this result, it was imperative that the control of all that mass should be exerted in a fraction of $\frac{1}{200}$ of a second (the periodicity being 100), it will be recognised that there was no time to be lost, and that the use of any self-induction or resistance, or of anything else that could in any way choke, retard, check, or interfere with the strength and instantaneity of the action was above all things to be avoided.

I should mention that the machines apparently synchronised equally well at speeds varying very considerably.

As to the self-induction of the machine itself, that is quite negligible. Its characteristic (fig. 1) is nearly straight, about

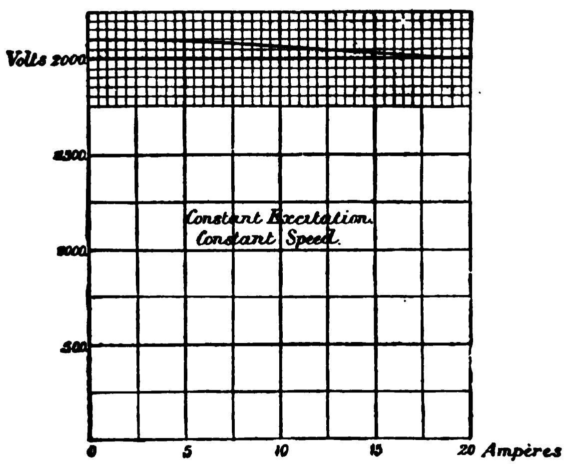


Fig. 1—Characteristic.

half the drop in the curve being due to resistance and half to self-induction.

Here was a machine generally allowed to be good when working singly, but possessing all the qualities that have been stated as unfitting it for parallel work, tested under all the conditions that are recognised as most trying, even for those types of alternators which are universally, and I believe erroneously, regarded as best suited for parallel work, and behaving throughout in a manner that simply left nothing to be desired, and carrying,

unimpaired, into parallel work those features which it possesses when run singly.

I trust that I have now said enough to justify the views I have expressed.

A few particulars of this machine may be of interest. It now takes 500 watts to excite it at full load, and rather less than 400 watts on open circuit with full E.M.F. The efficiency when working with a light load is very high. The power required to run at full speed, with full E.M.F. but no external load, is 3 H.P., of which 1·5 H.P. is ordinary mechanical loss in the bearings, etc., and 1·5 H.P. is electrical waste in the armature conductors and supports. This is from a most careful test, and shows that, so far as economy is concerned, with light loads the machine does not leave much to be desired. This is really of very great importance, for the expense connected with central station working during the long hours of daylight may be a serious item.

## Alternate Current Motors.

The bearing of the foregoing on the great and vital question of the construction of alternate-current motors is obvious. Much work has been done of late on this subject, and no doubt considerable success has been met with, but the use of alternators as simple synchronising motors has not appeared to attract much attention, probably because it is not known what excellent results may be obtained in this direction, and partly because of their inability to start from rest.

The experiments just described, and others that I have carried out, showed what perfect self-governors such motors are. Not only do they maintain synchronism, but they possess an inherent economy which is most valuable. Just enough current passes through them to keep them in step, and to do the work imposed on them. They become generators, and do work on the circuit, if from any cause there is a tendency for them to run faster than the generator.

I have devised a very simple means of starting such motors, which at a small expense removes the only drawback to their employment. The exciter, which is geared directly in some way

with the alternator, is used, in conjunction with a small accumulator, as a direct-current motor to start the alternator. When synchronism is attained the latter is simply switched into circuit. It will be obvious that a small battery capable of a heavy discharge for a minute or two is all that is required. This is recharged by the exciter. I need not go into the details of the arrangements. This is perfectly practical. I should have no hesitation in running these alternators in regular work as motors, and should expect to find their "commercial efficiency" very nearly or quite 90 per cent.—about the same efficiency, whatever it is, that they possess as generators. For instance, the machines alluded to will work as motors at a useful output of 50 H.P. with this efficiency.

And here is another advantage in getting alternators economical when on open circuit. By doing so it becomes possible to use the very small exciter as a direct-current motor to get them up to the synchronising speed.

In many situations a synchronising A. C. motor will fulfil all requirements, and will do so with fewer drawbacks than any other kind of electric motor whatever. It will be a better regulator, more economical, less troublesome to look after, much safer to handle, and (what is of very great importance in connection with the transmission of large powers to a distance) the difficulty of dealing with high pressures, which is so serious with direct-current motors, is very easily overcome, for not only will alternators stand a higher tension in themselves, but, if necessary, transformers may be used to reduce the pressure between mains and motor.

### Periodicity.

Some confusion is caused by the various ways in which rates of alternation are expressed, it being often quite uncertain whether periods or half-periods are referred to when "alternations" or "reversals" are spoken of. This confusion should be avoided by using the term "period," which has, I think, always been taken as expressing the complete cycle, or the changes undergone by a simple rectangle or elementary armature rotated from zero through a complete revolution in a simple magnetic field.

To save confusion I always write it thus, ∿; and I would suggest that this sign be used, unless a better one be forthcoming, and that we agree to speak of periods, instead of alternations or reversals, and of periodicity, instead of frequency or rates of alternation or of reversal.* Professor Silvanus Thompson informs me that formerly a somewhat similar difficulty arose in regard to sound, and that confusion was avoided in the science of acoustics by giving the word "vibration" the definite meaning of the double motion which it now possesses, the French tuning-forks being stamped with the letters "V.D." (*vibration double*) after the number.

The subject of periodicity is becoming a very important one, for both commercial and scientific reasons. It urgently demands settlement in the interests of the whole industry. The very wide differences found in scientific opinion on this matter, and in the practice of the various manufacturers, must lead, and are indeed now leading, to much confusion and inconvenience. As things stand at present, great uncertainty exists not only as to what rate is the best, but whether transformers, arc lamps, or other apparatus made by one maker, for a given E.M.F. or purpose, can be used on a circuit fed from an alternator constructed by another maker. It might be suggested that some joint action should be taken by manufacturers, and others concerned, in the direction of securing uniformity; but there are arguments against such a course, the principal one being that probably each maker believes he has good reasons for his own practice.

I venture to think there is no right or wrong periodicity. What may be best for one set of conditions, or with one type of apparatus, may not be best for another. Or perhaps it would be more correct to say that there is a right periodicity, and that there are right and wrong apparatus; but this is rather a delicate subject.

The relative advantages of high or low periodicity must be

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* This sign may be written something like, but rather larger than the algebraic sign ( ∼ ) for "difference."

considered as bearing on the five main divisions of apparatus used. Taking these in their natural order, they are—

1. *Prime Motors.*
2. *Alternators.*
3. *Conductors.*
4. *Transformers.*
5. *Lamps, &c.*

Each of these divisions contains matter in relation to periodicity which in the near future will probably form the subject of many papers, as the various subjects become better understood. I am not able to do more than touch briefly on each, in the hope that my remarks may lead to the expression of the views of others, and to some approach to uniformity.

1. *Prime Motors.*—At present questions affecting prime motors do not usually bear directly on the subject of periodicity. The only exception is the Parsons steam turbine, with which there seems to be no great difficulty in using a simple two-pole machine, thus making the number of revolutions and of periods the same. There may be found some want of flexibility as regards changes of size or output with such turbines, as, from the nature of the case, speed can only be varied by very large amounts. Thus, for instance, if 100 ∿ are required, there is no choice except between a two-pole machine at 6,000 revolutions per minute and a four-pole machine at 3,000 revolutions. This is, of course, on the assumption that the alternator is driven direct. If indirect driving is resorted to, the difficulty is much reduced. It is unlikely, however, that the bearings of a steam turbine would work satisfactorily except with direct driving.

2. *Alternators.*—It is not allowable to doubt that the divergencies in periodicity found in practice are to a great extent the esult of development and survival of what has been found to give the best results in each case; but from an examination of the machines in use it is very difficult to trace any common line on which action may be supposed to have taken place. It is true that the general tendency has been towards a reduction of the periodicity, but the causes which are responsible for that tendency are very obscure. I have seen large and small machines, made at the

same time by one maker, in which the number of coils was the same, although one had to run at about 2,000 revolutions a minute, and the other at less than half that rate of speed. At least one of these was wrong. For the purpose for which these machines were made the very high periodicity was almost unobjectionable in the small machine, but in the large ones it led to disaster.

It is a common mistake to suppose that high speed of driving, or high peripheral velocity, are necessarily connected with high periodicity and with large output. Of course any given machine, if run at a higher rate of speed than its normal one, must have a proportionately higher periodicity, but will not necessarily be capable of a greater output. It is quite possible to design a high-speed machine to give a low periodicity, and *vice versâ*. Perhaps the impression has arisen because with most alternators it is not possible or desirable to place the successive poles very close together on account of the magnetic leakage.

For machines with iron in the armatures it might be expected that the magnetic and electric losses, as well as the very considerable self-induction and consequent variations in the effective potential difference with changes of load, would lead to the reduction of the periodicity; but experience does not seem to have always led to this result. The lowest periodicity (42 with the Zipernowsky machine) and the highest (132 with the Westinghouse machine) are both found in alternators having iron cores. What is the reason of this great diversity? The machines have many points of resemblance, and yet one is run at more than three times the periodicity of the other. Not having seen the American machine, I can offer no opinion based on personal observation; but of the Austrian alternator I can say that, in my opinion, it would not be advisable to increase the rate.

In a communication from Messrs. Ganz & Co., published a few days ago,* Mr. Zipernowsky says that the low periodicity used was chosen on principle to enable them to couple their dynamos parallel. He very cogently adds, in justification of this: "At

* *The Electrician*, May 10th, 1889, p. 16.

"present no practical electrician will hesitate to acknowledge the "coupling of dynamos in parallel circuit to be a vital question of "any parallel system of central-station distribution."

This bears out very completely my contention that the periodicity is governed in most cases by some special feature of the type of apparatus. Zipernowsky has had to go down to 42 ∿ to get his iron-cored alternators to synchronise, and for no other reason; for his statement is that synchronous action was "not a secondary result, but just the end we were aiming at," and he acknowledges that there are disadvantages connected with this low rate in other parts of the system.

But Westinghouse with 133 ∿ and Lowrie-Parker with 80 —both having iron cores—can run parallel, the former with not less than half-load, the latter, as we now know, with any load, and I think I could run parallel without iron and at any load and periodicity, so that Mr. Zipernowsky's only reason fails to justify his action, except as regards his own particular apparatus.

Professor Forbes recently stated that the exhaustive tests made in America with the Westinghouse apparatus had shown that there was no reason for reducing the rate of alternation, which is the highest now in use. This is very important, and it is to be hoped that some account will be given of the nature of the trials and of the results obtained. Mr. Ferranti, who at one time used very high rates, has for some time been using 80 ∿ at the Grosvenor Gallery, and has fixed 68 ∿ for the Deptford machines. I shall not be surprised to see him increase this before long.

In my own machine the periodicity is 100, which, I believe, is on the whole the best rate; but if it be required to do so for any special purpose, there would not be much difficulty in changing it. The pole faces can be altered in number and in size by merely altering the pattern; the field winding, being in a single coil, would be equally simple and equally efficient for any periodicity. Even with a large number of poles placed close together, as would be required for a high rate, magnetic leakage would not increase. The size of the machine would not be sensibly affected. The armature would certainly be rather more

costly for a high rate, as a greater number of coils would be required; but the efficiency, output, and regulating qualities would remain practically the same. Regarding this machine, I think I may say that periodicity is not a question of the first importance. As far as I am concerned it is other divisions of the distributing apparatus that determine the rate at which it is desirable to work.

The bearing of the form and arrangement of any alternator on the best periodicity for that alternator is a very close one. The question cannot be settled on general principles.

I have referred elsewhere more fully to the conditions as they affect regulation under varying loads, parallel working, and other points in connection with alternators; and, as an appendix (see p. 20), I have quoted some observations made recently at the Institution of Civil Engineers during the discussion on Mr. Kapp's paper on "Alternate-current Machinery," and not yet published.

I now pass on to the next division of the subject.

3. *Conductors.*—The choice of a suitable periodicity depends also to some extent upon the conditions as they affect the conductors.

The effect of rapid alternations in virtually increasing the resistance of conductors has been brought before us in Sir William Thomson's Presidential Address, and it becomes necessary to recognise clearly what practical limitations are imposed by this effect.

My attention was drawn to this matter two or three years ago, when I was called upon to examine and report upon an installation of about 1,000 100-volt lamps supplied directly by an alternator working at about 150 ∿. It was found that there was a very serious drop of potential in the conductors, and, in consequence of this, the lamps could not be maintained at full power. On examining the circuit I found the cables were of ample size, and that the loss could not be explained in the usual way. As a result I came to the conclusion that it was to be accounted for only by some kind of inductional action, due to the alternating current, and I advised that a direct-current dynamo should be put down in place of the alternator. This was done

and with a perfectly satisfactory result. The lamps were for the first time run at their proper power, and no further trouble was experienced.

At that time I was, like most other electricians, a disbeliever in alternating currents, which I vaguely regarded as essentially unsatisfactory. I think there was a more substantial basis for this opinion than is now generally admitted. Alternating currents as then most often used were not satisfactory; and if great care is not taken at the present time, some of the old difficulties will be encountered, with the result of unnecessarily discrediting the transformer system of distribution.

From Sir William Thomson's Address, and from some further information he has kindly sent me, I have worked out and tabulated some figures showing what restrictions are imposed by the virtual increase of resistance of solid conductors with alternating currents, as bearing on the question of periodicity.

Sir William Thomson gives some figures for a periodicity of 80, to which I have added some columns giving diameter in inches, sectional area, and the current which the practicable sizes will carry, taken on the basis of 450 amperes per square inch, because that density gives 1 *per cent. loss per mile* with 2,000 volts. It also gives about 1 *per cent. loss per* 100 *yards* with 100 volts, or, more exactly, 1·15 per cent. I have also given the number of watts at 2,000 volts, and at 100 volts, for each size, to show the limits for ordinary primary and secondary working with this density. These figures are given for 80, for 100, and for 133 $\sim$, the diameters of conductors for the same percentage increase of virtual resistance over ohmic resistance being in the inverse ratio of the square roots of the periodicities.

*Table I.—Virtual Resistance, &c., of Conductors with Alternate Currents.*

| Diameter. | | Area. | | Increase over Ordinary Resistance. | Current at 450 amperes per sq. in. | Watts at 2,000 volts. | Watts at 100 volts. | ∿ per second. |
|---|---|---|---|---|---|---|---|---|
| MM. | Inch. | Sq. mm. | Sq. in. | | | | | |
| 10 | ·3937 | 78·54 | ·122 | Less than $\frac{1}{100}$ % | 55 | 110,000 | 5,500 | |
| 15 | ·5905 | 176·7 | ·274 | 2½ % | 133 | 266,000 | 13,300 | |
| 20 | ·7874 | 314·16 | ·487 | 8 % | 220 | 440,000 | 22,000 | |
| 25 | ·9842 | 490·8 | ·760 | 17½ % | ... | ... | ... | 80 |
| 40 | 1·575 | 1,256 | 1·95 | 68 % | ... | ... | ... | |
| 100 | 3·937 | 7,854 | 12·17 | 3·8 times | ... | ... | ... | |
| 1,000 | 39·37 | 785,400 | 1,217 | 35 times | ... | ... | ... | |
| 9 | ·3543 | 63·62 | ·098 | Less than $\frac{1}{100}$ % | 45 | 90,000 | 4,500 | |
| 13·4 | ·5280 | 141·3 | ·218 | 2½ % | 98·5 | 197,000 | 9,850 | 100 |
| 18 | ·7086 | 254·4 | ·394 | 8 % | 178 | 356,000 | 17,800 | |
| 22·4 | ·8826 | 394 | ·611 | 17½ % | ... | ... | ... | |
| 7·75 | ·3013 | 47·2 | ·071 | Less than $\frac{1}{100}$ % | 32 | 64,000 | 3,200 | |
| 11·61 | ·4570 | 106 | ·164 | 2½ % | 74 | 148,000 | 7,400 | 133 |
| 15·5 | ·6102 | 189 | ·292 | 8 % | 131·4 | 263,000 | 13,140 | |
| 19·36 | ·7622 | 294 | ·456 | 17½ % | ... | ... | ... | |

It will be seen that for 100 ∿, with wires of 9 millimetres, or ·35 inch diameter, the increase over the ordinary resistance is almost imperceptible; for a diameter of 13·4 millimetres, or ·53 inch, it is only 2½ per cent.; while what may be considered a practical limit is reached at 18 millimetres, or nearly ¾ inch. At greater diameters the increase is prohibitive, and multiple conductors, tubes, or strips must be used. Probably an increase of 8 or 10 per cent. will be considered quite permissible. It sounds a good deal, but it must be remembered that it is only 8 or 10 per cent. of the 1 or 2 per cent. loss for which primary conductors are usually calculated.

Looking at the limitations as regards output, it may be said that for primary distribution, even at no higher tension than 2,000 volts, the virtual resistance effect is, if not quite unimportant,

at any rate not very serious. A current of about 200 amperes may be conveyed without the effect showing itself to any very objectionable extent. There may be differences of opinion on the subject, but most people will agree that 300,000 or 400,000 watts is quite as much as should be supplied through any single conductor. In most cases, even at ordinary tensions, subdivision of the primary will be necessary or advisable before such a power is reached. With still higher pressure, of course, enormous powers can be dealt with without exceeding the permissible size of solid conductors. It is to be observed that there is nothing, in this virtual resistance effect, to necessitate the reduction of current by the employment of very high pressures.

It is only with the low-tension secondary conductors that this effect becomes in any way serious. With 100 volts the maximum load at this density that can be put on a single solid or stranded conductor is about 13,000 watts for 133 $\sim$, 18,000 for 100 $\sim$, and 22,000 watts for 80 $\sim$, unless much more than the 8 to 10 per cent. increase can be endured. This only means in large distribution work a subdivision of the primary and secondary feeding points in accordance with this limitation. The inconvenience has only to be recognised to be avoided. Fortunately it is not one that with ordinary care is likely to lead to any practical inconvenience. It exists with alternate currents, but it is a matter that the transformer system renders it very easy to avoid. Transformers lend themselves to subdivision of work, and with subdivision this difficulty does not exist.

It may be worth noting, in connection with this part of the subject, that the improvement in alternators, and the great ease with which they and their circuits can be worked and controlled, may lead to some revival of the practice of supplying direct from low-tension alternators. If this is done for any but very small installations, it is evident that special precautions must be observed to avoid the difficulties which have been referred to.

One other remark on this subject. Very large conductors are to be avoided in the transformers as well as outside of them. This is only mentioned on account of its bearing on transformers

for any very low tension work, such as electric jointing or welding, where one or two turns of secondary conductor is often sufficient.*

4. *Transformers.*—There appears to be a general belief that a high periodicity is best for transformers, and that this gives the greatest output, or—what amounts to the same thing—that with the same output in the two cases a higher efficiency is obtained when the periodicity is high than when it is low. This supposed fact has often been stated.

Professor Forbes—who has, I believe, given much attention to the theory of this very difficult subject—contributed a paper last year,† consisting of a mathematical examination, which is summarised and concluded by a statement that the periodicity may be diminished without loss of efficiency if the iron be increased; the corollaries being, of course, that the periodicity may be

* The following particulars refer to conductors for primary circuits working at 2,000 volts, and show the sizes for various numbers of lamps giving a loss of 1 per cent. per mile run.

Data for copper conductors working with 2,000 volts pressure at the primary terminals of the transformers.

Transformers taken at 95 per cent. efficiency.

To allow 1 per cent. loss of pressure per mile of conductor, the primary current-density should be 450 amperes per square inch.

*Conductor for any given number of 60-watt lamps.*

Sectional area, in square inches = number of lamps × ·00007
Weight per mile, in lbs. = ,, ,, × 1·44
Resistance per mile, in ohms = 630 ÷ number of lamps.

The following conductors comply with the above rule, and none of them is affected by the virtual resistance effect:—

*Table II.*

| Number of Lamps. | B.W.G. No. | Diameter. | Sectional Area. | Weight per Mile. | Resistance per Mile. |
|---|---|---|---|---|---|
| | | Inch. | Sq. inch. | Lbs. | Ohms. |
| 100 | 13 | ·098 | ·007088 | 144 | 6·3658 |
| 200 | 10 | ·134 | ·014102 | 286 | 3·1890 |
| 300 | 8 | ·165 | ·02138 | 433 | 2·1102 |
| 400 | 6½ | ·191 | ·02865 | 578 | 1·5747 |
| 1,000 | 1 | ·300 | ·07065 | 1,433 | 0·6401 |

† "Formulæ for Converters," *Journal*, vol. xvii., p. 153. This paper was no doubt intended only as a mathematical investigation. It contains several assumptions that Professor Forbes would probably not rely upon in practice.

increased without loss of efficiency if the iron be reduced, and that with any given transformer an increased periodicity results in increased efficiency for the same output, or in greater output with the same efficiency.

Professor Forbes confirmed this view recently, in the discussion on a paper by Mr. Kapp before the Institution of Civil Engineers, by pointing out that one of the reasons why in America they used a high periodicity was that "they wanted "to get the greatest output from their plant, and transformers "had to be increased in size in order to give the same output if "they lowered the number of alternations."* And, in his recent paper on "Central Stations" before this Institution,† Professor Forbes reiterated this. He states that the contrary is "opposed "alike to experience and theory. Westinghouse knows this. I "am informed Mr. Ferranti knows it, and Mr. Zipernowski has "told me that he finds it so."

Mr. Kapp, who works at 80 ∿, does not go so far as Professor Forbes, for he thinks that on account of viscous hysteresis there must be a limit beyond which an increase becomes disadvantageous. He wisely adds that it is not possible to determine this limit on theoretical grounds.

On the other hand, it has been said that it is not necessary to increase the size of transformers even for very low periodicities, because, when the number of cycles is small, it is possible to work at a higher magnetic density.

I venture to question the accuracy of both these extreme views.

So far as transformers are concerned, and speaking only from experience, I think that high and low periodicities are both wrong, and that a medium rate is right. For a given output and efficiency, not only must transformers be made large for low rates—they must also be made large for high rates; but between the two there will be found a periodicity giving the smallest size of transformer, or the greatest output, or the highest efficiency.

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* Proc. Inst. C.E. February 19, 1889.

† *Journal*, xviii., p. 194 (Feb. 28, 1889).

Fig. 2 illustrates this. It shows the rise of temperature of one of my transformers working respectively at 75, at 100, and at 125 ∿ per second. The primary and secondary virtual potential differences were measurably the same in all three cases, and in each case the same load was used, consisting of a certain number of glow lamps. The test was continued each

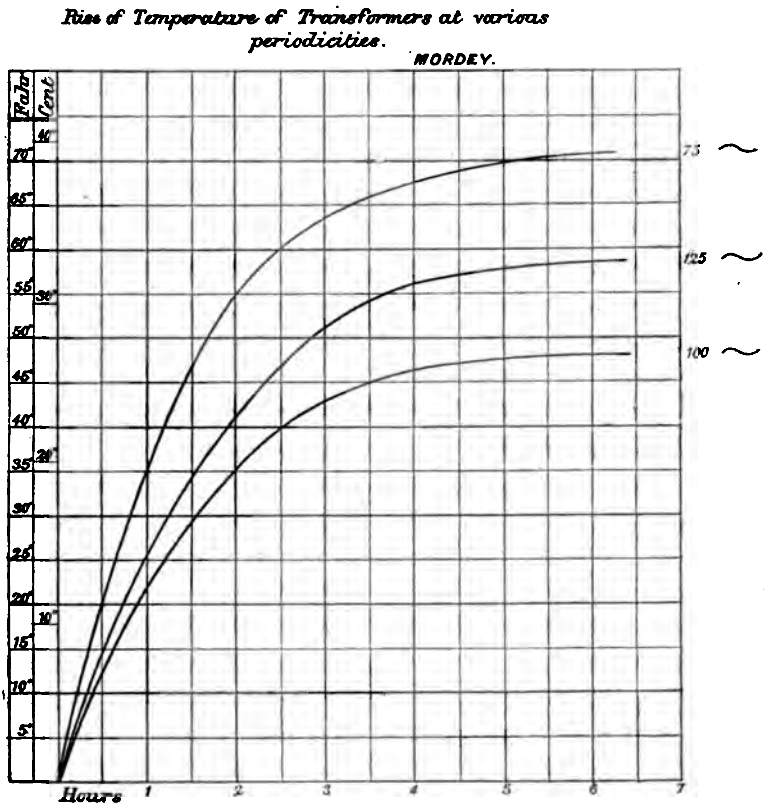


FIG. 2.

time long enough for the transformer to attain its maximum temperature. This was taken by a sensitive thermometer the bulb of which was placed directly on the iron, and covered by a packing of cotton waste to ensure that, as far as possible, at least the actual temperature of the iron should be indicated. Another similar thermometer was placed a few feet

away to show the temperature of the room (which was fairly constant at about 60° Fah.).

The curves give the actual increments of temperature, which may be taken as some sort of measure of the relative efficiency attained. With any given output the same rise of temperature is, of course, always obtained with the same efficiency, quite apart from how that efficiency is made up. It will be observed that the lowest temperature or the highest efficiency is obtained at 100 ∿; that the efficiency is lower at 125, and lowest at 75 ∿.

On these lines I may suggest a method of arriving at the efficiency of transformers, which, though lacking in directness, is simple, and requires only ordinary appliances and facilities.

First run the transformer for some time in the usual way, on incandescence lamps, the output being measured. It is not necessary to make any measurements of the primary. By means of a thermometer find the rise of temperature of the transformer. Then, when the transformer has cooled down, send a continuous current through it and measure the power in watts that raises the temperature of the whole transformer the same amount as when working with alternate currents. This takes some time to carry out, but it is accurate, and has the advantage that the lost power is measured in direct-current quantities, about which there is no question; while the power expended in the lamps, being in an inductionless circuit, is easily arrived at correctly. A wattmeter is the best instrument to use for the direct-current readings, as, on account of the rise of resistance of the conductor (which is necessarily required to carry much more than the usual current), some adjustment is necessary in order to keep the power constant, and this adjustment is effected much more conveniently with a wattmeter in circuit than if an ammeter and a voltmeter are used.

5. *Lamps, &c.*—Under this head all apparatus for utilising or measuring alternate currents should be considered, so far as the action is affected by the periodicity. I must, however, confine myself to the lamps, which are the most important, and are the only apparatus as to which I can make any definite statements.

The paper of Professors Ayrton and Perry read last year before the Physical Society* showed that the efficiency of glow lamps is the same for both direct and alternating currents. The authors of that paper did not directly allude to the question of relative efficiency with different periodicities, but they evidently had this point in their minds, as the periodicities used in the experiments appear to have been varied from 83 to 226 without showing any difference in efficiency. It may be taken, therefore, that no difference exists. Probably for very low periodicities this would not hold good, but for ordinary rates the question may be considered settled. At very low rates the temperature of the filament would follow to some extent the changes of the current, and would be affected both in life and in efficiency. I suppose that at ordinary rates there is no variation of temperature, the filament acting as a kind of thermal fly-wheel, maintaining its condition unchanged throughout the whole period. We all know, for instance, that, especially with low-resistance lamps, when the current is interrupted the time occupied in cooling down to blackness is quite appreciable. The periodicity at which the temperature begins to follow the variations of current is a point of some interest, and it is one that can be very easily investigated.

There is, as far as I am aware, no available evidence regarding the life of lamps with different rates, or even as to the relative life with direct and alternate currents. At one time it was considered that alternate currents would give better results than direct currents, because with the latter there was supposed to be some transference of the material of the filament in the direction of the current. Nothing has transpired to confirm or to disprove this supposition, and, as the number of lamps now in use on each system is enormous, the very absence of evidence may be taken as affording a strong presumption in favour of the belief that the life of lamps is independent of whether the current is direct or alternating, and independent also of the periodicity, within practical limits.



* *Jour. Phys. Soc.*, vol. ix., p. 208.

Alternate-current arc lamps are not seriously affected by variations of periodicity, within a fairly wide range. The feeding current of a parallel Brush alternate-current lamp remains sensibly constant between 75 and 125 ∿, but with the lower rate a slightly higher impedance or resistance is required to secure steadiness of the light.

### Secondary E.M.F. of Transformers.

The best potential difference to be used on transformer circuits has been a good deal discussed, especially with reference to the voltage of lamps. It is very desirable to arrive at some conclusions on this matter, not only as to glow lamps, but as to the general question.

The balance of opinion—which is, however, not very decisive—appears to be in favour of using 50-volt rather than 100-volt lamps, so far as durability of the lamp is concerned. The stout low-tension filament should last longer than the finer one for high tension, because disintegration does not so soon reduce the cross section to the breaking point. It is, however, not a question only of life, as the disintegration of the thick filament causes blackening of the globe; and if it can be shown that this blackening, and the consequent lowering of the effective light, is more serious than in 100-volt lamps, then the advantage of slightly longer life will not of itself be sufficient to recommend 50-volt lamps in preference to others of higher voltage.

Of course the most serious objection to lowering the voltage is the increased cost of the secondary conductors. This is not important in small and very compact installations such as can be so readily supplied by transformers—for instance, in small shops and ordinary houses, where a separate transformer may be placed within a few yards of the lamps, and where the fall of potential is negligible; but for larger work it is advisable to keep the potential difference high, as with direct currents, and for the additional reason that the use of very large alternate currents, with correspondingly large conductors, is accompanied by the

inconveniences arising from the high virtual resistance already alluded to.

It is impossible, however, to choose an E.M.F. that will meet all requirements. For instance, high candle-power lamps are coming largely into use, and as this practice extends, higher E.M.F. will be demanded in order to keep down the diameter of the "stick."

But, on the other hand, lamps of small candle-power can be more easily made for low than for high voltage; and as there is a growing tendency to use small candle-power lamps which in many situations give all the light that is required, this point should be kept in mind, especially as it is very probable that before long a demand will spring up for lamps of three or four candles. I know it will be said that it is easy to reduce the candle-power of ordinary lamps by inserting an impedance coil in the circuit. This is one of the many conveniences of transformer working, but it should not be resorted to for permanent purposes, as, however little power such a coil may absorb, lamps reduced in that way, being worked at less than their normal candle-power, are very inefficient.

Then there is another reason for sometimes preferring 50 volts to 100 volts, that I should like to mention, and that is that alternate-current arc lamps do not require more than 50 volts terminal pressure. It may be expected that many such lamps will be used on transformers—for instance, at small business places, where perhaps the bulk of the lighting will be internal and by incandescence lamps, but where it will be convenient to have one or two arcs outside. We know that when arcs are used in parallel with incandescence lamps on direct-current circuits it is necessary, in order to get the best results, to have 12 to 15 volts drop of pressure in a fixed resistance in series with each arc. Thus has arisen the custom of using 65 volts for mixed arc and glow lamp circuits, where the potential difference at the arc lamp terminals is reduced to about 50 volts by the insertion of such a resistance, the incandescence lamps, however, being made to work at 65 volts.



This is a very wasteful arrangement, but, apart from its waste-

fulness, 65 volts is a very inconvenient pressure both from the manufacturer's and the consumer's point of view. It does not lend itself to combination as does a pressure of 50 volts dynamos, lamps, or transformers, for 65 volts cannot be used in pairs on 100-volt circuits as can such apparatus for 50 volts. It is an irregular and inconvenient pressure that is demanded by the necessities of direct currents, but that I am glad to be able to say need not be imported into alternate-current work.

This very serious loss can be almost entirely avoided with alternate-current arcs, as the necessary steadying effect is readily obtained by the use of an impedance coil, absorbing much less power than does a resistance; and as such arcs require only 35 to 40 volts between the carbons, instead of 45 to 50 with direct currents, it is sufficient to provide 50 volts potential difference between the secondary conductors.

But it may be said that, as impedance coils absorb little power, the arc lamps may be put on 100-volt circuits in parallel. This may be done without much objection so far as the lamp and impedance coil are concerned, but there is the drawback that the "plant-efficiency" of the transformer will then be low.

The output of a transformer is limited partly by the size of the conductor. The actual output of a 100-volt transformer supplying arcs through impedance coils will not be much more than the product of the current × 50 volts, while for the same output in watts on glow lamps it would only have to yield about half the current. Therefore we see that, although not inefficient, the maximum allowable output of a 100-volt transformer on parallel arc lamps is only about one-half of its ordinary maximum output.

It will be said that two arcs may be run in series on the 100-volt secondary. This is true, but two arcs are not always wanted; and, further, the working of two arcs (which must for this purpose be shunt or differential instead of simply series-wound) in series on a constant potential difference is the least satisfactory way of arranging arcs, whether for direct or alternate current, as in this way they affect one another the most; having neither the margin of controlling power, by current, which

is provided in the parallel constant potential difference arrangement; nor by E.M.F., as in the series constant-current arrangement. Then, again, lamps so connected must be extinguished together, as no cut-out or switch can be used unless it is made to insert a resistance or impedance to take the place of the extinguished lamp.

On the whole, therefore, it is preferable to have 50-volt secondaries where arcs are to be used, or to adopt the intermediate plan of connecting the middle of the 100-volt secondary to a third terminal, using a third wire for any arcs or 50-volt lamps that may be required. It may be worth while to point out that the use of a third wire on transformer circuits may lead to difficulty if not carefully considered. With a single transformer it is as easy to use three or any other number of terminals for the lamps as it is to do so on a set of accumulators, and to get any corresponding potential difference. The various sections of the secondary may be unequally loaded by this arrangement, but the primary responds as a whole to the demand made upon it. If, however, instead of a single transformer, two transformers are used, with their primaries and secondaries respectively in series, then the use of the three-wire principle is only possible when it is applied to both the primary and secondary circuits, unless the load on each of the two secondaries is always to be alike and the same, in which case, as scarcely need be said, there is no particular advantage in using three wires. If the loads are not equal on the two, then the primary of the least loaded transformer acts as an impedance coil to the other primary, and all power of self-regulation is lost. One remedy for this is to apply the three-wire principle to the primary as well as to the secondary; but this is a serious matter, and one that could only be undertaken where the whole service demanded it. Another plan is to have two mains for the primary and three for the secondary, and to join the primaries parallel, the secondaries being in series with the third wire at the middle terminal.

In cases where arcs have not to be used, and in the absence of any very strong evidence of any difference of life in favour of 50-volt lamps, I think it is best to use 50 volts for small separate

installations, and 100 volts for larger ones; but it is evident from what has been said that there are so many points to consider that it is quite impossible to give any definite negative or affirmative reply to the question, "What is the best secondary E.M.F. to "use?" Each case, or at least each class of cases, must be settled on its merits. If for general convenience any particular pressure is selected, it is obvious that it will have about equally strong arguments for and against it.

### Alternate-Current Arcs.

Reverting to this subject, it is to be regretted that very little appears to be commonly known about A.C. arcs. I trust that in the discussion of this paper some imformation may be furnished with regard to them.

As to the regulating mechanism of lamps, although some difficulties were encountered in modifying the Brush arc lamp for this work, the result has been perfectly satisfactory; the feeding range is very small—quite as small as with direct currents, and with no greater loss of power in the lamp-box. Some trouble was experienced in getting rid of noise and vibration in the coils and mechanism, but this has been completely overcome, the lamp itself being quite silent. This is readily proved by connecting the carbon-holders by a flexible wire and adjusting the current to the normal value, when it is found that there is no noise. An A.C. arc is not silent, as is well known, a distinct hum being set up, by which it is quite possible to ascertain the periodicity.

This humming noise, which can scarcely be expected to be overcome, renders A.C. arcs unsuited for general use indoors; but for outside illumination, and for such places as railway stations, they will certainly find a very extensive application.

I venture to think that the opinion so often expressed, that A.C. arcs give much less light than those supplied with direct current, is founded on a misunderstanding of the proper conditions. It is quite true that if two arcs are compared, supplied with the same amount of current, the direct-current lamp will prove the better one. This, I think, is because the basis of

comparison is not right. In many cases the A.C. arcs are more efficient. They should be judged by the energy, and not only by the current. I have mentioned that an A.C. arc requires only 35 to 40 volts potential difference between the carbons, while a direct-current arc requires 45 to 50 volts. The mean may be taken at 37·5 volts and 47·5 volts respectively, and to expend the same energy in the two cases the current should therefore be in the inverse ratio of these values. Thus an A.C. arc, to give the same light and to absorb the same power as a 10-ampere direct-current arc, should be made for 12·6 amperes.

In one respect A.C. arcs are inferior, and that is in their power of directing the light downwards. They have no "crater," and therefore do not send the light down as do direct-current arcs. On this account it is advisable to use reflectors above them, unless they are suspended below and near a good reflecting ceiling or roof.

But even this drawback is counteracted in parallel work by their greater efficiency. On a 65-volt circuit with direct currents, as I mention elsewhere, there is a waste of 100 to 150 watts with a 10-ampere arc, and in the same proportion with other currents. This is a very serious proportion of the whole power, and it is only allowable to waste it because, as a light-producer, the arc is so much more efficient than the incandescence lamp. Nevertheless, any means of avoiding the loss is to be welcomed, and with A.C. arcs it may be greatly reduced by the use of impedance coils, which, if properly designed, waste very little power, and have all the steadying effect of resistances.

## Safety and Safeguards.

I take this opportunity of bringing up this question, not because I have anything new to say with regard to it, but in order, if possible, to bring it a little nearer to a rational settlement.

It will be acknowledged that the first duty of an "undertaker" using high tension is to provide for the complete and absolute safety of the consumers. If there are no means of doing this

then the system is fundamentally bad, and must sooner or later be abandoned.

There are several ways of protecting the house mains more or less completely. I only need allude to three typical plans, all of which have already been discussed before this Institution.

One is the plan introduced by Mr. Kent, of entirely separating the primary and secondary conductors in a transformer by a sheet or division of earth-connected metal. This is open to adverse criticism on some points, but on the whole it is an effective plan.

Another arrangement is the ingenious one of Capt. Cardew, consisting of a sort of static mouse-trap, which immediately goes off when an unauthorised P.D. enters it.

The third plan is that of earthing the secondary, and it would be very instructive if those who so strongly oppose this plan would state just one real objection to it. It is a perfectly sure method, and costs nothing.

It renders it absolutely impossible for the consumer to get a dangerous shock under any possible combination of circumstances whatever.

The faults that may occur are—

(*a*) Contact between primary main or mains and earth.

(*b*) Contact between primary and secondary in a transformer.

(*c*) Contact between a person and a portion of the secondary conductor.

(*d*) Contact between that person and earth.

Now if all these contacts occur at the same time, the "person" will get a shock.

But if the secondary is earthed, no shock can be obtained, except, perhaps, a slight one due to the secondary P.D.

This simple plan has been approved by almost everybody, but it is received with opposition on the part of some of the fire insurance offices and others.

It is difficult to ascertain where the supposed danger is. If the secondary is earthed, the effect of a contact between primary and secondary, when the former is accidentally leaky, is at once to blow out the primary safety fuse of the transformer, and so

cut out the offending house. This is the simplest and the best course to take, and prevents the continuance on the circuit of transformers that contain faults.

The Brush Corporation has adopted this plan with perfect success on a large installation abroad, and intends adhering to it where local circumstances permit.

It is to be remarked that it is not open to the objection that has been stated against earthing of one of the primary conductors, viz., that the telephones may be affected, for it is not permanent. The moment a contact occurs that could lead to a disturbance of the telephones, the fuse prevents that disturbance being continued.

While effectually removing life risks, this precaution does not increase fire risks; in fact, it tends to greater safety, as it is some sort of check on the character of the house wiring work, and so reduces chances of faults such as short-circuiting of the secondary.

The rupture of a primary safety fuse is not a serious matter, and in any case it is only what takes place under ordinary circumstances when any fault occurs that results in an excess of current.

As to static effects, I think it will be found that there will always be enough general leakage to prevent such effects being a cause of difficulty, even if the secondary circuits are not earthed.

### Hysteresis and Eddy-Currents.

Some explanation is necessary by way of preface to this section of the paper. The experiments described were made with a direct-current dynamo, and on that account they only touch the fringe of this division of the subject. As I had no suitable alternator, I was unable to pursue the question as far as I wished to do, but I hope I need make no apology for bringing forward such results as I have been able to arrive at.

As far as I am aware, no simple or practical method is known by which the power absorbed in eddy-currents can be determined and separated from that due to hysteresis in the armatures of dynamos. This is a point of some importance both as regards

alternators and direct-current machines, for many reasons; one of the principal being that in the absence of exact knowledge on these matters it is difficult or impossible to determine to what extent the lamination of the iron cores should be carried. There are wide differences in the practice of the various designers and makers of dynamos in the thickness of the iron used.

Lamination may be carried to such a degree of fineness for the purpose of reducing eddy-currents as to add very materially to the cost of production, the iron increasing rapidly in price as the thickness is reduced; and it may be that the point is sometimes passed beyond which no gain in efficiency is obtained. This is a question we wish to be enabled to settle.

Until Professor Ewing* and Dr. J. Hopkinson† read their Royal Society papers on the subject, very little was known of the heating effect of changes of magnetism. Professor Ewing opens the part of his paper dealing with this matter in the following terms (p. 552, *Phil. Trans.*):—

"The energy expended in a cyclic process of magnetisation "can take no other form than that of heat diffused throughout "the substance of the metal. Experiments have been made by "Joule and others to determine by direct observation the heating "effect of magnetisation in iron.‡

"In most direct measurements of this quantity no distinction "is made between the heating effect due to the induction of "electric currents and that due to changes of magnetisation *per* "*se;* and so excellent an authority as Professor Rowland, writing "in 1881, has expressed himself as doubtful whether changes of "magnetisation, considered apart from the currents they induce, "give rise to any development of heat at all."

Further on, Professor Ewing, referring to the dissipation of energy by hysteresis, which he had determined by laboratory experiments, says (*Phil. Trans.*, pp. 553, 554):—

* Professor Ewing, "Experimental Researches in Magnetism," *Phil. Trans.*, Jan., 1885.

† Dr. J. Hopkinson, "Magnetisation of Iron," *Phil. Trans.*, April, 1885.

‡ Joule, *Phil. Mag.*, vol. xxiii., 1843; Grove, *Phil. Mag.*, vol. xxxv., 1849 Villari, *Nuovo Cimento*, 1870; Cazin, *Ann. de Chem. et de Phys.*, 1875; Trowbridge, *Proc. Amer. Acad. of Arts and Science*, 1879.

"These developments of heat are so small as to make it "apparent that the very considerable thermal effects which "reversals of magnetism cause in the revolving cores of some "dynamo-electric machines must be due almost wholly to the "internal induction of currents, so far as they are not due directly "to the current circulating in the coils of the armature.

"The experiments have a practical value in showing that cores "which are so thoroughly laminated as to render the induction of "currents within them unimportant, do not involve any serious "loss of energy; and that the efficiency of a machine with a soft "iron core or cores whose magnetism is periodically reversed need "not, on that account, be materially less than that of a machine "which has no such cores. The absence of iron from the "armature has been claimed, on the score of efficiency, as an "important advantage possessed by some types of machine, but "unless the claim has some other basis it appears to me to be "illusory. Magnetic reversal does involve some loss of energy, "but if the cores are properly laminated it is so small as to be "practically insignificant."

I quote these passages as showing the position of the question. Professor Ewing and Dr. Hopkinson give laboratory determinations of the magnetic loss in iron with various inductions, and under various conditions. It is, however, left somewhat in doubt as to whether the loss in dynamo armatures will be the same as in the laboratory experiments, the conditions being very different in the two cases.

On this point Professor Ewing shows that hysteresis is much less when the iron is in a state of mechanical vibration than when it is in a state of rest, and states that in a dynamo the loss will be less than the experiments on still metal might lead us to expect.

Another way in which the dynamo conditions may cause some difference is the possible variations of the magnetic density that may exist in the core. Such variation may be considerable in armatures of large size.

Dr. Hopkinson (*Phil. Trans.*, p. 467) takes the magnetic loss in armatures as being the same as in the small pieces of iron tested by his method, and gives an example calculated for an armature core "finely divided, to avoid local electric currents."

It is to be noted, then, that both these authorities regarded eddy-currents as unimportant in laminated armatures; but in the later paper on "Dynamo-electric Machinery"* Drs. J. and E. Hopkinson published their famous investigation into the efficiency of dynamos, and gave very full particulars, from a study of which it is possible to ascertain the loss in eddies, on the assumption that the method of calculating the magnetic loss is correct when applied to armatures—a point which Dr. Hopkinson was not able to decide positively; but by assuming that it is applicable, then, by subtracting the calculated hysteresis loss from the ascertained total power unaccounted for, the eddy-current loss is obtained; and as this process gives the hysteresis as accounting for only about 40 per cent. of the total loss in the armature core, it shows that the eddies could not by any means be considered unimportant, although the lamination was carried out to the extent of making the iron about 0·02 inch thick.

I now wish to describe a method by which the amount of loss due to each of the two causes may be ascertained experimentally in the workshop on a practical scale. It is a very simple one, although not, perhaps, capable in all cases of the highest scientific accuracy.

(*a.*) First run the dynamo on open circuit at various speeds (two readings will suffice), with the fields unexcited, or, better still, entirely absent, and measure the power absorbed in friction. Plot this value to any suitable scale, as O M in Fig. 3, the ordinates representing horse-power, while the abscissæ represent revolutions per minute.

In the figure the mechanical friction, being a quantity with which in this investigation we are not directly concerned, is plotted below the base line O M′, in order that the other losses may be more clearly seen.

(*b.*) Next run again on open armature circuit with the brushes removed, and the field normally excited, and measure the power absorbed. This will be the total power dissipated in friction, hysteresis, and eddy-currents, if any, and must be plotted as

* *Phil. Trans.*, part 1, 1886, p. 331.

O N. Then the ordinates lying between O N and O M′ represent the sum of the losses due to hysteresis and eddy-currents, and somewhere between O M′ and O N must lie a line O P, separating the hysteresis from the eddy-currents.

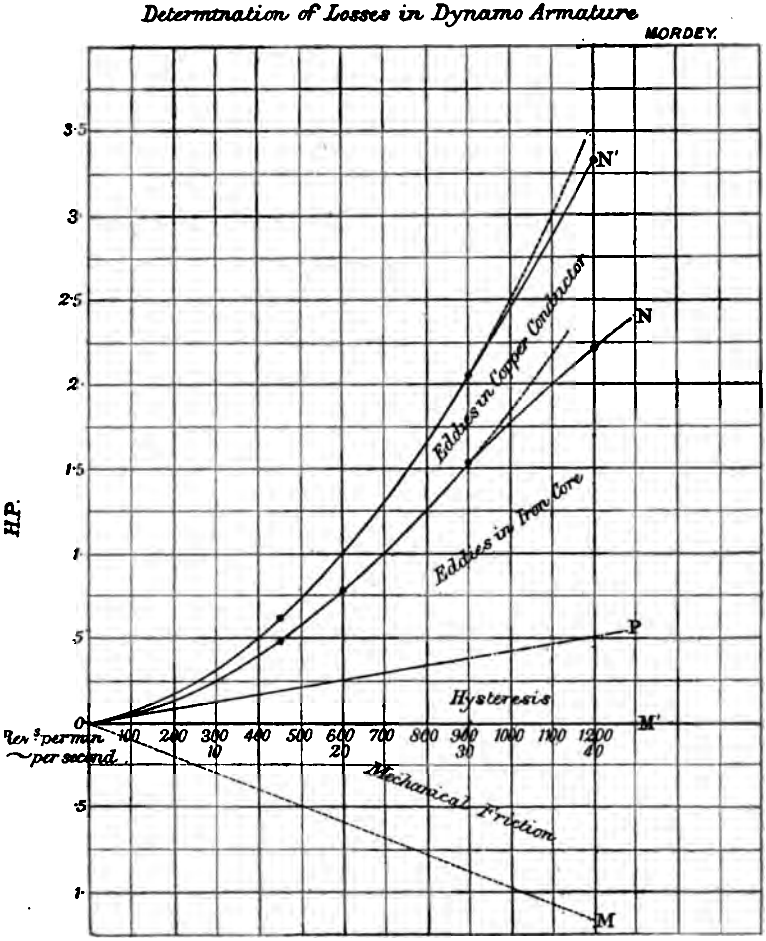


FIG. 3

We desire to find the position of this line. The difference between the rate of increase of the electric and magnetic losses enables us to separate them from each other. It may be taken that, with any given constant excitation, the loss from hysteresis is directly proportional to speed, within certain limits to be here-

after stated. The resistance (that of the iron) being constant, the eddy-currents must vary as the first power of the speed, and the loss therefrom as the square of the speed.*

It will now be seen that it is easy to separate the values which are directly proportional from those which are proportional to the square of the speed.

Professor S. P. Thompson, to whom I submitted this method, has kindly given me the following concise formulæ for finding the quantities:—

P and $P_1$ are respectively the combined magnetic and electric losses at the speeds S and $S_1$.

$$k = \text{magnetic loss};$$
$$k_1 = \text{electric loss}.$$
$$k = \frac{P_1 S^2 - P S_1^2}{S^2 S_1 - S S_1^2};$$
$$k_1 = \frac{P S_1 - P_1 S}{S^2 S_1 - S S_1^2}.$$

Then, at any speed $n$,

$$\text{Hysteresis} = k n;$$
$$\text{Eddies} = k_1 n^2.$$

If armature cores are run in an excited field before winding, it can be determined to what extent their lamination is satisfactory, and whether the iron is of good quality; and then, after winding the armature conductors, another test shows what the additional increase in the loss is, and separates the effects of eddy-currents in the core from those in the conductor, and the hysteresis from both. The method may also be useful to investigate the losses in the cores of armatures of different types, or differently built up, as, for instance, with iron wire or iron sheets.

This method has a useful application in rendering it possible to separate not only the magnetic loss from the electric loss in the iron, but to determine the waste by eddies in the copper winding of armatures.

It is usually entirely a matter of opinion as to how much of

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* Prof. Thompson's "Dynamo-electric Machinery." 3rd Edition, p. 102.

the power absorbed in running an armature on open circuit is due to any of the various causes referred to; and now that it is customary to wind armatures with heavy conductors, it becomes very desirable to be able to find in any particular case what proportion of the loss is in the copper, and what in the iron.

In Fig. 3 are shown some results obtained with a four-pole (direct-current) Victoria dynamo run at various speeds up to 1,200 revolutions per minute, with constant excitation. Power readings were taken by means of a White's transmission dynamometer, constructed by the Brush Corporation for the purpose of testing dynamos; and the H.P. absorbed in mechanical friction in dynamo, belt, and dynamometer was subtracted from the power spent electrically and magnetically.

To reduce errors in the dynamometer readings the dynamo was fitted with two pulleys, one twice the diameter of the other. This allowed of the dynamometer being run at the same speed, while two different speeds of the dynamo were obtained by changing the belt from one pulley to the other. Readings were taken at four speeds, viz., 450, 600, 900, and 1,200 revolutions per minute, corresponding to 15, 20, 30, and 40 ∿ per second. The position of O P was readily found by "trial and error," this being a shorter process for a concrete instance than the application of the general formulæ. The assumption on which this method of testing is based was found to be correct up to 900 revolutions. The reading at the higher speed of 1,200 revolutions, or 40 ∿, shows that disturbing influences began to affect the results by reducing the rate of increase of the losses. The continuation of the curve O N in a full line shows the actual loss; while the continuation in a dotted line, which is a parabola on O P as an abscissa, shows what the loss would be if the rate of increase holding for low speeds were maintained.

This shows that this method is perfectly applicable to ordinary direct-current machines—for instance, to two-pole dynamos up to 1,800 revolutions per minute, or 30 ∿ per second—but that it is less useful for alternators.

The disturbing causes are probably rather complicated. They may be due partly to self-induction in the circuits of the eddy-

currents, to lower magnetisation (although this effect, if present, is very slight, as was shown by the E.M.F. being almost perfectly proportional to speed), and to vibration. "Viscous hysteresis," if it exists, will raise the magnetic loss. There may be such an effect, in spite of the lowering of the curve, but it cannot be recognised, on account of the unknown variation of the eddies, at the higher speed. Iron-cored alternators working at high periodicities may be tested in the following way, which will give some approach to correct results:—Let the test be taken at a low speed (not more than 30 ∿). This will give the hysteresis per period, and the eddies at that speed. Then, if at any higher speed the E.M.F. is proportional to speed, it may be assumed that the magnetic induction is the same as before, and the hysteresis per period will at any rate not be less than at the lower speed. It may be more. The difference between the calculated hysteresis, O P, and the ascertained total loss, O N, will be the eddies; or, if viscous hysteresis is a fact, it will be extra loss due to viscosity plus that due to eddies.

Heating of the armature core, and consequent increase of the resistance of the iron, is not responsible for the bending down of the curve O N in Fig. 3 (although it would have that effect under ordinary circumstances), as the experiments did not occupy sufficient time to allow of any sensible rise of temperature. The effect is probably very complex and obscure, but it is one that well deserves study, on account of its present industrial importance.

It should be stated that the dissipation of energy (which has been spoken of as being in the armature) may be partly in the field magnets. This was, however, not the case in the experiments referred to. The conditions necessary for the prevention or reduction of eddies in the iron of field magnets are now pretty generally understood,* but it should be observed that it is not by any means as easy to check such waste in alternators as in ordinary dynamos. It is both better and easier to prevent than to cure them.

* See numerous practical notes on this subject scattered through Professor Thompson's "Dynamo-electric Machinery."

If a good transmission dynamometer is not available, results almost, or perhaps quite, as good may be obtained by running the machine at various speeds by means of an electric motor, and measuring the power required by the motor—first with the fields of the machine unexcited, to get the mechanical friction, and then with the fields excited, to get the total loss. The motor should then be run alone, to find what is absorbed in driving it at various speeds. The increase in watts over these last values, with the internal losses in the motor conductors calculated out and subtracted, will give, with a very fair degree of accuracy, the energy dissipated in the machine under test.

The upper curve, O N′, was plotted from readings taken after the armature was wound, and the difference in the ordinates between O N and O N′ shows the eddy-current loss in the copper conductor at various speeds. This loss, like that of the eddy-currents in the iron, rises as the square of the speed up to 900 revolutions, or 30 ∿, above which it rises rather less quickly. The full line O N shows the actual loss, the dotted line above it showing what it would be if the rate of increase were maintained. The difference is no doubt due to self-induction.

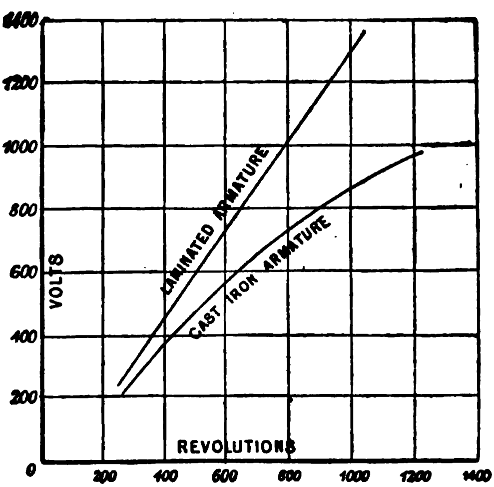


Fig. 4.

That the effect of self-induction is not noticeable unless the eddy-currents are considerable is in accordance with experiments

that I published some years ago in connection with the Brush dynamo.* The results then obtained are shown in Fig. 4. The straight line connects speed and the potential difference at the terminals with the laminated armature; the lower and slightly curved line shows the potential difference with the old form of cast-iron armature, which had only coarse divisions, scarcely meriting the term "lamination." The curve shows that the E.M.F. rose less quickly than the speed on account of the serious generation of eddy currents.

In the laminated armature, although, of course, there were still eddies, their effect was not sufficient to cause any bending down of the E.M.F. line at any practicable speed of driving.

Coming now to an examination of the results obtained in the experiments, and shown in Fig. 3, we see that the hysteresis loss in this particular dynamo—which I should mention is capable of working continuously with a normal load of 18,000 watts—was 0·195 H.P. at 450 revolutions, and 0·39 H.P. at 900 revolutions the latter corresponding to 30 ∿ per second.

The magnetisation† was about 12,300 c.g.s. lines—not by any means a high density. The armature had a ring core, nearly square in cross-section, of twelve sq. in. area, and contained 615 cub. in., or 10,086 cub. centim. of iron strip 0·012 in. thick (No. 30 B.W.G.) separated by paper. This is a degree of lamination beyond which it is inconvenient to go. The peripheral portion was wound with this iron in parallel bands $\frac{1}{2}$ in. wide, in order to reduce eddies caused by lines entering the periphery. The main inner portion of the core was of wider strip.

We find from the figures that the loss in hysteresis at 900 revolutions was as follows:—

0·39 H.P. = 291 watts = 0·473 watts per cub. in., or 0·029 watts per cub. centim. As the periodicity was 30, the dissipation of energy per complete period per cubic centim. was 9,613 ergs or 0·001 watts, nearly.

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* *The Electrician*, October 2nd, 1885.

† The use of the word "induction" in so many senses is apt to cause confusion. "Magnetisation" would perhaps be better than "induction," "magnetic induction," "density," "magnetic density," etc.

It is interesting to compare this result, obtained in what may seem a rather crude manner, with the values given in the papers referred to. Prof. Ewing says (*Phil. Trans.*, p. 553) ". . . the "double reversal of a strong condition of magnetism in soft iron "involves the expenditure of about 10,000 ergs per cubic centim." He gives the energy as 9,300 ergs, with a magnetisation of 13,190 in very soft annealed iron. Dr. Hopkinson gives 13,356 ergs, with a magnetisation of 18,250.

Much of course depends upon the quality and hardness of the iron, but my results come out very close to those of Prof. Ewing and Dr. Hopkinson. This may be taken as proving the method capable of considerable accuracy. The eddies in the iron, it will be seen, are by no means to be neglected, even with the very fine subdivision that was used.

The eddies in the conductor were rather greater than usual, as the armature, being intended for a very low speed, had a larger amount of copper on it than usual.

The bearing of these curves on iron-cored alternators and on open circuit-working generally is obvious. With a magnetisation of 12,000, the loss in hysteresis alone, at 100 ∿, would be over 1·5 watts per cub. in. of iron core, and the eddies would amount to a good deal more. I will only add that the eddies, both in the iron and in the copper, are greatest when no external work is being done, as was the case in my experiments. They are reduced by the effects of self-induction when the circuit is closed. A difference exists in this respect between generators and motors, a difference in favour of generators.*

I will not further extend this paper by dealing at greater length with this part of the subject.

## APPENDIX.

Extract from remarks by Mr. Mordey in the discussion on Mr. Kapp's Paper on "Alternate Current Machinery."—*Proc. Inst. C.E.*, March 5th, 1889.

"Alternators might be divided into two classes, those which had iron in the armatures, and those which had not. As his own machine, made by the Brush

* *Phil. Mag.*, Jan., 1886, p. 26. "The Dynamo as a Generator and as a Motor."

Corporation, and that of Ferranti were the only examples of the latter class described by the author, he might be permitted to mention the reasons which had led him to avoid the use of iron. He thought the makers of dynamos would agree that almost all the ills that the dynamo was heir to were due to the presence of the iron in the armature. Its use in direct current machines might be said to be a necessary evil, as, although attempts had been made to do without it, all successful types of such machines required it for structural purposes. Iron in armatures when worked at a high magnetic density, and with rapid reversals or variations of magnetism, became heated and wasted a good deal of power. In alternators this objection applied with very much greater force than in direct-current machines, for in the latter the reversals of magnetism were comparatively slow. Thus the first result arrived at in quite recent practice is that the magnetic density that can be used in iron-cored alternators must only be about one-half that employed in continuous-current armatures. This alone meant a considerable increase in the size of armatures, without any gain in output or efficiency. Although the loss per cubic inch was reduced by decreasing the magnetic density, the armature had to be made larger to compensate for it, and the total loss was actually increased, not reduced. Iron was used to reduce the magnetic resistance, to afford mechanical support, and to introduce self-induction into the circuit. The latter, an evil in itself, was said to be a modern necessity, caused by the convenience of working alternators parallel. Self-induction, for this or any other purpose, had probably not entered the minds of most designers at the time they first produced their machines, except, perhaps, as an objectionable feature, which they could not get rid of; but now it was being brought forward as an advantage. If iron was indispensable for this purpose, which he was not prepared to admit, it could readily be inserted in some part of the circuit where ample space and cooling surface could be provided, and from which it could be easily removed when not required, that was when it was only necessary to run one machine. The armature was the very worst place to put the iron, for there its presence was the most objectionable, and it was there least under control. Referring to the magnetic resistance of armatures, it would be seen that there were broadly two ways of arranging the armature-coils and the field-poles. The poles could be arranged north and south, facing one another, the armature-coils having their axes parallel to the lines of force; or the poles could be arranged in successive order round an iron ring or drum armature-core, on which the armature coils were wound or placed. If iron cores were used in the former case, the output would be only about one-half of what was obtained with air-cores, on account of the necessity to work with reduced magnetic density, while at the same time magnetic and electric disturbances would be set up, both in the armature and field, wasting power, causing objectionable heating, necessitating lamination of the fields, and demanding an increase in the exciting power. These reasons were sufficient to explain why he had not used iron cores; but there were others. Magnetic resistance, between pole and pole, was made up partly of the necessary clearance space, partly in most cases of the space occupied by the copper conductors, and partly of the iron or other core. Now, if in air-cored armatures the core could be so arranged, as was quite possible, and was done in his own machine, that it occupied no more space han was taken by twice the depth of the conductors in iron-cored armatures, it

would be evident that the former gained instead of lost by the omission of iron. They gained not only in magnetic resistance, but in all the other points to which he had referred, and they had the very great advantage that there was nothing to limit the magnetic density at which they could be worked except the magnetic saturation of the field-poles, the only losses to be met and provided for being the ordinary electrical losses in the conductor caused by the passage of the current, and a small waste by eddy-currents in the conductors and supports. Such armatures had the further advantages that their self-induction was negligible, their power of self-regulation was very great, and that on open circuit or light load they ran very economically; whereas it was well known that, when iron cores were used, the losses under such circumstances were often very serious indeed. He thought it was quite unnecessary to use iron cores for the purpose of obtaining mechanical support for the armature-winding. The holding and driving, with a very limited clearance, of an armature consisting of thin iron, separated by such unmechanical substances as paper, and overwound with copper wire, itself covered with cotton, and expanding and throwing out from the combined effects of heating and centrifugal action, was a problem that had never been very successfully solved in direct-current machines, and was certainly to be avoided, if possible, under the still more onerous conditions obtaining in alternators, where the electrical pressure was usually much higher than in direct-current machines, and in which, as the author showed, the maximum mechanical strain on the wires was greater. He ventured to think it was much the best to make the armature stationary as he had done. It then had only to resist the tangential drag of the field. He thought a great mistake was made in some alternators in using Pacinotti projections. In all cases there should be, as nearly as possible, a steady magnetic flux in the field. This could not be done if projections were used. The Zipernowsky and Parsons machines were faulty in this respect. It was impossible to tell, by inspection of the former, which was the armature and which the field. They seemed quite interchangeable, and this was of course not right. In these two machines it would be seen that the magnetic resistance varied very much, as the iron projections passed through the cycle. This necessarily led to losses in the field, the lamination of which recently introduced in the Zipernowsky machine showing that such losses must be serious. There was one other machine to which he wished to allude, and that was to him a very interesting machine indeed. He referred to the alternator (Fig. 28) shown and referred to by Professor Forbes as in some respects a developement of his alternator. In that machine Professor Forbes used the form of field which he (Mr. Mordey) had in his alternator; but he used it in combination with a form of armature having, like the field, only one coil. He ventured to point out that, for a machine with an iron armature, that form of alternator had some advantages. The armature might be described as a Gramme ring with the copper inside and the iron outside. The magnetic flux was, or might be made, perfectly even, the air-gap was very short, the magnetic resistance very low, and it followed that the excitation required was very small. The author had alluded to a very interesting and important matter, probably new to most people, as it certainly was to him, namely, that the armature-conductors in alternators showed a tendency to heat

much more than in the continuous-current machines, but in this machine described by Professor Forbes there was hardly any question of eddy-currents in the armature-conductor, as the coil was not swept by the lines of force. Further, it was evident that a very low current density could be used in the armature-coil without sensibly affecting the size, cost, or output, and it was also clear that the insulation of the armature-coil was a very simple matter. Another good point was that neither the armature-coil nor the field coil need be rotated. For a machine with an iron magnetic circuit he thought this was a very good one; and he had only one other remark to make about it, and that was that he had himself invented and patented it some time ago! It was, he thought, a natural development of his alternator. He was glad to find that Professor Forbes had had the same idea, because that showed it to be a good one. Much had been said about the form of the wave yielded by alternators. He had some time ago made an experiment with the first of his machines, using the method described by Professor Ayrton, and found that the curve (Fig. ) was almost a sine-curve. On the subject of the lag in transformers, to which Professor Ayrton had also alluded, he might be allowed to mention that, in the discussion of another paper by the author, he had first stated the fact,* and had described a very simple experiment, showing that the primary and secondary currents reached their maxima and minima practically at the same time."

On the motion of the President, a hearty vote of thanks was unanimously accorded to Mr. Mordey for his very interesting and valuable paper.

The PRESIDENT announced that the discussion on Mr. Mordey's paper would take place at an extra meeting to be held on May 30.

A ballot took place, at which the following were elected:—

*Member.*

George Hookham, M.A.

*Associates.*

| | |
|---|---|
| John Boyes. | Philip Peters. |
| William Foggin. | M. G. Simpson. |

Edwin Thornton.

*Students.*

| | |
|---|---|
| W. H. Merriman. | P. Hawkins. |

The meeting then adjourned.

* *Journal*, vol. xvii., pp. 215, 216.

The One Hundred and Ninety-sixth Ordinary General Meeting of the Institution was held at the Institution of Civil Engineers, 25, Great George Street, Westminster, on Thursday evening, May 30th, 1889—Sir WILLIAM THOMSON, F.R.S. (L. & E.), D.C.L., President, in the chair.

The minutes of the Ordinary General Meeting held on May 23rd were read and approved.

The names of new candidates for election into the Institution were announced.

The PRESIDENT: In the ordinary course I should propose that these names be suspended; but, as this is the last meeting before the recess, it has been usual in such cases to propose that the candidates be balloted for the same evening, I beg to move that that course be followed in the present case.

The proposition was agreed to unanimously.

The following transfers were announced as having been approved by the Council:—

From the class of Associates to that of Members—

Richard Lewis Cousens.

From the class of Students to that of Associates—

Arthur Henry Lea.

The PRESIDENT: Before the discussion on Mr. Mordey's paper takes place I will ask Mr. Mordey if he desires to make any further observations in reference to the subject with which it deals.

Mr. MORDEY: As time did not permit of my referring at the last meeting to all the apparatus that I have here, I wish to be allowed to do so now.

In the first place, I would say that the series of experiments described in the first part of the paper was not quite complete. In order to render it complete it was necessary that one of the

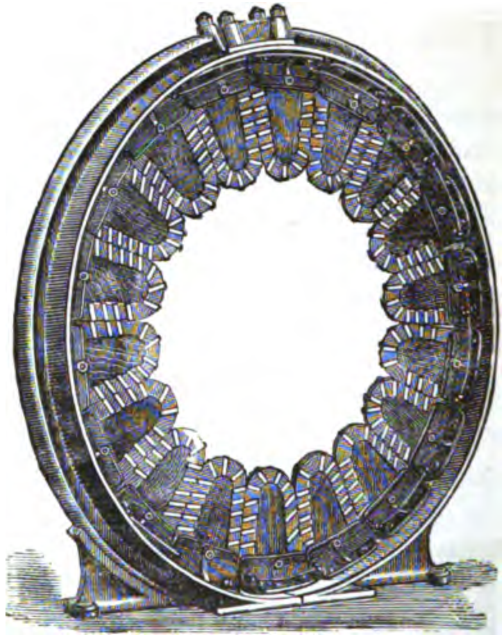

FIG. 5.—Armature of Alternator.

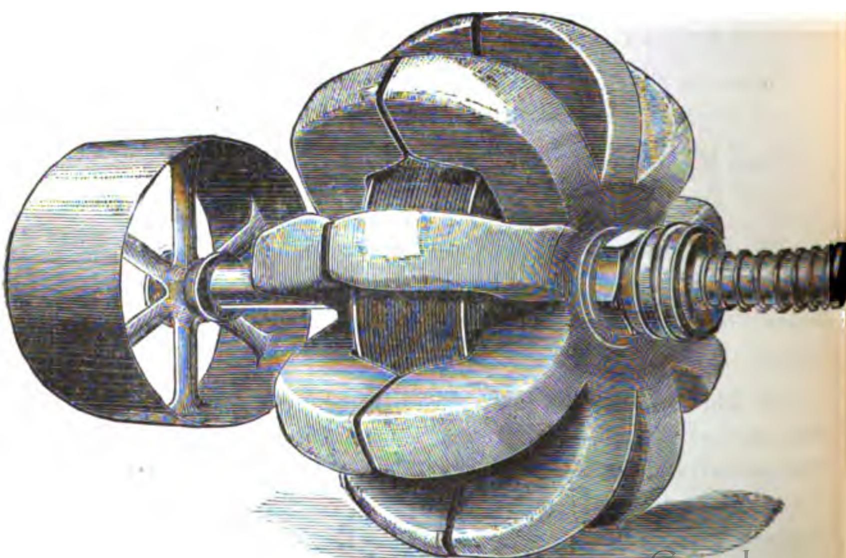



FIG. 6.—Field-Magnet of Alternator.

alternators should be run as a motor, developing mechanically the full power that it was designed to give electrically as a generator. Since the last meeting I have been able to do this, and have run a machine which is intended for an output of 37,500 watts, as a motor giving 50 horse-power effective.

The armature now before you is out of a similar machine (see Fig. 5). Fig. 6 is useful as showing the exact magnetic arrangement of the field-magnet. As a matter of fact, this figure illustrates the magnet of the first machine that was built. In the later ones, such as are shown by Figs. 8 and 9, there is a slight difference of construction, the horns of the magnet, which

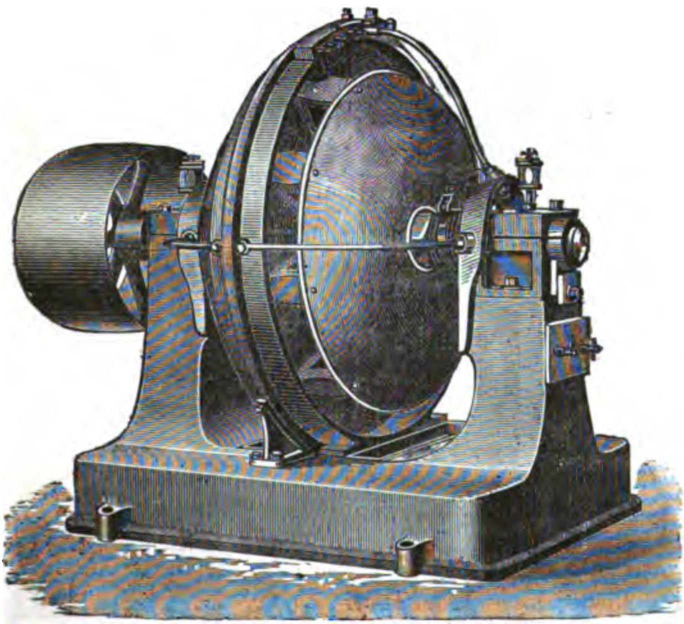

FIG. 7.—Mordey-Victoria Alternator.

are seen as quite separate in Fig. 6, being joined by a cast web on the outside, for the prevention of air disturbance when running. The whole machine is shown in Fig. 7. The armature has 18 coils, and is wound for 2,000 volts. Here is one coil [shown] which you will find only weighs two or three pounds, complete with its supports. Each coil is capable of working at about 3 horse-power.

A machine of exactly the same type, but of a larger size, is shown by Figs. 8 and 9. This machine is for an output of 100 horse-power, as generator or as motor. It runs at 500 revolutions.

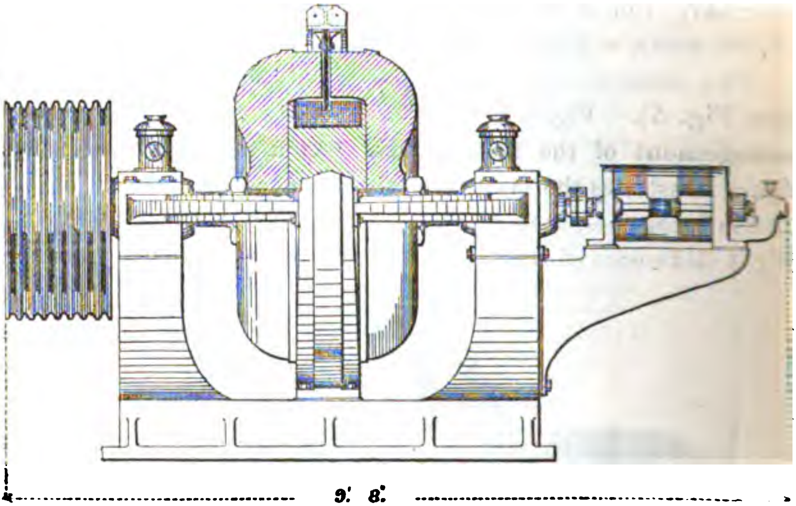


Fig. 8.—100 H.P. Alternator with Exciter.

Fig. 8 is a longitudinal elevation, partly in section, so as to show the construction of the magnet, and the method of mounting

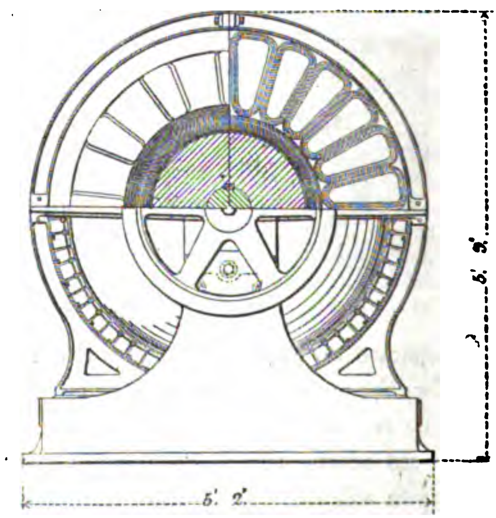


Fig. 9.



the armature coils. The exciter, which is driven direct, is carried on a bracket at one end of the machine.

Fig. 9 is an end elevation, partly in section. The armature coils are shown at the right of the upper part of the figure, while at the left they are removed in order to show the pole faces.

I have here an automatic regulator, which may be used either for alternate or direct currents. For alternate-current

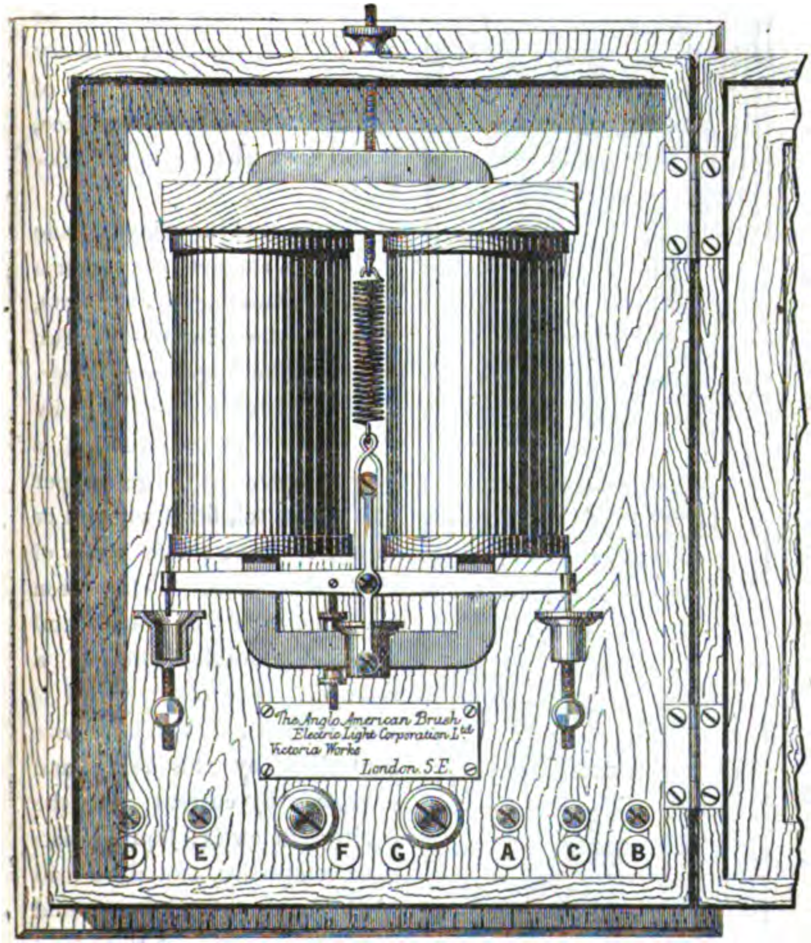


Fig. 10.—Compound Relay.

working it is made to act on the field of the exciter, which it varies so as to maintain a constant potential difference either at the terminals of the alternator, or at any distant point of the mains.

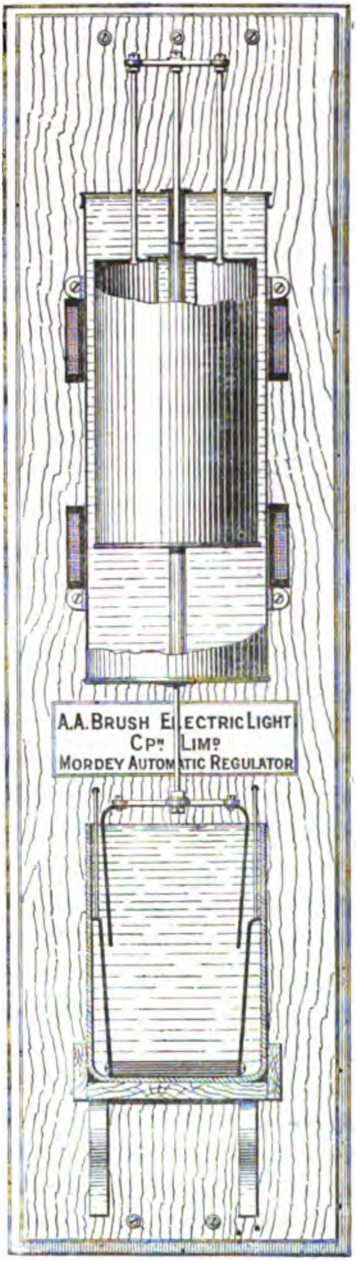


FIG 11.—Regulator.

It comprises a relay (Fig. 10) and a regulator (Fig. 11). The former is the brain, the latter the muscle. The relay (Fig. 10) is a core and solenoid arrangement very simply made to be as sensitive as possible. Every care was taken to get rid of reaction, and, as a consequence, it will work with a range of one-half per cent.

The core of the relay is made to move a contact-making arm which, by two mercury contacts, controls the circuits of two coils on the regulator proper. This regulator is illustrated by Fig. 11. The construction is very simple. A large copper or brass vessel, filled with oil or water, contains a float made of thin sheet-iron. This iron vessel acts both as a float and as the movable core of a solenoid. The outer vessel is provided with two coils of wire controlled by the relay and energised by current from the exciter. Below is a liquid resistance comprising two fixed lead plates in dilute sulphuric acid or other solution. A third lead plate, bent down at the ends to approach the fixed plates, is supported by the float, and by its movement varies the liquid resistance.

The arrangement, besides being simple and unlikely to get out of order, is very sensitive, the float providing an almost frictionless method of support, without any troublesome mechanism.

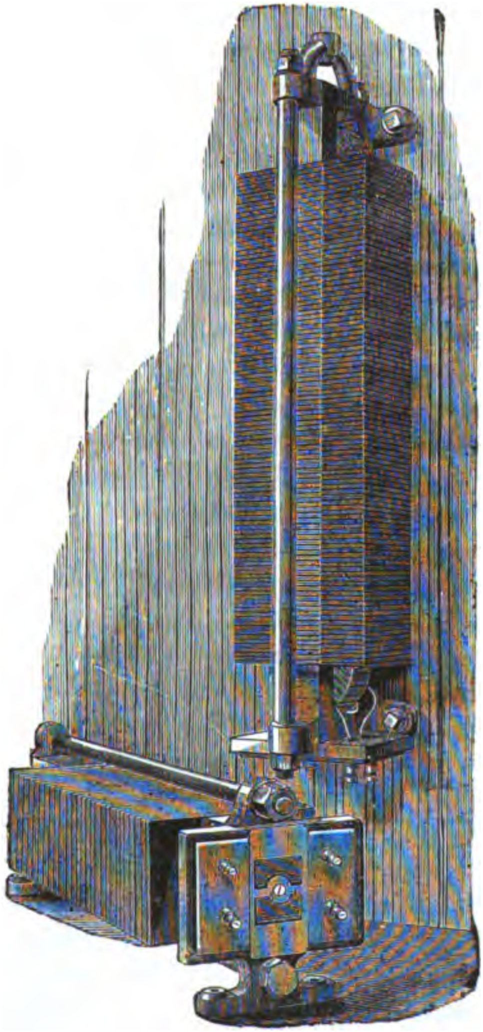

FIG. 12.—Mordey Transformer.

The float is balanced to remain in any position, from which it is only moved when an impulse is received from the relay.

I said that every care was taken to prevent reaction in the relay. The reverse is done with the regulator. By allowing

considerable inductive reaction in connection with the regulator coils, it has been possible not only to stop the motion of the floating core very quickly, but actually to enable it to make a slight backward movement on the cessation of the impulse from the relay. In this way any tendency to "hunt" may be checked. It should be explained that the figures (10 and 11) are not to the same scale. The former is much less reduced than the latter.

Some transformers, impedance coils, and a synchroniser are on the table. I need not pause to describe them further. The transformers (Fig. 12) have already been explained at a previous meeting. (*Journal*, vol. xvii., p. 115.)

I have said that I did not approve of iron in the armatures of alternators, but I wish, nevertheless, to be allowed to describe a machine with iron, not in, but outside of its armature. I have called it a "single-coil alternator." It is not a machine that I have made, but it is in some respects an outcome of the machine already shown to you. I simply describe it as showing one way of making an alternator. In the first machine (Figs. 5 to 9) there is only one field coil for the whole excitation, instead of the large number of coils usually employed. This principle has been carried further in the design shown in Figs. 13 and 14, and in the model before you, which illustrate a machine in which there is only one simple stationary field coil, and one simple stationary armature coil, for any speed or periodicity. The figures and model probably explain themselves. The field winding surrounds a cast-iron rotating cylinder, having gaps or notches at the ends so as to allow the lines of force to pass alternately round the field winding and round the field and armature windings. In this way, with every part fixed except the rotating cylinder, an alternating E.M.F. is obtained in the armature coil. [*Model shown.*]

Fig. 13 is taken from a set of drawings made about eighteen months ago, and illustrating several ways of carrying out the principle.

A is the armature coil, F the field coil. The field-magnet poles are shown at *n n*, *s s*. The laminated iron masses, which carry the lines of force alternately outside and inside the armature coil, are marked I I and K K.

I must mention, as one of the coincidences that so often occur in these matters, that a few weeks ago, in the discussion on Mr.

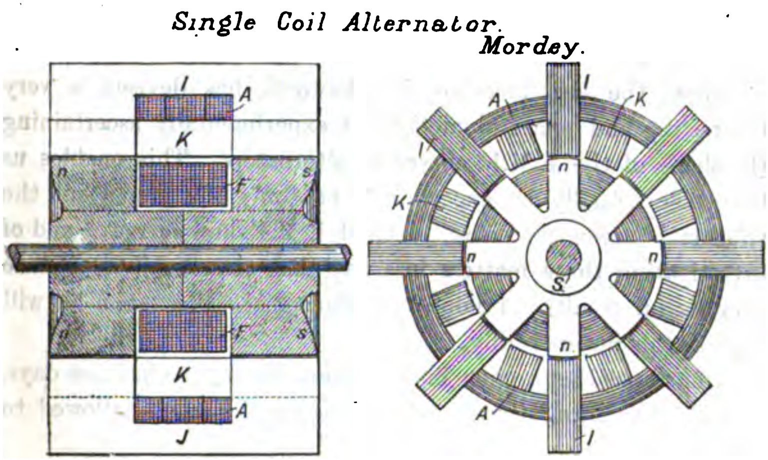


Fig. 13.

Kapp's paper at the Institution of Civil Engineers, Professor Forbes described an almost identical machine—a machine similar in nearly every respect to Fig. 13.

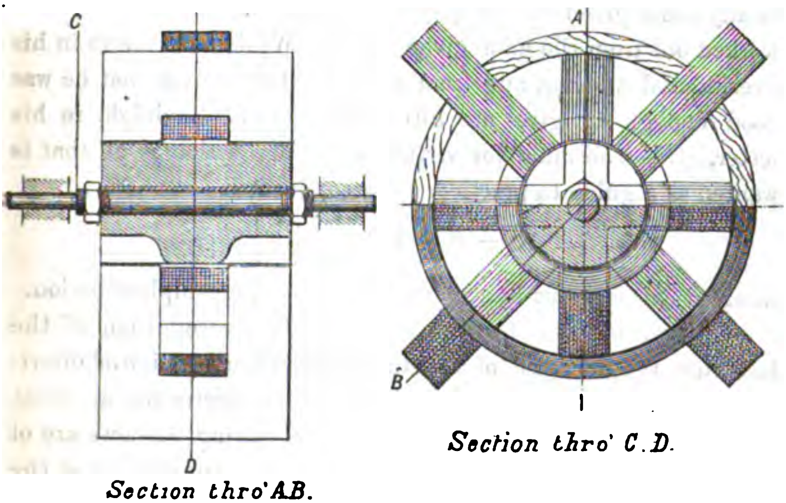


Fig. 14.

I should prefer the later form seen in Fig. 14, for the reasons that the field coil is stationary as well as the armature,

and that it is very easy to remove the field-magnet without disturbing any other part of the machine. Some of the qualities of this type of machine are further alluded to in the Appendix (page 629).

Since the last meeting, Mr. Raworth has devised a very interesting and practical method of experimentally ascertaining the shape of the E.M.F. curves of alternators. This enables us to decide actually by experiment, not only the shape, but the relation between the curves, both of E.M.F. and current; and of investigating these matters in a way that cannot fail to produce very useful results. I hope that during the discussion he will explain this method fully.

I have received a good many letters during the last few days, and, as they form part of the discussion, I may be allowed to refer to one or two of them.

Lord Rayleigh kindly sends me a copy of an article in the *Philosophical Magazine* for May, 1886, in which he deals with the question of virtual resistance of straight conductors with alternating currents, and arrives at conclusions which are practically those given in the paper. My figures, as I stated, were worked out from the data given by Sir William Thomson in his Presidential Address, and from some further figures that he was good enough to give me afterwards. Lord Rayleigh, in his letter, gives a formula for virtual resistance which is all that is wanted as a guide to practice. For copper it is

$$R^1 = R \left\{ 1 + \frac{1 \cdot 2\, a^4}{10^5\, \tau^2} \right\},$$

where $a$ is the radius in centimetres, and $\tau$ the complete period.

The article seems to have been the first recognition of the fact that the increase of resistance due to non-uniform distribution of current over the section of a conductor was an effect that might be serious in practice. The following extracts are of interest as bearing on more than one matter of importance at the present time:—

Referring to the mathematical examination:—"These "results are merely very special cases of a general law, from "which we may learn that as the frequency of alternation

"gradually increases from zero to infinity, there is a steady rise "of resistance and accompanying fall of self-induction. The "application of the general idea to the present case is very "simple. At slow rates of alternation, the distribution of "current, being such as to make the resistance a minimum, is "uniform over the section; and this distribution, since it "involves magnetisation of the outer parts of the cylinder, leads "to considerable self-induction, especially in iron. On the other "hand, where the rate of alternation is very rapid, the endeavour "is to make the self-induction a minimum irrespective of resist-"ance. This object is attained by concentration of the current "into the outer layers. The magnetisation of the conductor is "thus more and more avoided, but, of course, at the expense of "increased resistance. We may gather from the general argu-"ment . . . that as $p$ increases without limit, R′ also be-"comes infinite, while the part of L′ depending upon the "magnetisation of the conductor tends to zero.

"The increase of resistance proper (not merely of the "'throttling' due to the combined effect of resistance and self-"induction) in iron wires of moderate diameter subjected to "varying currents is one of the most striking of Professor "Hughes's results. So far as I am aware, neither Maxwell nor "any other theorist had anticipated that the alteration of resist-"ance would be important under such circumstances."

Lord Rayleigh objects, as a matter of nomenclature, to my use of the word "periodicity," and prefers the word "frequency." He remarks that "the great point is to insist that vibration shall "always mean complete vibration." That was precisely my reason for suggesting a word that was not likely to lead to confusion. "Vibration," "frequency," "reversal," "alternation"—the words in general use—all carry an uncertain sound. They may mean one or two currents, and I venture to disagree with Lord Rayleigh when he thinks it better to use a word which requires us to insist that it shall mean something which it does not necessarily mean if left to itself. As an instance of the lengthy explanation which I wish to avoid, I may refer to Lord Rayleigh's paper, from which I have already quoted. He there says, "From an alternate-

"current machine we may have currents of period ·01 second (100 "positive and 100 negative pulses per second)."

But if my use of the word "periodicity" is not allowable, let me put in a plea for the sign ∿ as rendering confusion impossible.

Professor Ewing writes from Dundee criticising my paper. He says :—"I am sorry I cannot be present to hear it discussed; "if I were, there are just two remarks I should like to make "about your plan of distinguishing between loss of energy by "magnetic hysteresis and loss by eddies in the armatures of "dynamos. Your plan is, of course, based on our taking the loss "by hysteresis (under constant excitation of the field) as propor- "tional to the speed, and the loss by eddies as proportional to "the square of the speed. But we cannot be sure that the loss "by hysteresis is proportional to the speed, especially when the "speed is high. The process of assuming magnetisation, on the "part of soft iron, takes some time, and we have little or no "information at present as to how far this fact affects the loss of "energy by magnetic hysteresis in rapidly performed cycles.

"Again, as to the eddy current, you say, 'The resistance "'(that of the iron) being constant, the eddy currents must vary "'as the first power of the speed, and the loss therefrom as the "'square of the speed.' But will not a cause which you refer to "further on—viz., the impedance of the eddy circuits—operate "even at low speeds to make the strength of the eddies increase "less rapidly than the speed, and the loss of energy through "them, consequently, to increase less rapidly than the square of "the speed?

"The general effect of this will be to give the magnetic "hysteresis, as estimated by your method, a lower value than it "ought to have.

"I mention these points only by way of showing that the "method is open to some little uncertainty. The fact that in the "example you have taken the hysteresis has a calculated value "just such as one would expect it to have no doubt goes a good "way to show that the method is in the main a sound one."



Now, I think, these objections raised by Professor Ewing are

really dealt with in the paper. As to the first point it may be taken that for any range of speed, so long as the E.M.F. is found to be proportional to speed, the assumption of magnetisation must be considered as being the same throughout (see page 624).

As to the second point, that the impedance of the eddy circuits will operate even at low speeds to make the strength of the eddies increase less rapidly than the speed, it will be found, on referring to Fig. 3, and to the explanation given, that the increase is only proportional up to 30 ∿ per second (see page 625). Below this the law on which the method is based apparently holds good. At higher speeds there is a falling off shown by the difference between the full line ON and the dotted line springing from it.

I quite agree with Professor Ewing that these points show the method to be open to some uncertainty, but an examination of the results obtained experimentally show clearly what limitations it is necessary to impose upon the application of the method, and within those limitations I venture to submit that the method is quite applicable. I should mention that the curves (Fig. 3) were from many careful and concordant tests.

Mr. Zipernowski writes that he intends taking part in the discussion in writing. His experience, and that of his firm, has been very large, and I hope that we shall have his contribution in the Journal, with the paper.

Dr. JOHN HOPKINSON [*read in his unavoidable absence and on his behalf by Professor George Forbes*]: A great deal might be said on many of the points which are ably discussed in Mr. Mordey's valuable paper. I shall confine myself to brief remarks on one of them. Dr. Hopkinson.

Some misapprehension may exist as to the conclusions which legitimately follow from the theory of running alternate-current generators parallel, which I gave five or six years ago. To obtain a great control of one machine upon another it is not of itself desirable to have any large self-induction, as Messrs. Kapp and Forbes appear to think, nor is it desirable to have it as small as possible, as Mr. Mordey appears to think, when he says, "If it (self-" induction) were absent probably the machine would run parallel

Dr. Hopkinson.

"all right." The machines will best control each other when, $\frac{2\pi\gamma}{T}$, $\gamma$ being the self-induction, is equal to the resistance of the armature circuit and the leads to the junction with the leads of the other machine. This is an obvious immediate consequence of the equation of paragraph 4 of my paper read before this Institution in November, 1884. If Mr. Mordey adds a substantial resistance to the armature-circuit of each machine, such, for example, as the resistance of a feeder, he will find it less easy to run parallel. He will then find the difficulty somewhat diminished by adding a little self-induction to each armature-circuit.

In paragraph 3 of the same paper I considered connecting a machine to a circuit of different potential, and I pointed out the limit within which such a machine would work as a motor. Let us consider the closely analogous problem of working two machines of widely different fields connected togther as Mr. Mordey has done. Let the machines be identical, excepting that one shall have double the field of the other, and let them be simply connected together with no external work, the equation of currents, modifying those of paragraph 1 of my paper to suit the case, may be written—

$$2\gamma\dot{x} + 2rx = 2\,\mathrm{E}\sin.\frac{2\pi t}{\mathrm{T}} + \mathrm{E}\sin.\frac{2\pi(t-\tau)}{\mathrm{T}},$$

$$x = \frac{\mathrm{E}}{r^2+\left(\frac{2\pi\gamma}{\mathrm{T}}\right)^2}\left\{r\sin.\frac{2\pi t}{\mathrm{T}} - \frac{2\pi\gamma}{\mathrm{T}}\cos.\frac{2\pi t}{\mathrm{T}}\right\}$$

$$+\frac{\frac{1}{2}\mathrm{E}}{r^2+\left(\frac{2\pi\gamma}{\mathrm{T}}\right)^2}\left\{r\sin.\frac{2\pi(t-\tau)}{\mathrm{T}} - \frac{2\pi\gamma}{\mathrm{T}}\cos.\frac{2\pi(t-\tau)}{\mathrm{T}}\right\}$$

work done by machine of greater electromotive force in generating electricity

$$= \frac{\mathrm{E}^2 r}{r^2+\left(\frac{2\pi\gamma}{\mathrm{T}}\right)^2} + \frac{\frac{1}{2}\mathrm{E}^2}{r^2+\left(\frac{2\pi\gamma}{\mathrm{T}}\right)^2}\left\{r\cos.\frac{2\pi\tau}{\mathrm{T}} + \frac{2\pi\gamma}{\mathrm{T}}\sin.\frac{2\pi\tau}{\mathrm{T}}\right\}$$

Now suppose, for the sake of example, that

$$\frac{2\pi\gamma}{\mathrm{T}} = 2r,$$

Dr. Hopkinson.

work done

$$= \frac{E^2}{r}\left\{\frac{1}{5} + \frac{1}{10}\cos.\frac{2\pi\tau}{T} - \frac{1}{5}\sin.\frac{2\pi\tau}{T}\right\},$$

and this will be zero if

$$\frac{2\pi\tau}{T} = \frac{\pi}{2}.$$

There is, therefore, nothing astonishing in two such machines running together; it is just what one would expect if the resistance is not too great in comparison with the self-induction. The potential difference as measured between the two machines

$$= E\sin.\frac{2\pi t}{T} - \tfrac{1}{2}E\sin.\frac{2\pi(t-\tau)}{T},$$

and its mean square

$$= \frac{E}{\sqrt{2}}(1+\tfrac{1}{4})^{\frac{1}{2}} = E\frac{\sqrt{5}}{2\sqrt{2}}.$$

It thus seems that my old theory is sufficient to predict all the results obtained by Mr. Mordey in regard to running alternators in parallel. Its imperfection is that the co-efficient of self-induction is assumed constant, which is, in fact, not exactly true. To treat it as variable renders the equations unmanageable.

Professor Adams.

Professor W. GRYLLS ADAMS [*read in his unavoidable absence and on his behalf by Professor S. P. Thompson*]: The principle of the parallel working of alternate-current dynamo machines was fully established by Dr. John Hopkinson in 1883, and it was with the view of testing or proving the truth of this principle that in July, 1884, the first actual successful trials in this direction were made by me at the South Foreland Lighthouse on the running of the De Meritens alternate-current magneto machines in parallel.

Dr. Hopkinson was not present at these first experiments, but on reference to my paper, read November 13th, 1884, it will be seen that I then and there established the fact that alternate-current machines worked in harmony without being rigidly connected, and that they mutually acted and reacted on, or governed, one another, thus giving a steadier result than when the two machines were both driven yoked together. Also it will be found that I then and there established the fact stated by Mr. Mordey in his paper, that the question of alternate-current parallel

Professor Adams.

working very largely depends upon the question of synchronising alternate-current motors.

For as soon as I had succeeded in getting the two machines to work in harmony when driven independently, I at once threw the lamp out of circuit, and threw the belt off one of the machines, when it continued to run at the same uniform speed, being driven as a motor by the electric current from the other machine. In fact, the statement that the working of alternate-current machines in parallel very largely depends upon their synchronising as motors is very similar to the statement that tuning forks synchronise when they are in unison. Of course they do.

On the same occasion, in July, 1884, I made the very interesting experiment of loading the driving machine with an arc in the external circuits, when beats were produced in the arc from the two machines getting out of harmony; just as beats are given by two tuning forks when one is loaded.

These beats, produced by two machines working together, are beautifully shown in Mr. Mordey's experiments, both when the machines are nearly in unison, and also when one is running nearly twice as fast as the other—let me rather say when one is giving nearly the octave of the other.

I have been very much interested in Mr. Mordey's experiments, which I have seen since our last meeting, and have noted one or two facts to which I would draw attention.

When one machine was running at full speed, and the speed of the other was gradually rising, the lamps attached to the two secondaries were first fitful, then gave beats at first rapid and then slower, until the two were in harmony; the speeds were now in the ratio of 2 to 1, or one was the octave of the other; then, as the speed increased, the beats again became rapid and then irregular, until the speed of the second approached nearly to the first, when the getting into unison was accompanied by beats regular and rapid, then slower and slower, until they blended, just as in the tuning of two musical instruments.

Whether Mr. Mordey has made the experiment successfully I know not, but I see no reason why he should not succeed in

running two machines together, when one is running two or three, or even four times as fast as the other, provided they harmonise exactly. Mr. Mordey has given us the octave when he drives one at 1,000 and the other at 2,000 volts, but could he not also give us the 12th, or even the double octave, and combine together 666, or even 500, with 2,000 volts, giving respectively an E.M.F. of 1,330 or of 1,250 volts? Professor Adams.

On page 592 Mr. Mordey has stated the results of eight separate and distinct experiments. Five of these experiments, viz., those numbered 1, 2, 3, 6, and 8 were made in the experiments at the South Foreland in 1884; also the second machine, when driven as a motor, was made to do work on a friction brake, and gave off more than 4 H.P. Each of these machines absorbs 4 H.P. when running at full speed with the circuit open.

In another experiment at the South Foreland three machines were driven parallel together until they synchronised, and then the belts were thrown on to loose pulleys from two of the machines, and they were both driven as motors by the current from the third machine, and the E.M.F. at the terminals of the generating machine remained the same as when the three were all being driven together in parallel circuit.

In another experiment three machines were driven parallel with an arc in the external circuit; the belt was thrown off one of the machines, which continued to run as a motor, and the arc became steadier when the third machine was being driven as a motor than when the three machines were all driven directly from the engine. Also, the work done or light given in the arc with two machines in parallel was considerably less than the work done when the two machines were supplying the arc and at the same time driving the third machine as a motor.

The illumination given by the arc was—

(1) 13,500 candles with two machines alone parallel.

(2) 16,000 candles with three machines all being driven parallel.

(3) 17,300 candles with two machines driving the third as a motor.

It may well be said of these experiments in 1884, in the

Professor Adams.

words of Mr. Mordey, "These experiments show what perfect "self-governors such motors are: not only do they maintain "synchronism, but they possess an inherent economy which "is most valuable. Just enough current passes through them "to keep them in step, and to do the work imposed on them. "They become generators, and do work on the circuit if from "any cause there is a tendency for them to run faster than "the generator." When we enquire the cause of this synchronising, to what can we attribute it but to the self-induction of the circuit? Indeed, Mr. Mordey's experiment, in which he puts two machines on the same circuit when they are not in step, but running at the same rate, shows that the self-induction is sufficient to bring them into harmony in an exceedingly short time. This sudden coming into step is no doubt due to the fact that there is not much iron in the armature; but the strong quiver and groaning of the motors accompanying the getting into step is very sufficient evidence of very considerable self-induction, in consequence of which the speed of one machine is suddenly increased and of the other is as suddenly diminished.

Major Cardew.

Major P. Cardew, R.E.: Mr. Mordey, on page 607, refers to the question of the periodicity at which the temperature visibly follows the variation of current. A good many years ago, when the Swan lamp first came out, we tried them for signalling from balloons by making the lamps flash, and we very soon, of course, found that if the current were simply taken on and off the lamp the signalling was very slow indeed. So in order to increase the rapidity of signalling by the key, which was operated from below, we merely altered the current by causing the key to short-circuit a certain amount of resistance, altering the volts on the lamp about 20 per cent.; they were 50-volt lamps, and we kept them burning at 40 volts through resistance; and when we put the key down the volts were increased up to 50, by cutting out the resistance. The difference in the flash was very visible, but not too visible to be quite clear at a distance, and we found we could get about 20 words a minute on the key. I have worked that out, and it is something like what Mr. Mordey writes thus ∿, a periodicity of about 10 per second. That is much below what most

alternators are run at. I do not think we could ever get it really visible and distinct over 20 words a minute. Major Cardew.

On page 616 he gives four possible contacts, *a*, *b*, *c*, *d*, and says if all these occurred at the same time a person would get a shock. That is as much as to say that there is the extreme improbability of the four things happening simultaneously. That is not quite the correct way to put it, because of these four things the contact between the primary mains and earth is a thing which is always unavoidable, at any rate when circuits have been up some time; unless the greatest care is taken of them there is pretty sure to be some sort of contact between the primary mains and earth, and the contact between the primary and secondary in a transformer is a thing which may happen and nobody know anything about it; it may be on for months. So that those two contacts, *a* and *b*, are waiting ready for a person to make the third and fourth himself, and of course, even then, I quite grant that it may happen that the person touches, say, the secondary conductor when standing on a dry carpet and does not make earth, and so might not receive a shock; but if he does not receive a shock he does not know anything about this fault in the transformer, and he goes on touching his secondary circuit with confidence until some occasion when he does make earth by perhaps touching a gas-pipe at the same time; that is the chance of course, and it is obvious that, considering the result of this happening, it is worth while taking any precaution which is justifiable. Mr. Mordey does not see any objection to the plan of earthing the secondary. Well, as regards safety to life from shock, I do not suppose anybody does: it was the very first thing to occur, I suppose, to everybody that the danger to life was got over by earthing the secondary. But when you come to consider the fire risks, I am afraid that we cannot say it is so satisfactory. There is no doubt that putting a connection to earth on a 100-volt circuit, which is what it comes to, because this secondary circuit has generally 100 volts, does increase the fire risk, especially where the wires are laid under floors, and perhaps in close contact with metal pipes. Nobody really recommends earthing a 100-volt circuit as a regular thing.

Major Cardew.

Mr. W. M. MORDEY: I do.

Major CARDEW, R.E.: I beg pardon. Mr. Mordey is perfectly consistent if he earths a 100-volt circuit and does not consider that that increases the danger or gives any danger of fire; he has got to persuade the fire companies to that effect, and then it will be all right. As far as the direct shock goes, everybody is satisfied if the circuit is earthed. Talking of that, there is a general impression, and I have always understood, that the Westinghouse Company did earth the secondary, but their representative, who has specially come over here, distinctly denies that they do. I should like to know whether any American company using transformers do earth the secondaries or not.

Dr. Fleming.

Dr. J. A. FLEMING: Some points which I had intended to deal with in Mr. Mordey's interesting paper have already been touched upon in the remarks of previous speakers, and nothing further need be said by me upon them, but there are one or two as yet undealt with. At the end of his paper Mr. Mordey refers to the question of the life of lamps used with alternate or direct currents. We have, of course, the broad experience of Europe and America on this point. There are some instances, however, of the systems running side by side, as at Milan. When at Milan lately, I put the question to Mr. Lieb whether he thought there was any difference in the lamp life of the theatres which were lighted by alternate and by direct currents. Two theatres are lit by Zipernowski machines at a distance of 1,800 metres from the station. Then there are other theatres which are not very far away from these, which are on the direct current circuits, and in all these theatres the lamp life is about the same. The regulation of the alternating current machines is there carried out very carefully, for the particular reason that the electric lighting company has to replace all the lamps in which the filaments are broken by the current; the customers replace lamps if they are mechanically broken.

Then, coming to a previous point in Mr. Mordey's paper with reference to the curve which he has drawn, showing the relative amount of waste energy in the transformer at different

periodicities, I should like to throw out a suggestion for his consideration. If I understand those curves aright, they mean this; that with a periodicity of 100 per second, the total heat in the transformer is less than at 125, and less also than at 75. The suggestion that I would make is this, that this is determined by the relative waste of energy produced by the eddy currents and that produced by the viscous hysteresis. We have reason to believe that the waste due to hysteresis increases with the speed, and therefore at a high speed one would expect increased heating due to this cause. On the other hand, at low frequencies the eddy currents are better able to find their way further into the iron and to waste more energy. Hence, at a certain frequency there must be a minimum of heating in the transformer core. Dr. Fleming.

Returning to the main subject of interest in Mr. Mordey's paper, the working of these alternating-current machines in parallel, I think we must be indebted to Mr. Mordey for calling our attention afresh to the fact that successful working in parallel is possible. In order to secure a practical working of the machines in parallel, it is not merely necessary that they should bring one another into phase when one machine lags behind the other, but that there should be considerable stability when in step. The force of restoration when disturbance of unison occurs must be great. What is really required is, that as soon as one machine lags slightly behind the other, the forces which are brought upon that machine to bring it into phase again must act strongly and quickly. The actions which must take place in order to secure this must be of such a nature that the armature of the machine which is lagging behind is pushed up promptly again into its position. That is, of course, putting the matter very roughly, but it really is determined by the form of the E.M.F. curve of the machine.

I do not desire to indulge in minute criticisms on a paper which is so very interesting as this one is, but I do not like to see Mr. Mordey use the word "magnetisation" at the end of his paper for what is generally spoken of as "induction." The word magnetisation has a definite meaning; it is used for the intensity of

Dr. Fleming. magnetisation, and it seems undesirable to take from the word the perfectly definite signification which it now has, to use it for what is generally known as the induction. It is perfectly true that the word induction is a very hard-worked word, but it is, notwithstanding, generally well understood.

Mr. Kapp. Mr. GISBERT KAPP: It is customary for the various speakers to compliment the author on the merits of his paper, and sometimes their doing so is rather a formality. In the present instance, however, I am quite sure you will not take it to be a mere formality when I express high appreciation of this very interesting paper. I trust you will agree with me that his paper is one of the best contributions our Society has ever had. The experimental facts brought before us are of great value, and in estimating this value we must not allow ourselves to be influenced by the author's attempt to use these facts in upsetting a well-established theory. In this he has failed, but his facts nevertheless remain all right, and are certainly of great practical importance. In the beginning of his paper he gives a quotation taken from a recent paper of mine where I say that self-induction is necessary to parallel working. He, on the contrary, says that the absence of self-induction and the absence of resistance is the essential quality in an alternator, in order that it may synchronise and work as a motor; and his reasoning is apparently sound. He shows a characteristic of one of his alternators, from which it appears that self-induction only absorbs 2½ per cent. of the total E.M.F., and this alternator is then put to work as a motor, and the experiment succeeds perfectly. From this experiment he concludes that a machine without self-induction would be the best possible motor. He was kind enough to show on Friday last to several of his friends the experiments quoted in the paper, and all succeeded perfectly. Amongst these experiments was one which could not possibly be performed if the machines were devoid of self-induction or some equivalent property. I refer to the coupling in parallel of two machines running at the same speed, but differently excited, one to give 2,000, and the other only 1,000, volts terminal pressure. When these two machines were put in parallel the drop in pressure of the 2,000-volt machine was 500

volts, and the rise in pressure of the 1,000-volt machine was 500 volts, the pressure between the coupling up leads and in the external circuit generally being 1,500 volts. Now, it is quite evident that in this case the characteristic of the generator cannot have been the very flat curve shown in the author's diagram in which the small drop of pressure (about 2½ per cent.) is supposed to be due to very small self-induction. The fall in pressure was something like 25 per cent., and this cannot possibly be due to ohmic resistance, since the latter was small, and the author does not suggest that any very large current passed through the two machines. We are therefore face to face with this very curious and apparently inexplicable experiment: here is a machine which is supposed to have very little self-induction, and, indeed, gives an almost constant terminal pressure when used with an inductionless resistance in the external circuit, but as soon as we use that machine as a generator in connection with a similar but weaker machine working as motor, the terminal pressure is by no means constant, and there is a considerable drop. Now this drop cannot be due to anything else but self-induction, or some property equivalent to self-induction, and I was at first greatly puzzled how to account for the different behaviour of the machine in the two cases. As Mr. Mordey, in his paper, has merely recorded the experiment, but has not attempted to explain it, I shall, with your permission, make this attempt. At the outset I must confess to having been in error when I assumed that the armature of this machine has only an inappreciable self-induction, and I must thank Mr. Mordey for having put the quotation referring to this matter into his paper, and thus shown me that I was wrong. Mr Kapp.

I am afraid that all of us who have worked with alternating-current machinery have gone wrong, more or less, on this question of self-induction. In the first place we have been in the habit of considering the armature as a thing by itself, without taking account of the influence of the field magnets, and, secondly, we did not distinguish between what is properly called self-induction and an equivalent property, which might better be described as magnetic change. This armature, as it stands on the floor

Mr. Kapp. removed from its field magnets, has probably very little self-induction. It must, of course, have some self-induction. To expect that a dynamo machine should be absolutely without self-induction would be as unreasonable as to expect that a steam engine, turbine, or other mechanical apparatus should have no inertia. Now, although the armature may have only little self-induction when by itself, when it is put into its field, the self-induction must be considerably increased, as can be easily seen by reference to the drawings of the machine.

Now if you look at the wall diagram, showing a section of the machine taken parallel to the shaft, and consider only the coil which is shown between the two field poles, you can regard it as a short solenoid, the two field-pole pieces forming its iron core. It is obvious that in this position the coil must have great self-induction. The coil next to it, which lies between neighbouring poles, has a minimum of self-induction, since there is no iron circuit through it. The third coil will have again a maximum of self-induction, the fourth coil a minimum, and so on. Half the total number of coils have therefore very great, and the other half very small, self-induction. Now consider the position of the coils a quarter period later. One side of each individual coil is now between the polar faces, the other side is beyond them. Each coil can now be considered as a solenoid only partially supplied with an iron core, and its self-induction must therefore be less than it was when coinciding with the pole faces. Since, however, all the armature coils (and not only one half their number) are now provided with iron cores, the self-induction of the armature taken as a whole must still be considerable. As long as the machine is working upon an inductionless resistance, this self-induction (which, although subjected to periodic variation, can never be zero) is the principal cause of the $2\frac{1}{2}$ per cent. drop in terminal pressure, as recorded in the paper. But when the machine supplies a circuit of sensible self-induction, another cause tending to lower the terminal pressure comes into play. It is what I have before called magnetic change. When there is little lag of current behind induced E.M.F., the maximum current through the armature occurs very nearly at the time when each coil is only half covered by polar faces. In this position the

current through alternate coils tends to magnetise the field poles more strongly, and that through the intermediate coils tends to demagnetise them, the two effects very nearly eliminating each other. If, however, there is a considerable lag of current behind induced E.M.F., the maximum current occurs when each alternate coil is more nearly covered by the polar faces, and the current through these coils tends to demagnetise the field, that through the intermediate coils having, by virtue of their position at the time, no effect, or only a very small effect, in strengthening the field. In the machine which acts as a motor the opposite takes place; the current in the coils which face the poles tends to strengthen their magnetisation, whilst the intermediate coils have very little effect, or no effect, in weakening them. We have thus superimposed upon the effect due to what may properly be called self-induction a certain effect, which is due to magnetic change, and which only comes into play if there is considerable lag. The field of the generating machine is weakened by the armature current, and that of the receiving or motor machine is strengthened by the current through its armature, and in this manner it is possible that the original terminal pressure of the generator may drop by 500 volts, and that of the motor may rise by an equal amount. Mr. Kapp.

Mr. J. SWINBURNE: I should like to add to Mr. Kapp's compliment to Mr. Mordey's paper, if such a thing were possible. It is eminently a paper by an electrical engineer for electrical engineers, and is therefore the sort of paper wanted here. For my part, however, I disagree entirely with Mr. Mordey's theory, that his motor kept in step because it had no self-induction. If there were no self-induction, the generator would have no control over the motor at all. Mr. Swinburne.

It seems to me that electrical engineers do not altogether realise what is wanted in parallel running. That alternating machines tend to keep in step is already perfectly well known, but to make them run parallel commercially needs a large margin corrective tendency. This can only be got by making the machines so that the maximum current is not at the same time as the maximum E.M.F. If the machines are working on resistance, this means merely a

Mr. Swinburne.

smaller output; but if one machine is working another as a motor, or simply controlling it as a lagging generator, the loss by resistance in the armatures is excessive in proportion to the power given out. This means that the machines must be made larger to give a given output. They must also be somewhat less efficient.

Mr. Mordey's attack on the existing theories of alternating current machines seems unwarranted. He makes a number of experiments, and gets results which are perfectly in accord with existing theories; he then asserts that the existing theories are wrong, without any experimental evidence, and gives no theory of his own to take their place. That his own machines have considerable self-induction is obvious from his experiment with the 2,000 and 1,000 volt machines. Apart from the question whether self-induction kept the machines in step, it is obvious that there would have been an enormous current through both armatures, if there were no self-induction. I do not mean in the least to undervalue Mr. Mordey's experiments; the degree to which alternators can control one another was not fully realised till these results were published.

In discussing motors I do not want to repeat anything already said, and will therefore cut my intended remarks down. Running a motor from a single dynamo is not a fair test, as in practice motors will be run from supply circuits.

Passing to the question of frequencies, or vibration frequencies, Mr. Mordey's comparisons of dynamos seem inaccurate; and it is very difficult to discuss these things, owing to the loose way "self-induction" is generally used by electrical engineers. Generally speaking, if you make an armature of a given size, and allow a given waste of power in it which admits of cooling by ventilation, a larger output can be got with a small frequency. I therefore prefer a very small frequency, and I think Mr. Crompton will here agree with me.

As to transformers. In comparing converters under different loads and different circumstances people are often apt to be misled by taking the same transformer in each case. The best transformer for each particular use should be taken to give results of any value. I have recently made a large number

of transformer calculations, making tables for different circumstances, working out the most efficient proportion in each case, assuming an iron induction and a copper current density which will admit of practical working. I find, for instance, that a 50-light transformer can be made to give 92 per cent. efficiency, at 33 vibrations per second at full load. At 83 vibrations per second a 50-light transformer can give 93 per cent. The saving in cost of material is fifteen shillings, or about fourpence a light, which is inappreciable. A house transformer may be taken as running at full load for an hour a day. Such a 50-light transformer can be made to give an average percentage of 60 at the low frequency, and 62 at the high. Both these last transformers are prohibitive in price, so a lower efficiency would have to be tolerated. In the case of a two-hour-a-day transformer the efficiencies are higher, being 70·5, and a little more with the higher frequency. Mr. Swinburne.

As to lamination, I think Mr. Mordey over estimates the effect of eddy currents. His experiments do not really distinguish between eddy currents in the discs and variations of mechanical, or of air friction, and eddy currents in the wire and in the hub of the armature or elsewhere. I recently calculated the loss by eddy currents in a laminated direct-current core, but the results are in a paper not yet published; I may mention, however, that they are exceedingly small, in fact generally inappreciable. Mr. Mordey's method seems exceedingly ingenious, but the results cannot be got with any accuracy, as so many unwarrantable assumptions have to be made.

There are several methods of starting alternate current motors. I may perhaps be allowed to mention that starting by secondary batteries was mentioned in a patent specification of mine in 1887. The exciter may have its field magnets laminated, and the alternating-current can be turned on to it to start the motor. This is in a specification of 1886, and seems to me a still simpler plan.

Professor S. P. THOMPSON: There is one most pregnant remark pointing to the very remarkable experiments which Mr. Mordey has shown to some of us: on p. 591, he says, "The foundation and parallel working should be that the prime motors Professor Thompson.

Professor Thompson.

"are under the control of the generators." Those of us who saw the way in which that generating dynamo persevered in its work, when the second alternate current machine was suddenly thrown upon it as a motor, could not help enquiring in what way the steam-engine was governed which kept up the speed. Mr. Mordey rather startled me by telling me that his steam-engine had no governor. That may account to some extent, I think, for some of the peculiarities of the experiments. I would like to know a good deal more on that point.

Mr. Mordey takes us to arc lamps, and points out that with alternate currents 50 volts are required, whereas with continuous currents 60 volts or so are required. Here we have a case of which very little is known. Why is it that an ordinary arc lamp with a continuous current will not work well without at least the ordinary 60 volts? We know that there is in effect a back E.M.F. in the arc, or at least there is something which can be measured as such. I have lately been busying myself employing a group of students to investigate the question at what position in the arc does this back E.M.F. come in. I have satisfied myself that, in direct current arcs, the great drop of potential in the arc, usually some 39 volts in amount, occurs at the positive end or crater, and that it does not occur in the arc itself, nor at the negative pole. There is a definite sort of polarisation at the crater end. I have not yet examined alternate current arcs, and I cannot give the meeting any information upon that point, but probably we should find that there is some other disposition when we make the corresponding measurement for alternate arcs.

Mr. Mordey speaks of the drop in the characteristic curve of his machine as being partly due to resistance and partly to self-induction. I do not want to quarrel over terms (and after all it is a question of terms), but I would rather express it that there is a drop in the curve due to resistance, a further drop possibly due to the spurious addition to the resistance arising from self-induction, and a further drop due to the demagnetising effect of the armature currents on the pole-pieces. But I am not all sure whether that does not involve the question of self-induction, for the last two things are really parts of the same

phenomenon, the self-induction of the armature and its magnetising effect being bound up together. Professor Thompson.

Mr. Kapp has said that the armature standing away from its field magnet could have very little self-induction, but that all that would be changed directly you put it in the massive iron circuit. I differ from him on that point, and will tell the Society why the presence or absence of the iron field-magnet cannot make very much difference.

In order that the self-induction should be very much increased by the presence of the iron there, the circumstances must be such that the iron can act. The presence of iron can increase the self-induction in a coil only when and as the iron is magnetised by, and adds to, the magnetic action of the coil. But in the case of great masses of iron like these polar projections, with a cross-secton in each of, I suppose, from 15 to 20 square inches, not laminated, how much magnetism will you get into that from a current in a coil that flies past at the rate of 1-200th part of a second? It has no time to magnetise the iron, and therefore cannot perceptibly produce self-induction. Consequently the presence of the solid iron field-magnets surely cannot make much difference in the self-induction of the armature.

I have devised a way of elucidating a point in the action of an alternating-current machine used as generator and motor by a graphic construction, which is as follows:—

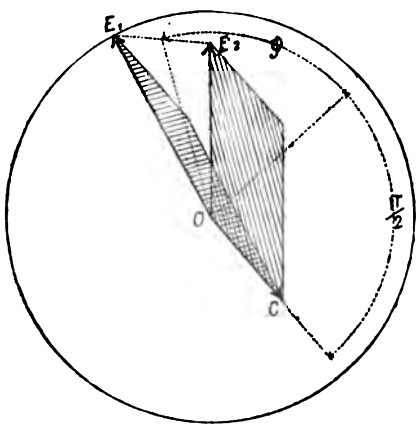


FIG. 15.

Professor Thompson.

Let O $E_1$ and O $E_2$ represent the two alternating electromotive forces in the two armatures, agreeing nearly in phase when the two machines are both acting in parallel, with the current from each going in the same direction as the E.M.F. The current will (because of self-induction in the circuit) lag behind the resultant E.M.F. by an amount represented by the angle $\phi$. When the steam is suddenly cut off from one of the machines the current through it is instantly reversed in sign; hence, though its E.M.F. remains in the same direction, electric work is done on instead of by the machine. Now, in this diagram, a mere variation of the clock-diagrams used for harmonic motions, the work done in a cycle may be represented as an area, by the simple device of putting the line that represents one of the two factors—say the current—back by just one quarter in phase, and then completing the parallelogram between the two factors. Let O C represent the resultant current, retarded behind the resultant E.M.F., and further retarded by 90°. The area contained by O $E_2$ and O C (relating to the machine lagging in phase, the generator) will represent the electric work done by the one machine, whilst the area contained by O $E_1$ and O C (the latter being now considered negative) will represent the electric work done on the other machine (the one advanced in phase, the motor).

When you throw off the belt and one machine becomes a motor, before it has had time to change its speed to any appreciable extent, the E.M.F. will have altered a little in phase, and an excessively slight difference in the relative position of armature coils and field-magnet poles—corresponding, perhaps, to one-hundredth of an inch at the periphery—will make all the difference between its action as generator or as motor.

The stability of the machines in running, and its dependence upon self-induction, surely ought to be cleared up. I do not at all agree with Mr. Kapp that you want self-induction in the armature in order to produce stability; I should be disposed to agree with him that self-induction in the circuit outside the machines would tend to make them synchronise, because the lag of current which produces synchronism is itself due to self-

induction; but self-induction is not needed in the armature for this purpose. Professor Thompson.

Dr. Fleming tells us we ought not to use the word "mag-"netisation" where we mean to express the intensity of the magnetic induction. But did not Dr. Hopkinson use the very word in the title of his paper before the Royal Society on the magnetisation of iron—a paper devoted to the intensity of the induction in different specimens? Mr. Mordey's paper seems to me to be distinguished for its perspicuous and conspicuous common sense. Ten years ago we did not know how to design direct-current machines to run as direct-current motors; the question was in a perfectly chaotic condition. With better designs of generators, however, came a better understanding of motors. The subject of alternate-current machines was almost in as chaotic a condition until we had Mr. Mordey's paper to enlighten us. He has certainly proved to us that good design in the machine is the first essential of successful simultaneous working of alternate-current machines.

Professor W. E. AYRTON: Although I have notes of many remarks to make, I will confine myself, owing to the lateness of the hour, to those points of the greatest importance. Professor Ayrton.

We must, of course, commence by sincerely congratulating the author, for, indeed, if he had done nothing more than show that Dr. Hopkinson's mathematical conclusions were experimentally correct, there would be an enormous debt due to him by all electrical engineers.

Anticipating that there would be some question to-night as to what was, or was not, the self-induction of the Mordey armature, and whether it was, or was not, greater when the iron was in a particular position, one of my assistants—Mr. Sumpner—and some of my students—Mr. Lamb, Mr. Smith, and Mr. Woods—went to the Belvedere Road, and, by the kindness of Mr. Mordey, made experiments on the self-induction of the armature, with different positions of the iron pole-pieces, and with different currents passing round the field magnet. The method employed consisted in balancing the unknown self-induction of the armature against the self-induction of our adjustable secohm-standard, and using the

Professor Ayrton.

secohmmeter to rapidly alternate the battery and galvanometer connections—an arrangement which supplies a very exact and sensitive method of measuring self-induction.

Figures A and B show the relative positions of the armature coils and the pole-pieces of the field magnet.

A

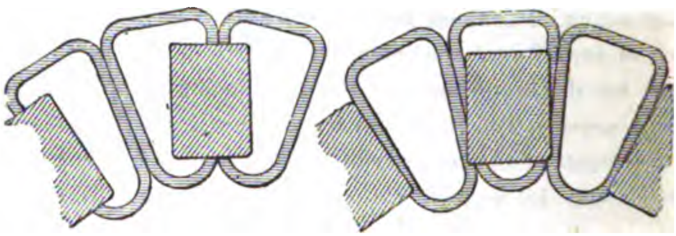

FIG. 16.

*Self-Induction of the Armature of the Mordey Alternator.*

| Exciting Current round the Field Magnet, in Amperes | Position of the Armature. | *p*. | *q*. | Secohms, by Secohm-Standard. | Self-Induction of Armature, in Secohms. |
|---|---|---|---|---|---|
| 0 | A | 1,000 | 1,500 | 0·0255 | 0·038 |
| 0 | B | 1,000 | 1,500 | 0·0218 | 0·033 |
| 0 | B | 1,000 | 1,500 | 0·0230 | |
| 40 | A | 1,000 | 1,000 | 0·0365 | 0·036 |
| 40 | A | 1,000 | 1,000 | 0·0360 | |
| 40 | B | 1,000 | 1,000 | 0·0295 | 0·030 |

The first column in the preceding table contains the exciting current, in amperes, passing round the field magnets; the second column the relative position of the armature and field magnet for each test; $p$ and $q$ are the resistances of the two proportional coils of the Wheatstone's bridge; the fifth column gives the secohms as indicated by the secohm-standard when it was adjusted to produce balance; and the last column contains the self-induction of the armature itself. In all the experiments the secohmmeter was rotated to give a frequency of about 100, which is the frequency actually employed by Mr. Mordey in the working of his dynamos. The armature has the minimum self-induction, 0·030 secohm, when it

is in the position B relatively to the field magnet, and when the exciting current of 40 amperes is passing round the field magnet; while the greatest value, 0·038 secohm, is attained when the armature is in the position A and no current is passing round the field magnets. Professor Ayrton.

It might have been imagined that the armature would have had a greater self-induction in position B than in position A, since in position B the iron of the pole-pieces of the field magnets covers the coils better than in the A position. The increase of self-induction of every other coil in position B is, however, more than compensated for by the diminution of self-induction in the alternate coils; so that it is in position A, when every coil is partially covered by iron, that the greatest self-induction is obtained.

Professor Silvanus Thompson considers that the presence of a large mass of iron cannot influence the value of the self-induction of a coil for a frequency 100; but this, I think, is a mistake, and I would refer him to the experiments we described when we first had the honour of bringing the secohmmeter to your notice, and in which we showed that the introduction of a *solid* iron core into a solenoid increased the self-induction of the coil 26 times when the frequency was fairly high. No doubt the greater the frequency the less effect will iron have, and for very great frequencies the presence of solid iron will probably even diminish the effective self-induction of a coil; but for a frequency of 100 I think that it would be quite wrong to conclude that the presence of a large mass of solid iron did not increase the effective self-induction of the coil.

The very remarkable experiments carried out by Mr. Mordey of coupling two alternators together which are producing a very different E.M.F., and obtaining as the resultant E.M.F. the arithmetic mean of the two E.M.F.'s, are, as Mr. Kapp has pointed out, probably due to the weakening of the field of the one machine and the strengthening of the field of the other by the action of the currents in the armature. Indeed, when Dr. Hopkinson brought his admirable paper on the "Theory of "Alternating Currents" before this Society in 1884, I pointed out

Professor Ayrton.

(see pp. 530 and 531, vol. xiii., of our Journal) that there was only one point of incompleteness, as it appeared to me, in his investigations, and that was the neglect of the action of the current in the armature on the field.

Through the kindness of Mr. Mordey in lending us one of his transformers, a series of experiments has for some time past been conducted by the students at the Central Institution on the efficiency of this transformer for various frequencies and with various loads. As far as we have gone at present—making experiments at frequencies 53, 80, 160, 240, and with loads varying from a light load up to the maximum working load intended with this transformer—we have obtained no indication whatever of the remarkable result described by Mr. Mordey, that his transformer is more efficient for an intermediate frequency than for a higher or lower frequency. But, on the contrary, we have found the efficiency to steadily increase with the frequency, this increase being particularly marked for light loads. Our experiments were not made by the calorimetric method, which is a very tedious one, but by the double method of using a quadrant electrometer, which Professor Fitzgerald and I hit on during the meeting of the Electrical Congress at Paris in 1881, for the accurate measurement of the watts given by an alternate current (which may be any function of the time) to a circuit which may contain any amount of self and mutual induction. The non-inductive resistance which this method of measurement requires to be used in series with the transformer is, in the experiments now being carried out at the Central Institution, composed of long strips of platinoid about 2 inches wide placed back to back with thin shellacced silk, the current going up one strip and down the other one. In this way a resistance is obtained with a large cooling surface and with practically no self-induction.

How the efficiency may vary with the frequency for a load greater than the maximum load that we have hitherto put on the transformer I do not know, and I shall not proceed to ascertain until I get Mr. Mordey's permission to send more current through his transformer than it is intended to stand, but for all loads less than this our experiments have not hitherto confirmed his result

of obtaining maximum efficiency for some intermediate frequency. That reminds me, in passing, that the method Mr. Mordey suggests for getting the absolute efficiency when using the calorimetric method I do not think is applicable; indeed, the method he suggests was thought of by ourselves, and rejected, when we were making the calorimetric experiments which we described here a year ago. The method we used in that investigation was to have a stream of water flowing through the calorimeter until it arrived at a certain constant temperature; then, by knowing the temperatures of the ingoing and of the outgoing water, and the amount of water flowing per minute through the calorimeter, we obtained the total quantity of heat lost. Mr. Mordey has suggested in his paper a different method. He has proposed sending a *steady* current through the transformer and raising the transformer by the steady current to the same temperature to which it is raised when you are working with an alternate current; then, knowing the watts wasted by the steady current—which can, of course, be easily measured—he assumes you would know the watts wasted by the alternate current, which are much more difficult to measure. But with the steady current the waste of energy is entirely in the wire, whereas with an alternate current there is, of course, waste due to the Foucault currents in the iron. Now, if an amount of heat that is generated by the alternate current in the iron is to be generated by the steady current in the coil, it will require that the steady current shall be far greater than the coil is intended to carry. And it was this fear of burning up the coil of the transformer we were then experimenting with, and which was also a borrowed transformer, that prevented us trying this method suggested by Mr. Mordey in his paper. Professor Ayrton.

Professor G. FORBES: I would, in the first place, ask Mr. Mordey to make quite definite what does not appear in the paper, but what has been let out in the discussion, that in these experiments with parallel working there was no governor on the steam-engine. It has a most important bearing, I think, on the whole question whether there was a governor or not, for we must have a governor in actual work. As a matter of fact a steam- Professor Forbes.

Professor Forbes.

engine is supposed to be governed by a perfect governor, in order to keep the speed perfectly constant, and then the only interruption that can take place between it and the dynamos is by the slipping of the belt, if a belt is used at all, and if not then it is only by the imperfection of the governor that the variation can take place. I should like some more information on this point, as I think it will explain many apparent anomalies.

I have a good deal more to say about parallel working, but at this late hour I will not continue.

At the beginning of the paper Mr. Mordey is perpetuating an error which a good many have been propagating of late, as to assuming that there are only two means of distributing alternating currents, one is by a single large machine, and the other by a number of small machines, which must be working in parallel. Now I have noticed it so continuously stated as the only alternative method that I cannot understand it. I thought there had been almost too much said about the American practice, at least many people thought so, but the American practice has been invariably not to work the small machines parallel, and to have a large number, the difference being that the mains are not all connected in parallel; there are a large number of each, and each district is supplied by a number of separate mains and feeders. One dynamo will have a number of feeders even when the maximum current is on, and when a small load is on all these feeders may be put on to one machine.

As to the machine which Mr. Mordey has shown on the paper, he has not actually made it, but he has drawn it, and, as he tells me he had drawn it a good long time before I published mine, therefore the credit all belongs to Mr Mordey, for he has priority.

I have made one of those machines, and although at the present moment the results have not been completely satisfactory, still there are great hopes of getting a good machine out of that. The self-induction of the machine in the form in which it was made was enormous, and the variations in volts when the different strengths of current were taken off were something perfectly surprising, in fact you might almost say that the current

was constant; you could get almost any volts out of it, but always nearly the same current going. It was a very interesting dynamo machine. Professor Forbes.

The PRESIDENT: Before calling on Mr. Mordey to reply I may perhaps be allowed to make a few remarks on his beautiful experiment in the case in which two alternators, one with a potential of 2,000 volts, and the other 1,000 volts, are coupled in parallel. The secret of the surprising result of this novel and original combination has not, I think, been quite touched by either Mr. Mordey himself or any of those who have spoken in the discussion except Mr. Kapp. The 500 volts difference of potential would, notwithstanding ohmic resistance and impedance by self-induction, produce current in the circuit of the two armatures vastly greater than any that Mr. Mordey has found in his experiments, without some equalising influence which has not been hitherto suggested except by Mr. Kapp. To find what this influence is, suppose, for simplicity, the ohmic resistances of the armatures to be zero, and suppose the two armatures to be simply joined in parallel ready to do external work but not as yet set to do it. Thus we have a simple circuit of the two armatures. The two shafts might be mechanically constrained to run synchronously in such relative positions that the electromotive forces of the two armatures conspire in the circuit. The 3,500 volts would produce a prodigious current for all that self-induction could do to impede it; but this current would enormously pull down the magnetisation of the iron claws of the field magnets; and might even annul and reverse that of the weaker, and thus after the first making of the circuit of the two armatures the "prodigious" current through it would almost instantly become much moderated, even supposing the supposed initial phase-relation to be mechanically maintained. This is the phase-relation for series co-operation; and, as shown by Dr. Hopkinson in his lecture on electric lighting, delivered before the Institution of Civil Engineers in 1885, it could only be maintained by rigid mechanical connection between the two shafts. But, in Mr. Mordey's actual experiment, the two shafts are free and independent, and they therefore "fall Sir William Thomson.

Sir William Thomson. "into step" cophasally for parallel working. Their electromotive forces become thus exactly opposed in the circuit of the two armatures, and would yield exactly 500 volts as working E.M.F., if the two field magnets remained unchanged. The ohmic resistance being still, for simplicity, supposed zero, the phase of the current kept going in virtue of the E.M.F. would be a quarter-period behind the phase of the E.M.F. Thus the maximum of the current would be at the instant when the nine magnetic fields are in the middle of the apertures of nine alternate armature coils, and the direction of the current would be that tending to demagnetise the iron prongs of the 2,000-volt machine, and to augment the magnetism of those of the 1,000-volt machine. Thus (the period in Mr. Mordey's experiment being a 100th of a second) two hundred times per second, the former experiences a demagnetising, and the latter an enhancing, magnetic force. The prongs are of continuous iron, and there must therefore be ample self-induction to prevent any considerable change of magnetisation in the 200th of a second period of this varying magnetic force. Thus the magnetic fields are kept, one of them weaker, and the other stronger, than they were before the armature circuits were connected; and I believe so much weaker and stronger as to reduce the electromotive forces of the two to very near equality. If the iron of the 2,000-volt machine is nearly saturated, its magnetism would be less diminished than the other's increased, and the resulting E.M.F. might be nearer 2,000 than 1,000; but Mr. Mordey has found it to be actually very near to 1,500. A few turns of insulated wire round one prong of either machine, connected to a ballistic galvanometer, would give a ready means of testing the suggested changes of magnetisation.

There is really not much more left for me to say, except that I do wish to be allowed to join in the general and most sincere chorus of admiration at the results put before us by Mr. Mordey. We are all admirers of his previously-known alternate-current generator. We have now before us for the first time another form, invented also by Mr. Mordey—a form which strikes me as exceedingly beautiful, very admirable in many respects, and, I am pleased to add, quite wonderful.

Mr. J. S. RAWORTH: When I heard Mr. Mordey's paper read last week, I thought this vexed question of alternate current was about to take a new departure, that we were going to get out of the region of theory and into the region of fact. Mr. Mordey described certain experiments which proved directly that previous statements made before this Institution were not correct. But to-night I am sorry to say we have returned into the region of theory again. Happily for us, however, the Professors are not all on one side this time, but take opposite views; we may, therefore, hope to get at the truth in course of time. When Professor S. P. Thompson was at our works the other day, I had a little talk to him about the amount of lag between the dynamo and the motor, and he suggested that it would be 100th of an inch; I thought it would be about 1 inch. We have now succeeded in measuring this lag, and I daresay Professor Thompson will be interested to hear that it is exactly 13-16ths of an inch. Mr. Raworth.

Professor S. P. THOMPSON: Was it a full load, or running open?

Mr. J. S. RAWORTH: A full load.

Professor S. P. THOMPSON: That is not my point—which was that you had two machines, one driving the other open.

Mr. J. S. RAWORTH: I do not disagree with you there.

Professor S. P. THOMPSON: The load that would convert a generator into a motor need not be more than 100th inch.

Mr. J. S. RAWORTH: The exact difference between the two when running with full load is 13-16ths inch, or 24° of a complete cycle.

Now I want to show you that we have been trying to arrive at some more facts, and to find out from actual experiments the curve of E.M.F. at different points of the period.

The sketch (Fig. 17) shows a Mordey-Victoria alternator with a wood drum fixed on the driving pulley. This wood drum carries a small piece of copper wire (part of a helix) sunk in its surface. Two bobbins of the alternator, marked A A, are coupled to one end of the copper wire carried by the drum, and the other end to one pole of the continuous-current dynamo, the other pole of the

Mr. Raworth.

continuous-current dynamo is led to a movable handle carrying a copper pointer, marked P. A voltmeter, V, is put across the terminals of the continuous-current dynamo. The field of the said dynamo is controlled by a rheostat, marked R. The secondary

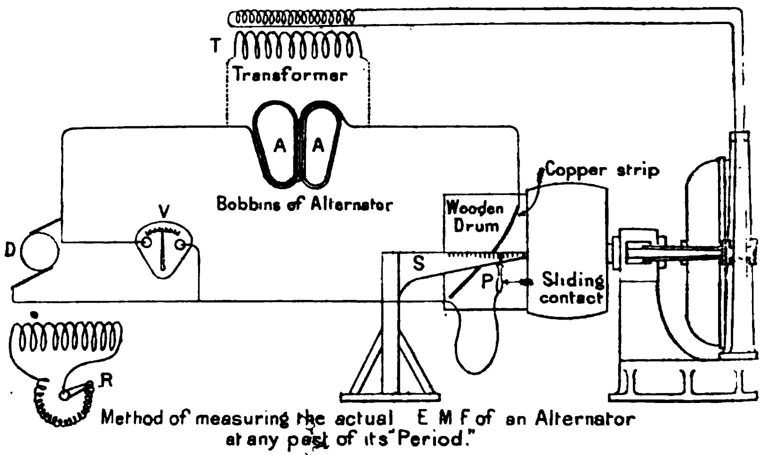


Fig. 17.

coil of a transformer, marked T, may be substituted for the bobbins, A A, the primary coil being excited by all the bobbins of the alternator in series (as usual). In taking the curve of E.M.F. during one period the dynamo, D, is regulated to give various differences of potential, ranging from nothing up to the maximum required, and while the alternator is running the pointer, P, is tried along the scale, S, to find positions of no spark. These positions on the scale correspond with known positions on the helical wire. From the readings obtained it is easy to plot a curve showing the electromotive forces developed at every point in the period.

By traversing the pointer along the scale we can measure exactly the points in the cycle, where we get an E.M.F. equal to the opposing E.M.F. of the continuous-current dynamo. That point has been ascertained by the fact of there being no spark at the moment of contact. We have not had time yet to carry out a complete series of experiments, and I am therefore unable to produce the diagram to-night. We have, however, succeeded

in plotting down the transformer curve on the top of the alternative curve. Mr. Raworth.

The curves are very astonishing, and everybody who has seen them is surprised at the extraordinary results that have been obtained. The corollary is that it is not safe to prophecy until you are sure. We have seen a good many curves drawn on this wall that have been very beautiful, but not much like the actual curve.

Mr. C. ZIPERNOWSKI [*communicated*]: In his paper upon "Alternate Current Working," read before the Institution of Electrical Engineers on May 23rd, Mr. Mordey has made several statements with regard to our dynamos which we have found to be erroneous. Mr. Mordey says, "The "Zipernowski alternator will, however, work parallel, but "apparently not very well; to get it to do so, the periodicity "has had to be reduced to 42, as we now know." At another place we find, "In a communication from Messrs. Ganz & Co., "published a few days ago, Mr. Zipernowski says that the low "periodicity used was chosen on principle, to enable them to "couple their dynamos parallel;" and some lines further down:— "This bears out very completely my contention that the "periodicity is governed in most cases by some special feature "of the type of apparatus. Zipernowski has had to go down "to 42 to get his iron-cored alternator to synchronise, and for "no other reason; for his statement is that synchronous action "was 'not a secondary result, but just the end we were aiming "at,' and he acknowledged that there are disadvantages con-"nected with this low rate in other parts of the system." Mr. Zipernowski.

First of all, we must state that our alternate current dynamos are running in parallel circuit in a thoroughly satisfactory manner, even when the load varies from zero to the maximum output for which they are built. This may be seen at our central stations at Rome, Terin, Livorno, Frankfort-on-Main, and Marienbad.

We have, also, not found any difficulty in running large dynamos parallel with small ones unto the maximum output of the latter. Thus, in May of last year, Mr. Blathy has

Mr. Zipernowski. coupled two 600 h.p. dynamos in parallel with two of 150 h.p. each, at Rome, the former of these dynamos making 125 the latter 250 revolutions per minute. Each dynamo is directly driven from its own engine, the smaller engine being of Sulzer's, the larger of Van den Kherhove's construction. The dynamos work parallel with greatly varying load to perfect satisfaction, although neither they nor the engines are of equal size, the latter being even of unequal construction.

We pretend to have been the very first who have made parallel coupling "an industrially proved fact" (*vide* Mr. Blathy's letter in *The Electrician* of June, 1888), and this, perhaps, may have led Mr. Mordey to conclude rather generally, and not with reference to concrete facts, that this first practical application of parallel coupling worked as most first applications do, "but apparently not very well."

Moreover, we state that we have not reduced the periodicity because of the coupling in parallel. Since we have commenced building alternate current dynamos we have kept the number of 85 alternations with only slight variations. And we still remain convinced that we can run our dynamos parallel with this number of alternations *easier and in a more simple manner* than with a higher one, but contest that this is the only reason for which we do not increase our number of alternations, which we have found suitable for so long a time.

If Mr. Mordey had followed our publications with attention, or had asked us for information, which we would have willingly given him, he would have known that, amongst others, we profit from the low number of alternations in our alternate current motor, this low number enabling us to build small motors without too high speeds; a motor with four poles, having our periodicity, would make 1,250 revolutions per minute, but 2,950 with Mr. Mordey's; or must have 10 poles in order to make about 1,250. Of course this number of poles is rather high, and too dear for small motors.

In the appendix to the reprint of his paper, Mr. Mordey says, "He thought a great mistake was made in some alter-"nators in using Pacinotti projections. In all cases there

"should be as nearly as possible a steady magnetic flux in "the field. This could not be done if projections were used. "The Zipernowski and Parsons machines were faulty in this "respect. It was impossible to tell by inspection of the former "which was the armature and which the field. They seemed "quite interchangeable, and this was, of course, not right." Mr. Zipernowski.

As Mr. Mordey does not say one single word to ascertain his opinion that the magnetic flux ought to remain constant, we cannot contradict any arguments and need not contradict the mere statement. But we must say that we are very much astonished to hear that Mr. Mordey, who, with full right, so highly appreciates the "sense of proportion," is not able to distinguish the field magnet from the induction coils in our dynamo.

We are glad to see that the figures we published about our alternate current motor have animated others to a similar "success." Our motor, which consists of *one* machine only, possesses all the qualities published some time ago, which are at present also claimed, and very ably pointed out, by Mr. Mordey. Whether Mr. Mordey's motor, which consists of an electro-motor, an exciting dynamo, and an accumulator battery, will hold good under all circumstances and for large forces as well as for small ones, practice will show.

Mr. G. C. Fricker [*communicated*]: Not having taken part in the verbal discussion on Mr. Mordey's paper I should like to contribute a few remarks in this Journal on some points which appear to be of the greatest practical importance, as bearing upon the inter-controlling power of alternating machines when working parallel. Mr. Fricker.

I may say at once that I entirely agree with the principles of construction which are advocated by Mr. Mordey, and that to me, in common, I imagine, with the majority of practical workers in this field, his doctrine has come as a great revelation.

With regard to this question of the motor action of alternators in parallel, it is perfectly obvious that synchronism is only attained by the pull which the impressed field exerts upon the field induced by the impressed current from the leading machine;

Mr. Fricker. that is to say, by the self-induced lines of force being drawn into, and made to form a part of the impressed lines. It is evident that the greater the angle between the armature coil and the polar face of the field magnets, when the former is carrying its maximum current, the more difficult it will be for the self-induced lines to become interpolated into the lines of the impressed field, and that, in consequence, any condition which would retard the rush of current in the armature of a lagging machine would be detrimental to its speedy recovery of phase.

Besides the condition of rapid rise of current in an alternate-current motor, it is essential that the couple exerted by the attraction of impressed and self-induced fields should be as large as possible, and therefore it is necessary, when taking steps to reduce the one at the same time to increase the other force. The proper conditions for what Sir William Thomson terms co-phased synchronism are thus an exceedingly strong impressed field and an exceedingly weak self-induced field, the latter condition being necessary to ensure instantaneous rise of impressed current, and the former to effect a powerful controlling couple. I therefore agree with Mr. Mordey, in his argument, that the most perfect alternator is one which has an indefinitely small co-efficient of self-induction and an infinitely strong impressed field. On the other hand, it is equally true that some self-induction must be present, otherwise no controlling couple could exist. In practice, of course, the impressed field is limited by the receptivity of iron, and therefore, in order to obtain an adequate controlling couple, it may be necessary to tolerate a considerable co-efficient of self-induction. Mr. Mordey's experiments, however, prove beyond a doubt, that in a machine in which the effective impressed field is very large, that the self-induction may be made very small indeed, and controlling properties of hitherto unprecedented power attained.

It seems probable that in formulating the conditions of maximum controlling power with any given strength of impressed field, Dr. Hopkinson, Mr. Kapp, and others, have lost sight of the importance of the field itself in regard to the problem, and that so far from such theories being "well established," as

Mr. Kapp puts it in his remarks, they are in reality established on a very incomplete basis. Mr. Fricker.

The PRESIDENT: This is the last evening before the vacation, and, as we have another interesting communication to be brought before us, I think it would be agreeable to the meeting not to close at 10 o'clock, but to continue for at least half-an-hour longer.

I will now ask Mr. Mordey to reply to the discussion.

Mr. W. M. MORDEY, in reply, said: I wish in the first place to express my thanks for the very kind way in which you have received this paper. The criticisms have, in some cases, been answered by other speakers. The remaining points in regard to which the speakers are not in accord with me I must deal with so far as I can in the time at my disposal, but I need not occupy time by referring to other matters. Mr. Mordey.

Dr. Hopkinson's statements on electrical subjects are always received, not only with great respect, but as being usually correct and final. I join in the respect, but venture to question the correctness in this instance. If Dr. Hopkinson would be better satisfied by my stating that the most perfect alternator conceivable is one which has an indefinitely small self-induction and resistance, I am quite prepared to express my meaning in that way. I was acquainted with the general practical results, as regards parallel working, brought before us in 1884 by him, but, for reasons which will be understood, I did not derive any assistance from his theory, nor do I now agree that that theory "is sufficient "to predict all the results obtained." I cannot fully argue this point, but must ask Dr. Hopkinson if he will kindly look into it again, for I am strongly of opinion that he will find that it is not as complete as appears to be supposed. I am the more convinced of the correctness of my explanation by the measurements of the self-induction of my armature by Professor Ayrton's assistants, using the new Ayrton and Perry instruments, and by the statement made by Professor Ayrton that the time-constant, or ratio of the self-induction to the resistance, was about the same in my machine and in that of an alternator of another maker, which latter machine only exhibited very feeble synchronising properties when attempts were made to run two of them in parallel.

Mr. Mordey. The point where mathematical treatment of the subject appears to have failed is that it has not taken sufficient account of the "stiffness" of the field. This is at once apparent when this measurement of the self-induction of one of my machines is referred to, from which it will be seen that the self-induction was practically the same whether any impressed field existed or not. Manifestly, with no impressed field, the controlling power of the machine would be quite inconsiderable, although the values of self-induction and resistance—upon which Dr. Hopkinson relies—would be the same then as when the machine was working with a strong impressed field and under the conditions giving the maximum generating and controlling power. Hence it is evident that a theory which only takes cognisance of self-induction and resistance must be altogether illusory.

There are several reasons for the view that his theory is incomplete. His paper has been before the world for several years, and has been often studied, but neither its author (who has been engaged on the subject) nor anyone else has ever based any practical advance on it. Working parallel has been recently considered, as a question of great urgency and importance, before this Institution and elsewhere, and there have been abundant opportunities for bringing forward any useful information, but the whole subject was misunderstood; and Dr. Hopkinson, who has been a witness of the struggles that on all sides were being made towards a better understanding of it, has remained silent. I am sure that nothing would be more unlike him than to let the whole industry suffer for want of a little clear and definite information, if he had been able to give that information. Dr. Hopkinson even took part in the discussion on Mr. Kapp's paper a few weeks ago, and threw no light on this question, thus tacitly subscribing to the current views.

It would be easy to find several other instances, besides those mentioned in the brief enumeration in my paper, to show that the views generally held were entirely misleading and incorrect. For example, Mr. Swinburne alludes to* Dr. Hopkinson's mathematical

* *The Electrician*, June 14th, 1889, p. 154.

treatment, and says that he has put the matter into English in his book;* and I find in that (recently written) book it is stated (on p. 151) that there are no alternate-current motors. But nothing less than the actual words can show how completely misleading this theory was. Mr. Swinburne proceeded, in the book in question: "It seems generally to be assumed that the "only difficulty with alternating-current motors is to start them. "It seems probable, however, that there will be very great "difficulty in designing a motor which will work with reasonable "efficiency and varying load. An alternating- is a widely different "thing from a direct-current motor." Mr. Mordey.

Having seen all these statements disproved—having seen a 50-H.P. synchronising motor running with high efficiency under varying load, and designed on the lines of a direct-current motor—Mr. Swinburne now comes forward and says: † "I see nothing in "Mr. Mordey's interesting experiments which conflicts with the "recognised theory; in fact, *the position of synchronous motors* "*is not altered*"—the italics are mine. Truly the Hopkinson theory, as rendered into English, must be capable of infinite elasticity.

Then again, only a few months ago, Mr. Swinburne, who had assimilated this theory, stated‡ that such machines as mine would not run in parallel at all, and he described an "induction-"coupler" which he had devised for the purpose of making them so run by inserting self-induction. That device is a very good illustration of the results of the old theories. I may mention that when I was labouring in the slough of those old theories I independently devised that identical arrangement, which is one quite in accord with the requirements of those theories, so far as I was able to understand them; and having entirely failed to get two ironless alternators of a well-known type to work parallel in their simple condition, I tried that "induction-coupler," without any improvement in the result. That was, in fact, one of the experiments that opened my eyes to the whole fallacy. It helped me

* "Practical Electrical Measurement."

† *The Electrician*, June 14th, 1889.

‡ *Journal*, vol. xvii., p. 401.

Mr. Mordey. to see that anything that prevented the instantaneous passage of the necessary correcting current must inevitably tend to prevent synchronous action.

Referring to Professor Adams's criticism, I may say that I was fully aware of his experiments at the South Foreland Lighthouse, and referred to them. Professor Adams considers those experiments as conclusively establishing Dr. Hopkinson's theory. I need not again consider them in that connection, but would only say that the motor effects obtained were not powerful, and that they have not been taken as forming a basis for practical construction. Indeed, they have remained on record as the not very high tide-mark beyond which no substantial advance was found practicable, until machines having very different qualities were developed. It would cause me no surprise to find that the South Foreland experiments succeeded from quite different causes than those imagined by Professor Adams. It is not at all clear that any of the advocates of Dr. Hopkinson's theory really understand what that theory is. It seems to cover a multitude of quite opposite interpretations, and resembles some of the Hebrew prophecies in its comprehensive applicability to occurrences—after the event. Professor Adams, I am afraid, rather supposes that I make some sort of claim to having introduced parallel working. The object of the first portion of my paper was rather to state what lines I thought should be followed in order to obtain the best results, and to show that on those lines complete success could be attained at a time when, in spite of Professor Adams's well-known and valuable paper, considerable and well-founded doubt existed as to the commercial possibility of parallel working. In one respect, however, I am quite unable to follow Professor Adams, and that is in some of his analogies between alternators working parallel and tuning-forks vibrating in unison. He says that I gave the octave when I had one machine working at 1,000 volts and the other at 2,000 volts, both being at the same speed, and he suggests that I should give the double octave, and various other ratios. Now I fail to see that there is any true analogy between various rates of sound vibration produced by tuning-forks and corresponding ratios of electrical pressure. It would be just as correct to say

that a steam boiler with a pressure of 120 lbs. on the square inch was giving the octave of another boiler working at 60 lbs. Mr. Mordey.

As to the very different problem of running alternators parallel when the alternations are an octave of one another—a possibility that is also put forward by Professor Adams, and which has been mentioned to me by Mr. Sumpner and others—I can only say that I have not tried it, partly because it does not hold out any promise of a useful result if successful, and partly because I feel pretty sure it would not succeed, except with machines having so much self-induction as to render them unsuitable for practical purposes. A little consideration will, I think, show that this must be the case.

Professor Adams refers to the loud humming noise made by my alternators when they were connected parallel while out of phase as "very sufficient evidence of very considerable self-"induction, in consequence of which the speed of one machine is "suddenly increased, and of the other as suddenly diminished." This noise is really a proof of the contrary. It is caused by the enormous rush of current which the comparative absence of self-induction renders possible, and this momentary rush puts the machines in phase with a jerk. As bearing on this point, I may mention that some machines that are said to have large self-induction do not make this noise under the same circumstances. Their self-induction prevents any such rush of current, and, according to my view, also prevents their working parallel. Although dissenting from my explanation, this rush of current is recognised by Mr. Swinburne (p. 656) as a proof of absence of self-induction.

It is interesting and important to consider another action arising out of this matter. In several machines—some with and some without iron—the self-induction is so serious that it does as well to short-circuit armature coils as to cut them out when it is desired to reduce the potential difference at the terminals. In fact, these machines cannot be injured by accidental or intentional short-circuits. They obtain this advantage, however, at the cost of every other good quality. The rush of current, the effect of which Professor Adams noticed,

Mr. Mordey. but misunderstood, in my machine, could not take place with such high self-induction machines. They could be put into connection when out of phase, and would simply choke back the current which attempted to put them into step.

I think there is no real difference of opinion between Major Cardew and myself on the subject of safeguards. I quite agree with him that the two contacts *a* and *b* may be "ready waiting" for a person to make the other two contacts and so receive a shock. It is to prevent the possibility of this that I so strongly recommend earthing. It is an absolute safeguard, costs nothing, and does not cause interference with the telegraph or telephone service. I cannot admit that it increases fire risks. I think it lessens them, because it makes it certain that a faulty transformer or house circuit cannot remain on. The safety fuse on an earthed circuit instantly cuts off a faulty house, and so prevents that gradual accumulation of unsound or actually defective places that, without some such safeguard, would soon render a large supply service unworkable. As to the supposed excessive strain on a 100-volt circuit if it is earthed, that does not exist in practice. The secondary is earthed in the middle, so that the strain is really only 50 volts; and an installation that will not stand that strain is radically bad and ought not to be allowed anywhere. The great advantage of earthing is that it secures safety, protects the customer against bad work and leakage through his meter, and protects the system against the presence of rotten installations. If the fire offices do not see before long that earthing does not increase fire risks, I shall be greatly surprised.

One or two points in Dr. Fleming's remarks require some reply from me. His suggestion to account for the behaviour of transformers with different periodicities is, I think, very pretty and very ingenious. At first I could not fully accept it, as the greater penetration of the eddy currents into the iron at a low periodicity seemed to be an effect that would not be likely to be felt with very thin conductors, such as the laminated iron (see under "Conductors"); but I now see that the currents in the laminations, although very close together and insulated from

each other, can act on one another and produce the "virtual "resistance" effect. Mr. Mordey.

Professor Fleming's objection to my substitution of the word "magnetisation" for "induction" is not a very strong one. I have only to say that I think "induction" would not be a good word even if it had not already been secured for a dozen other purposes. Some limit should be placed to the multiplication of meanings given to this one word, which, after all, is not an electric or magnetic word at all.

Mr. Kapp has latterly given much attention to this subject of alternate-current working, and understands its difficulties. On this account, and also because I was obliged to strongly oppose some of his views, I highly appreciate his kind remarks about the paper. He accepts all my facts, but rejects my interpretation. He will, I feel sure, accept them both on further consideration.

The experiment of which he suggests an explanation, which is in some respects similar to the explanation proposed by Sir William Thomson, is no doubt a very interesting one. It is a case not very likely to occur in practice, but deserves attention. I do not, however, quite agree with Mr. Kapp and Sir William Thomson. I wish their explanations were more convincing, and were not so much opposed to Professor Ayrton's measurements of the self-induction with different relative positions of armature and field, and with various field strengths, which showed that the actual conditions are exactly the reverse of those relied upon in Mr. Kapp's supposition.

I think that in any case the actual magnetising or demagnetising action exerted by the armature on the field magnet must be small, partly for the reason, pointed out by Professor Silvanus Thompson, that there was not time in a half-period for the solid iron to undergo any change, and partly because the armature is purposely made so as to exert as small an effect of that kind as possible. The machine consists of a very powerful electro-magnet, the field; and a very weak electro-magnet, the armature. Even if coils similar to the armature coils were wound on the extremities of the polar horns, and had a direct current

Mr. Mordey. sent through them equal to the maximum working alternate current, they would not produce a very powerful effect on the field.

One of the difficulties met with in considering this question is that there is no means of knowing which machine was generator and which motor. This depends, of course, upon the relative power of the engines. It is quite possible that the lower E.M.F. machine was acting as the generator.

I can only say that I will take the first opportunity of acting on Sir William Thomson's suggestion as to testing the supposed changes of magnetisation, and hope thus to be able to settle this question.

Mr. Swinburne challenges my conclusions on several points with his usual vigour, and I very willingly take up the gauntlet. I have already alluded to some remarks of his which place him in the honourable light of an interpreter of Dr. Hopkinson's mathematics. The position is one that requires great powers of endurance and considerable courage.

Mr. Swinburne says that "electrical engineers do not realise "what is wanted in parallel running." I accept this candid statement as, at any rate, quite correctly describing his attitude in the matter; and am therefore, of course, prepared to find that he disagrees with most of my views.

He commences by stating that so far from the motor being kept in step because its self-induction was small, that, if there were no self-induction, the generator would have no control at all over the motor.

This is an instance of a very common mistake—that of supposing that if there is practically no self-induction there can be no generating or motive capability. The exact contrary is the case, as I tried to explain in the paper when I expressed the view that "a perfect alternator for any and every purpose should have no "resistance and no self-induction" (page 591). Of course this condition is unattainable in practice. It is a mere expression of the direction in which it appeared desirable to advance; and, as I have already said, I am prepared to put it that the most perfect alternator conceivable is one which has an indefinitely

small self-induction and resistance. This idea that the generating or controlling power is connected directly with the self-induction is clearly upset by the fact, shown by Professor Ayrton's experiments, that the self-induction is not sensibly altered by variations of the field strength or position. I need scarcely repeat that the generating or controlling power depends almost entirely on the field strength and position relatively to the armature. Mr. Mordey.

But Mr. Swinburne sticks to the ordinary fallacy and says—or implies—that such a machine would not act at all either as generator or motor, and reproaches me for attacking existing theories without giving one of my own to take their place. Even that course would have been quite justifiable if I had found the existing theory bad. But I have given a theory. I have said what I thought a perfect generator or motor should be, and have attempted to explain the *rationale* of the action of two such machines when controlling each other. Let me take a case. The armature of my machine has a certain resistance and a certain self-induction. These qualities are unfortunately unavoidable, but I made the machine as good as I possibly could by reducing them both. This was done by the use of a strong and "stiff" field. Need I explain that, if the properties of iron had permitted it, I should have been able so to increase the field as to allow of a proportionate reduction of the resistance and of the self-induction of the armature? And if I could have got an indefinitely strong field I should have reduced the armature resistance and self-induction also indefinitely. Then I should have the "perfect "alternators" that I alluded to working as generators or as motors, singly or parallel.

But Mr. Swinburne, bearing out his opening statement, implies that such machines would not work at all.

Then, again, speaking generally, he says: "To make them run "parallel commercially needs a large corrective tendency. This "can only be got by making the machines so that the maximum "current is not at the same time as the maximum E.M.F." This is exactly the opposite of the actually best conditions. Of course they need a large corrective tendency and great stability, as Dr. Fleming says. I urged this strongly in my paper

Mr. Mordey. (page 591). Inverting Mr. Swinburne's statement—which really means that the machines must have large self-induction—I say this large corrective tendency can only be got to its fullest extent by making the machines so that the maximum current is at the same time as the maximum E.M.F., at any rate when working on an inductionless circuit. Mr. Swinburne quite correctly interprets the restriction imposed by the conditions he thinks best when he says "that the machines must be made larger to give a given "output. They must also be somewhat less efficient." The whole object of the first section of my paper was to show that such views were both practically and theoretically wrong, and I am surprised that a literal restatement of them should now be put forward.

The best practicable alternator for any and every purpose should have the lowest practicable resistance and self-induction. It will not be larger for a given output or less efficient. It will not have its maximum current at a different time from its maximum E.M.F., and it will exert a large corrective tendency in the most powerful and unhesitating manner, and will maintain synchronism with the least difference of time-phase between any two machines. Such are the views to which I am driven by the logic of facts. On one point I agree with Mr. Swinburne—that if a large corrective margin is required, the internal loss must be large; but even here we do not fully agree. With the old theories the corrective margin was out of all proportion to the effect required to be produced. By the sudden and strong action I obtain by departing as far as possible from the old theories I get this large corrective margin of power by a small margin in the capacity of the machine. It is obviously unwise, however, to attempt to run machines parallel when they require to exert a large controlling power. My experiments showed that they could exert this power; but the driving plant that requires large control is not suitable for parallel working.

Mr. Swinburne's calculations on the efficiency and dimensions of transformers are, I am sure, very interesting; but I scarcely think we are yet in a position to settle these matters entirely on paper. There are so many variables, and there is so little actual

knowledge of the laws governing the heating by eddies and by hysteresis in transformers, that it is probably best to rely mainly on experiment for the present. Thus Professor Ewing says (page 642) in his letter: "We cannot be sure that the loss by "hysteresis is proportional to speed, especially when the speed is "high;" and my experiments on armature cores support this view. Mr. Swinburne, however, is sufficiently sure on this, and all the other points, to rely upon his calculations for transformers working under widely different conditions. I admire his courage, but advise him to be prepared for disappointments when the transformers are constructed. I cannot at all agree, either, that the eddies in the iron are negligible, in spite of any calculations showing them to be inappreciable. It will be found that my tests bear out very closely the results as regards loss that are known to be obtained in low-period direct-current machines. Those experiments confirm, as regards hysteresis, the results of Ewing and Hopkinson; and as to the eddies it is open to anyone to check them by experiment or by reference to any reliable tests. I would, however, point out that in the celebrated paper of the Drs. Hopkinson on "Dynamo-electric Machinery,"* one of the two power readings which are available for this purpose is wrong, the total loss of power being given as less than the calculated hysteresis loss. I have pointed out this discrepancy to Dr. Hopkinson, and find that he is aware of it. It was no doubt an incorrect reading. Mr. Mordey.

Professor Silvanus Thompson and Professor Forbes ask for information as to the governing of the engines driving the two alternators in the experiments. Those engines were not governed automatically at all; they were simply controlled by hand. Of course, if they had been well governed automatically, the tests would have been much less severe. As it was, it was left to the engine-driver to give each engine as much steam as he thought was necessary. In such a case, if one engine is more powerful than another, the power of mutual control is severely tested; if the engines are working under similar conditions, then the

* *Phil. Trans.*, Part i., 1886.

Mr. Mordey. corrective margin is not required to be large. The utmost strain is put on the arrangement when, as was done in the experiments, steam is entirely shut off one engine.

I trust Professor Thompson will continue his very interesting experiments on the seat of the back E.M.F. in arcs. The subject requires investigation. The method used will probably yield useful results as regards a comparison between direct- and alternate-current arcs. There is one point to which I wish to draw Professor Thompson's attention, and that is whether there is, in an A.C. arc, an effect of the opposing E.M.F. in producing a displacement of the current phase. I suppose it may be taken as proved that the arc has an E.M.F. as well as a resistance; and with a rapidly alternating current it seems possible that this may produce a lag, and may act in that respect something like electromagnetic self-induction. Such an effect may perhaps also be found in A.C. electrolysis. I note Professor Thompson's criticism of my statement that in the characteristic the drop is partly due to resistance and partly to self-induction. He does not say that I am wrong. Several effects are produced by the currents—eddy and useful—generated by motion, and it is, I think, allowable to describe the net result on the characteristic as due to self-induction.

I have already referred to Professor Thompson's expression of doubt as to whether there is time in a half-period for the large mass of iron in the polar horns to become magnetised. He is probably quite right, but I have had no opportunity of settling the point. If there is any effect it will probably be almost a steady one, as pointed out by Sir William Thomson; that is to say, the iron will not follow the varying impressed magnetising force, but will take up an average state, depending upon the current and the relative position of armature and field.

Professor Ayrton, in his opening words, appears to ally himself with the views that I have sought to upset; but I have to thank him for having contributed an account of experiments which, perhaps more than anything I can say, tend to support my contention. He referred to the method of testing transformers that I suggested. That method may be safely used, except with a very inefficient transformer.

As to the curves of temperature, I can only say that I have no reason to doubt the accuracy of the tests, which were most carefully carried out. Mr. Mordey

Professor Forbes pointed out that it is wrong to suppose there are only two means of distributing alternate currents. I quite agree with him; but although I have dwelt at some length on the subject of working parallel, I do not think that method is suitable for all cases. It is a great convenience, and in a properly designed station can always be adopted; but where, either from the design of the machines or from the character of the engines, a large corrective margin is necessary, then it may be better to work singly, putting such of the mains in parallel on one machine as may be convenient.

If the alternators are suitable for parallel work, then the different mains may be run parallel from a common or omnibus pair of terminal bars, each main being fed through a safety fuse, in order to prevent more than local extinction in case of a short-circuit.

With regard to Mr. Zipernowski's communication, I am sorry he should think the statements I made about working his alternators parallel are erroneous. Those statements are substantially quotations from his own writings. He objects to a passage in my paper in which I said that his reason for working at the very low periodicity of 42 was to enable him to run the machines parallel. My authority was his own letter, published a few days before my paper.* In this letter—to quote it more fully—he said: "The "relatively low number of alternations (5,000 per minute), which "we have chosen on principle . . . that we by this means are "enabled to couple our dynamos in parallel is not a secondary "result, but just the end we were aiming at." Mr. Zipernowski now writes (page 672): "We state that we have not reduced the "periodicity because of the coupling in parallel."

I must leave these two opposed statements to speak for themselves.

The other statement which Mr. Zipernowski questions is that

* *The Electrician*, May 10th, 1889.

Mr. Mordey. the magnetic flux in the field ought to be constant, and I am blamed for not giving my reasons for this statement. I did not go into the matter very fully, as it seemed unnecessary; but when I said that want of constancy "necessarily led to losses in the "field, the lamination of which in the Zipernowski machine "showing that such losses must be serious" (page 629), I am sure I sufficiently indicated the nature of the waste which occurs under the conditions referred to.

I am glad to have elicited Mr. Zipernowski's remarks about the Ganz alternate-current motor, and shall look forward to seeing an actual description of that apparatus. The publications on that subject, to which my attention is called, are remarkable for the small amount of definite information they contain. I can learn nothing from them. We are not told anything at all about the form, mode of action, or principle of the machine. There are no doubt good and sufficient reasons for this reticence.

Of Mr. Fricker's contribution I need only say that it is a very clear statement, with which I wholly agree.

The PRESIDENT: I will now call on Mr. Hookham, our newest acquisition as a member, to describe the current-meter which he has been good enough to bring before us this evening.

Mr. Hookham. Mr. G. HOOKHAM: I am very much obliged to you for the indulgence which you have given me this evening, in prolonging the meeting on purpose to hear me explain my meter. Of course I understand that the time is extremely short, and I will make the best use I can of it. My meter is a rather small affair, and it will be impossible to explain it satisfactorily to a meeting like this without diagrams; but I knew that my time in any case would be extremely short, that I should not be able to make use of the diagrams, and that I must content myself entirely with just placing the meter on the table and introducing it to you, doing what I can to explain its working parts from the meter itself.

The meter consists essentially of an electro-motor, and is self-contained, requiring no extraneous motive power like clockwork or anything of that sort, the field being provided in this case by

permanent magnets. The brass tube contains 10 or 12 bar-magnets of tungsten steel, extremely hard, weighing about half a lb. apiece, so that there is perhaps 5 lbs. or 6 lbs. weight of tungsten steel in the brass tube. The iron pole-pieces fit fairly well between the magnets and the cast-iron, so as to reduce the air-space resistance at those points. There is a simple anti-friction train, the practical use of which is that it avoids the necessity of lubrication. A double mercury commutator is employed, the connections between the sections on the two halves being so arranged, that when the two halves are dipping with their lower edges in two mercury cups on the same level, the effect is the same as when in an ordinary dynamo one brush presses on one side of the commutator and the other on the opposite side. [Detail working of meter described.] Mr. Hookham

The proportionality is obtained in this way. This wire is mounted on a solid copper disc, and as that revolves in the field, Foucault currents are generated in the copper disc, at the sharp edges of the pole-pieces, and the work done on those Foucault currents will be as the square of the speed; the brake-force increases as the speed, consequently the speed will vary directly as the driving force. In this case the armature carries the current to be metered, revolving in a constant field, consequently the speed will vary directly as the current. I cannot in the time at my disposal show you its proportionality, but I think every electrician present will understand that if we can get rid of friction—*i.e.*, make all other work done negligible compared with the work done on Foucault currents—the thing must be proportional; as a matter of fact, it is most accurate. Of course, for a very small current, where the driving force is also extremely small, friction, both static and the liquid friction of the mercury, enters as a disturbing cause. Thus, in a 40-light meter, with one lamp there may be an error of perhaps 25 or 30 per cent., with two lamps 10 per cent., with three or four lamps it practically disappears, and for all numbers above that it is absolutely proportional. [Meter shown in operation.]

The power is supplied by cells kindly lent by the Electrical Power Storage Company. The meter has been in use 18 months,

Mr. Hookham.

and there has been no falling-off in the field. In our recent patterns we omit the short-circuiting bar, as we find it quite unnecessary.

The PRESIDENT: You have not made it quite clear on what fundamental principle the meter rests—that is, the way the magnetic brake acts.

Mr. G. HOOKHAM: The brake is made by Foucault currents generated in the disc, and the brake increases proportionally to the speed. Ten amperes are now running, and that is the full current for which we sell this meter; it is now running at 14 amperes, but it is inconvenient——

The PRESIDENT: Inconvenient in what?

Mr. G. HOOKHAM: It might throw the mercury about.

The PRESIDENT: I have seen the action of the mercury on the pieces of copper dipping into it.

Mr. G. HOOKHAM: In the shunted meter we use an armature of comparatively high resistance, so that the variation of resistance at the mercury contacts—the only variable part—is unimportant. We tested this variation on a meter whose armature had a resistance of 1-500 of an ohm, and found a variation of 10 per cent. The armature of this shunted meter has a resistance of ·1 ohm, so that the error is reduced fifty times. I wish to call your attention to this form, as an important point is involved. When showing it to Sir William Thomson, he said I should find an error when there was a variation in atmospheric temperature, the shunt conductor being of platinoid and the armature of copper; but it is a curious fact that there is a compensation here for the temperature error. We have heated the meter up to 50° F. above the atmosphere: the resistance of the german silver remains constant, consequently it gets more current; but at the same time the disc is heated up so that the resistance to the Foucault currents is increased, and consequently the work done less; and up to 50° we find no temperature error in a meter shunted with platinoid.

The PRESIDENT: Will you say a word or two about the metal contacts?

Mr. G. HOOKHAM: That is a point we had to test in practice, and we have had to appeal to it as the only test for showing

which is the best form of contact. We have had meters under observation now for more than eighteen months; we have tried platinum and other forms of dry contact, but the best of all is an amalgamated contact along the edge of a copper commutator plated with nickel, the nickel being removed along a fine line at the edge of each section. This remains quite unaltered month after month, and we never have found any trouble with that, and the resistance of an amalgam contact is practically nil. Mr. Hookham.

Professor J. PERRY: May I ask what Mr. Hookham finds is new in this? Is it the shape of the magnet he has used, or what does he claim as new?

Mr. G. HOOKHAM: I quite understand the bearing of Professor Perry's question. What is new is that this is a working instrument. I am quite aware of, and I have not the slightest wish to disparage, the work done by Professors Ayrton and Perry; I am familiar with the specification in which the principle is laid down independently. This is not the time to discuss such a question as that; but I suggest to Professors Ayrton and Perry that they have not described a working instrument in that specification.

The PRESIDENT: The principle of using Foucault currents to control speed for an electric meter was, I believe, first suggested by Messrs. Ayrton and Perry, and thus became known to most of us. The way of carrying it out in Mr. Hookham's meter is decidedly novel, I believe; and, when we consider the difficulties to be overcome, we cannot but admire the result. Mr. Hookham's meter has been in practical use for some time, I believe? Sir William Thomson.

Mr. G. HOOKHAM: There are about 200 in use, and the longest has been in use for about 20 months.

The PRESIDENT: The exceedingly convenient adjustment of the magnetic field is important, but I am still more interested to learn that no readjustment has hitherto been found necessary. That shows that the magnetic part of the instrument is of a valuably durable character.

Mr. R. E. CROMPTON: I have had some experience with meters used on central station work, and have recently, through the kindness of Mr. Hookham, had an opportunity of testing one of his meters, or rather, I should say, that it has been tested for Mr Crompton.

Mr. Crompton. me by the resident engineer of the Kensington Court Company. From our tests it appears that this form of meter registers in favour of the Company supplying at very small loads, although it is correct at ordinary loads.

Mr. G. HOOKHAM: I ought to say that Major Cardew has had the meter under test for months, and has reported favourably on it.

Mr. R. E. CROMPTON: I cannot account for this peculiarity in which this meter differs from all others, other than by supposing that the braking action of the Foucault currents induced in the copper have not sufficiently compensating effects at the lower portion of the range.

Mr. G. HOOKHAM: I understand you to say it is in favour of the Company.

The PRESIDENT: It is against the Company.

Mr. R. E. CROMPTON: I state that it is in favour of the Company.

The PRESIDENT: It is against the Company.

Mr. R. E. CROMPTON: I think you will find the facts are as I say.

Mr. G. HOOKHAM: On theoretical grounds.

Mr. R. E. CROMPTON: Yes, on theoretical grounds. I do not attach great importance to this point. I asked it more with a view of ascertaining whether others have noticed the same thing. I would desire, however, to testify my admiration of the meter itself, which has been beautifully thought out, and the design has been well worked out from a mechanical point of view.

Sir William Thomson. The PRESIDENT: In regard to the consumer and the supplying company it is rather the other way, because at all speeds the Foucault brake-current is in simple proportion to the speed, while friction is greater relatively at low speeds. Thus, at the smallest rates of consumption, the error is favourable to the consumer.

Mr. R. E. CROMPTON: Have you made any test, Mr. Hookham, to see if that is the case? I think you will find that the curve is very much flatter.

The PRESIDENT: Every dynamo affords a demonstration that

the E.M.F. is proportional to the speed. The question, then, is merely a matter of resistance. Sir William Thomson.

Mr. R. E. CROMPTON: I have no doubt it is, and it has very great range, but during the early part of the range I do not think it is so.

The PRESIDENT: From the most absolutely infinitesimal speed to the highest speed, the Foucault current is simply proportional to the speed, except so far as resistance is altered by temperature. The importance of Mr. Crompton's remark as to the accuracy of a meter at the small output is manifest. In respect to the times of small consumption, there are two views as to the overcharging and undercharging the user. It is very cheap for the Company to give a small current all day long, for the Company is obliged to have work kept on continuously during the whole 24 hours; and, provided it is not too great, the demand may be not enough to sensibly increase the work required of the engines, or the fuel which must be used to keep them going. In that case the Company might be glad to give each consumer a quarter of an ampere of current for nothing, simply because it cost the Company nothing to produce. There is no doubt that the best meter is one that records most accurately through all ranges from the lowest to the highest, and if there is a fault I agree with Mr. Crompton that it ought not to be against the consumer, but rather against the Company, for the very smallest currents; and I may say, from tests that Mr. Hookham showed me in my own laboratory, we found it so. It recorded too little in proportion for the smallest currents; but from half an ampere or an ampere upwards it gave a wide range of remarkably accurate simple proportion to strength of current in its record.

Mr. S. JOYCE, jun.: It is well known that instruments of the so-called permanent magnet class may, with great care, be kept fairly constant in strength, and, indeed, we have had in Messrs. Paterson & Cooper's experience several examples of this. We have had, for instance, an instrument of Ayrton & Perry's old style permanent magnet type come back for re-testing after being in use for six years, and have found no practical variation in the strength of the magnet. But the objection to so-called Mr. Joyce, jun.

Mr. Joyce, jun. permanent magnets is that they may be easily altered in strength by being placed near a dynamo or other powerful magnetic apparatus; or even by vibration of their particles due to knocks, especially when in a magnetic field. I would ask Mr. Hookham whether there is any provision to prevent the customer or other person from tampering with the strength of the magnet of his beautiful meter? Is the case which covers the instrument when in use of stout iron, and is it difficult of removal?

Mr. S. JOYCE: I mean is it protected from interference on the part of the consumer?

Mr. G. HOOKHAM: But would you let him take the cover off or not?

Mr. S. JOYCE: That is rather a question of what arrangement you make.

The PRESIDENT: It would be a very clever consumer that would increase the strength of the magnet, and if he diminished it he would have double to pay.

Mr. Hookham. Mr. G. HOOKHAM: I was sorry to see that Major Cardew had left the room when I heard Mr. Crompton's remark, because, except myself, I think he has made most experiments on the meter, and knows best that it measures in favour of the consumer with small currents. We have tested hundreds of them, and we find this the case with every single meter, and on theoretical grounds it must be so: there is a certain strength of current when the armature does not move at all, and the consumer gets his current for nothing. The permanency of the magnet is of course a question of the greatest importance, and I should like to have given a much longer explanation than I have in disposing of it to-night. I should like to explain how the magnets are made. This type is made with a good many turns of wire, and a saturating current is flashed through them. We then take the speed, and then this is hammered with a heavy weight of copper, as much as the construction of the magnet will stand, and we can rarely reduce the field more than one per cent., owing to the low resistance of the air space. We then by a reverse current reduce it about 10 per cent.; after that the tendency of the magnet is upwards for a considerable time. What the history may be

afterwards I cannot say, but the tendency of the magnet is to strengthen a very little for weeks after that treatment. I would ask Mr. Joyce if he has ever had experience of a permanent magnet the strength of whose field rises after he has performed his experiment on it? Mr. Hookham.

Mr. S. JOYCE: Yes; in the calibration of our magnets they are worked down, and are never sent out with their full strength, and we often notice that after weakening a magnet it has a tendency to rise in strength.

Mr. G. HOOKHAM: It would, no doubt, if it was very much weakened; but if only weakened 5 or 10 per cent. would that be so? I think Professor Hughes found that a magnet reduced to zero by a reverse current rose in strength on the removal of the current, and continued to rise. In practice, after 18 months' testing, we have never found the smallest perceptible variation, and the severest mechanical treatment makes no difference to our magnets.

Mr. B. W. SMITH: I do not quite see how it depends on the strength of the field.

Mr. G. HOOKHAM: The stronger the field the slower it goes.

A hearty vote of thanks was unanimously accorded to Mr. Hookham for his communication.

A ballot took place, at which the following candidates were elected:—

*Member:*

Leonard William Holmes.

*Associates:*

Roland Chambers.
Percy B. Crowe.
Capt. W. F. Hawkins, R.E.
Frank B. Lea.
Dr. Henry Leipmann, F.C.S.
Allen F. Scott.
George Crosland Taylor.
James Taylor.
John Williamson.

*Student:*

Herbert Carpmael.

The meeting then adjourned.

# ORIGINAL COMMUNICATIONS.

THE POLYTECHNIC, BOURNEMOUTH,
31st *May*, 1889.

DEAR SIR,—As the information may be of some interest to members, I quote the results of tests of my lightning conductor at Dhubri, in Assam. It is what I call my pattern, or the "Arborial" Lightning Conductor, made of eight No. 9 B.W.G. wires, worked up into a cable with top branches, consisting of the wires themselves and a long tap root with rootlets of the same. I cannot lay my hands on the different measurements at the present moment, but considering that my house stands on a dry gravel site, the results obtained by my method of dealing with the earth, or roots of the conductor, leave little room for improvement if minimum of resistance is to be accepted as the test of a good conductor; and the fact that although standing in the most exposed part on the banks of the river Brahmaputur, and that the supports of the roof consist of iron posts, for so many years, and amidst the most violent thunder-storms, the house has never been struck, though built over thirteen years ago:—

| | | | | | |
|---|---|---|---|---|---|
| 15th May, | 1878—Resistance, | | 11·92 | B.A. | units. |
| 27th " | " | " | 3·564 | " | " |
| 1st June, | " | " | 2·20 | " | " |
| 12th " | " | " | 4·78 | " | " |
| 20th " | " | " | 4·03 | " | " |
| 21st October, | " | " | 5·40 | " | " |
| 30th " | " | " | 2·35 | " | " |
| 28th February, | 1879 | " | 5·49 | " | " |
| 4th May, | " | " | 2·54 | " | " |

From May, 1878, to July, 1879, all the other results were 3·, the above figures being the only variations of note in the *fourteen* months. The first result, 11·92, was due to the leading wire from my house to the telegraph office testing-room being

No. 12 B.W.G., which was changed to No. 1 B.W.G. Leaving out this exceptionally high resistance explained by the high resistance of the lead, the average of 29 tests, giving two per month for the fourteen months, I get 3·085—even this was due to the distance—and the temporary nature of connections, when tested at the house, the resistance was ·35!

It would be interesting to know from other members if they can improve on this, or beat my record, as I laid out over 60 rupees (£6) in completing my earth trenches, &c., but I doubt not, under all the surrounding circumstances, it was money well laid out. Like the late Dr. Mann, in Africa, I feel some interest in my Indian Monument to Science.

It would also be of great interest to know the experience of members in dealing with linear *versus* other forms of earth for both lightning and telegraphic purposes. I helped the late Mr. Schwendler in some very interesting experiments in connection with this important branch of the subject, when, by the use of what he was good enough to term "McGregor's exploring wire," we sought and found spots of least resistance. Some day I will be glad to send you an account of these experiments.

Meanwhile I hope the Editing Committee will consider the present communication deserving of a corner in our *Journal*. I only regret, and regret most sincerely, that the state of my health prevented my taking part in the late discussion on Professor Lodge's most interesting paper.

W. McGREGOR, Member.

The Secretary,
Institution of Electrical Engineers.

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## REPORT OF A FATAL ACCIDENT FROM LIGHTNING, IN INDIA.

By P. V. Luke, Member.

Arratoon Apcar, *April 12th*, 1889.

I send you an account of a fatal accident from lightning, which you may consider of sufficient interest to publish in our Journal. It is incomplete, in so far that I am unable, from there not being any offices of the Telegraph Department supplied with

testing instruments near the spot, to give the electrical resistance of the "earth." It may, however, be inferred, from the fact that the circuit always worked well, that at any rate the earth was a fairly good one.

The accident happened at a place called Gnatong, in Sikkim, the present terminus of a field-telegraph line put up last year, from Darjeeling, in connection with the Sikkim Field Force operating against the Thibetans. The telegraphic office is probably the highest in the world, being situated at an altitude of 12,200 feet. The fort of Gnatong, in which is quartered a wing of a British regiment, is in a hollow, surrounded by hills at no great distance on all sides. At the bottom of the hollow is a large pond, frozen over during the winter, affording grand skating and sliding for the soldiers.

The fort is built on rising ground just above the pond, and the telegraph office is in the fort. The building is of wood, with stone foundations and stone ends; a roof of wooden shingle with a zinc-covered ridge. Half of the building is used as a telegraph and half as a post office. The rubble masonry partition between the two contains the fireplaces and chimneys, and on shelves over the fireplace in the telegraph office the batteries are placed to keep them from freezing. The soil on which the building is erected is rocky, and the floor of the office is formed of rammed earth and stones.

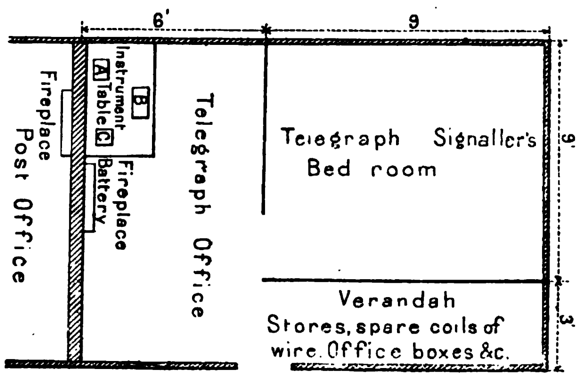



Fig. 1.

The above sketch (Fig. 1) shows roughly the arrangement of the

rooms. On the instrument table, shown enlarged in Fig. 2, A is the lightning discharger; B, the instrument; and C, a call bell.

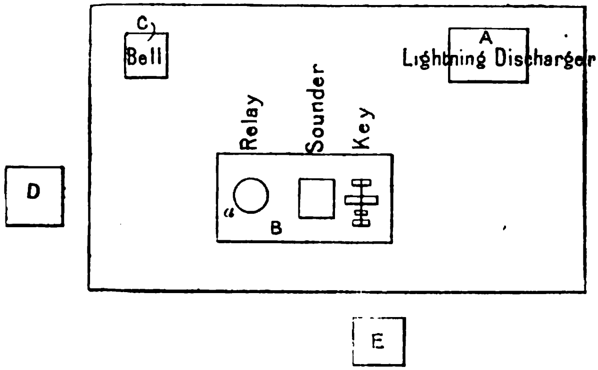



FIG. 2.

The lightning discharger is a more or less rough one, used only in field equipments, the object in view being lightness. It consists of three brass plates about 1 in. wide mounted on a wooden board 5 in. × 4 in., thus—

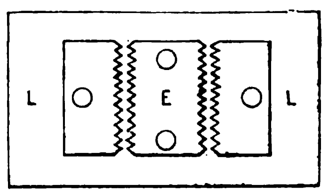



FIG. 3.

The instruments are a relay, sounder, and key mounted on one board, with the usual connection for working direct or in translation.

At about 9 p.m. on the 25th March, 1889, two military signallers, named Clarke and Hewett, were in the office, and at the moment the accident occurred they were in the position shown in Fig. 2.

Clarke was standing at D, with his elbow on the table, bending over the relay. Hewett was sitting at E, with his hand on the

key, calling up the stations on the line to see if they were clear before closing for the night. When the flash came it took a small piece out of the brass cover of the relay, at (*a*), and struck Clarke, who was killed instantaneously, falling on Hewett, whose legs, by the same flash, were paralysed. At the time, the light (a hurricane lantern) was extinguished. The only mark on the man killed was a round ring on the left cheek, which appeared the next morning, when the body was very much swollen, with signs of bleeding from the nose and mouth. There was no sign of actual scorching or burning; and the medical officer was of opinion that death was caused by a *shock*, and not by the man's being *struck* by a flash. The man Hewett is slowly recovering the use of his legs, which were temporarily paralysed.

The "earth" connection was made to a coil of wire buried in the earth just outside the office. The *same earth* was used for the lightning discharger and for the instrument. Neither the lightning discharger nor the key showed any distinct signs of a discharge having passed. The only visible damage done was the small piece of brass taken out of the rim of the cover of the relay.

The action of the lightning in this case appears to be quite unaccountable. Admitting that the flash came along the line into the office, and that the lightning discharger failed to act, why should Clarke have been struck in preference to Hewett, who had his hand on the key?

The line is an insulated one, supported on trees and wooden poles, running over great altitudes; and thunderstorms in that district are frequent. On two previous occasions military signallers working in the Gnatong office had received severe shocks There seems no doubt that the *line* was struck and the lightning conducted into the office, and that the lightning discharger failed to act; but the rest seems inexplicable. The relay coils were not fused, nor any damage done beyond the piece chipped out of the relay cover.

P. V. LUKE.

The Secretary,
Institution of Electrical Engineers.

# NOTES ON A STATIC ELECTRO-MOTOR.

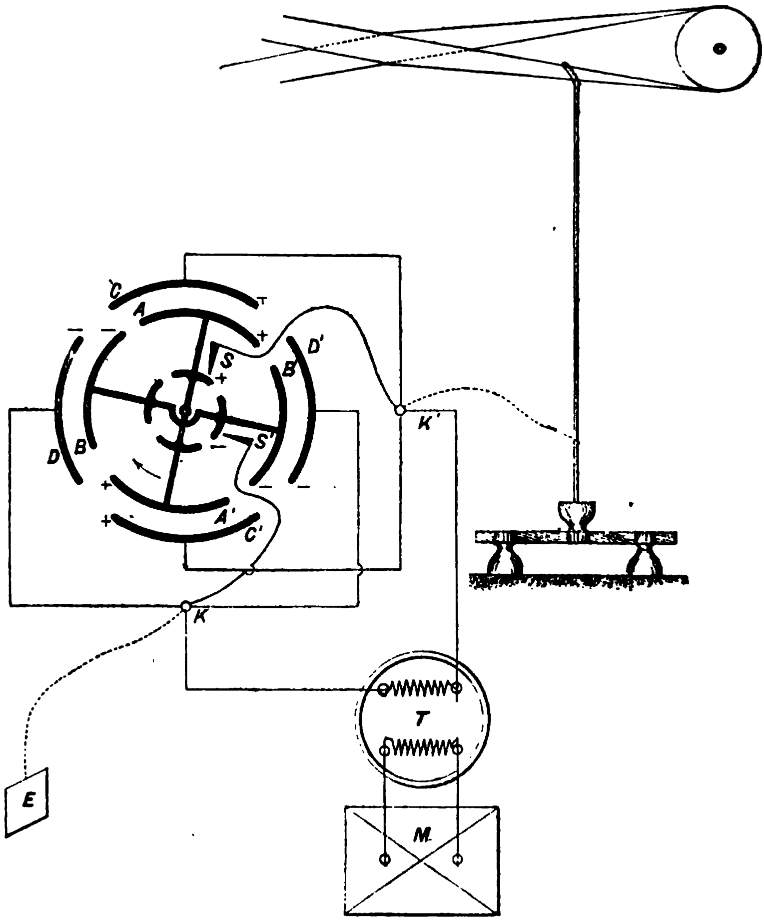
By CHARLES ZIPERNOWSKI.

The well-known fact that bodies charged with electricity of the same sign repulse each other was first employed by Franklin (in 1780) to produce a continual rotation. He used a horizontal wheel, consisting of horizontal glass strips provided with copper balls at their ends; the wheel was pivoted, and could revolve between the balls of two oppositely-charged Leyden jars. Its own balls could thus receive a charge of the same sign as that of the jars, being, therefore, repulsed by the latter. After 180 degrees of revolution each of the balls discharged before the oppositely-charged Leyden jar, and, at the same time, took away part of the electricity summed up upon the latter. A new repulsion, therefore, ensued, and so on. Franklin called this apparatus the "electric turn-spit."

Later on, Poggendorf showed that a pivoted disc of glass or ebonite begins to revolve when charged with quantities of electricity of opposite signs by means of two oppositely-placed combs of points, provided that it receives an impulse in any direction. Poggendorf also caused an influence machine of Holtz' to revolve by charging its collecting spikes by another influence machine.

In order to study the so-called static actions of high tension alternating currents, I have, amongst other things, constructed a small rotatory apparatus, derived from Thomson's quadrant electrometer, in the idiostatic form invented by Joubert. I have succeeded in making this apparatus revolve speedily with a high tension alternating current as well as with a high tension continuous current. In the former case, the speed was so high that the number of revolutions could no longer be counted by the eye. In the latter case, I took the charge from a belt between the working motor, a high speed engine, and the testing room of the electrical department of Messrs. Ganz & Co. of Budapest. Of course, the speed was still a higher one than with 2,000 volts of alternating current.

The figure is a sketch of the connections in the two above-named cases.




The movable part of the apparatus consists of two couples of aluminium sectors, shown as quadrants $A$ $A'$, $B$ $B'$, for the reason of simplicity. The fixed part consists of four double sectors of brass embracing the movable ones. They are likewise represented as quadrants $C$ $C'$, $D$ $D'$. The movable part is, besides, provided with a commutator of four parts, from which the two couples of sectors $A$ $A'$, $B$ $B'$, are charged, by means of points $S$ $S$, with electricity of the same sign as that of the opposing fixed couples of sectors. Connecting the terminals $K$ $K'$ to those

$S$ $S'$ of the transformer $T$, the low tension of the alternate-current dynamo $M$ will be transformed into high tension, and the apparatus will start, provided that the movable sectors are not exactly opposite to the fixed ones. If you connect one of the terminals to the collecting spikes, and the other to earth, the result will be the same.

It is worth remarking that, with the second arrangement, an instantaneous charge of the sectors will suffice, as the fixed sectors do not change the sign of their charge; in the first case, however, a continuous charge is required, as the electricity must change sign simultaneously in the fixed and the movable parts.

It is not impossible that this apparatus, in some form, may be adapted for practical purposes, such as a volt-compteur for series working arc lamps; as recording apparatus for earth-circuits, or for the variations of the primary tension in central stations; as motor for very high tensions, &c.

BUDAPEST, *June* 24, 1889.

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**Bennett** [A. R.] Electric Traction. Sm. 4to. 13 pp. [East of Scotland Engineering Association. Session IV. Tenth Meeting.] *Edinburgh*, 1889

**Berly** [J. A.] Universal Electrical Directory and Advertiser, 1889. The Electrician's "Vade Mecum," containing a complete Record of all the Industries directly or indirectly connected with Electricity and Magnetism, and the Names and Addresses of Manufacturers in Great Britain, America, the Continent, &c. 8vo. 437 pp. *London*, 1889
[Presented by Messrs. W. Dawson & Sons (Publishers).]

**Blakesley** [T. H.] Papers on Alternating Currents of Electricity, for the Use of Students and Engineers. 2nd Edition. 8vo. 129 pp. *London*, 1889
[Presented by Messrs. Whittaker & Co. (Publishers).]

**Board of Trade.** [*Vide* Marindin, Major F. A.]

**Cundell** [Major J. P.] A Dictionary of Explosives. 8vo. 108 pp. *Chatham*, 1889
[Presented by Major Francis J. Day, R.E.]

* **Douglass** [J. C.] A Manual of Telegraph Construction: the Mechanical Elements of Electric Telegraph Engineering. 2nd Edition. 8vo. 468 pp. *London*, 1877

**Institution of Civil Engineers.** Minutes of Proceedings. Vols. XCV., XCVI. 8vo. 532 pp. and 480 pp. *London*, 1889

**Institution of Mechanical Engineers.** Proceedings, 1889. No. 1, January. 8vo. 201 pp. *London*, 1889
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**Italian Telegraphs.** Relazione Statistica sui Telegrafi del Regno d'Italia nell anno Finanziario 1887–88, Fo. 149 pp. *Roma*, 1889
[Presented by the Minister of Posts and Telegraphs, Rome.]

**James** [Charles Streatfield]. The "Morse" Signaller's Companion. Fo. 41 pp. *Calcutta*, 1888

**Marindin** [Major F. A.] Report to the Board of Trade with reference to Inquiry held under the Electric Lighting Acts, 1882 and 1888, for certain Provisional Orders and Licenses, dated May 9, 1889. Fo. 28 pp. *London*, 1889
[Presented by Major P. Cardew, R.E.]

**Rust** [Arthur]. Electricity: theoretically and practically considered, by the aid of Thermo-Electricity. 8vo. 32 pp. *London*, 1889

# LIST OF ARTICLES

RELATING TO

# ELECTRICITY AND MAGNETISM

Appearing in some of the principal Technical Journals during the Months of MAY and JUNE, 1889.

## I.—BATTERIES AND ACCUMULATORS.

P. Chroustchoff and A. Sitnikoff—Electro-motive Force of Batteries.—*C. R.*, vol. 108, p. 937, 1889.

— von Müller—Behaviour of the Zinc Electrode in Manganese Batteries.—*El. Zeit.*, vol. 10, p. 294, 1889.

W. Kohlrausch and C. Heim—Experiments on Accumulators.—*El. Zeit.*, vol. 10, p. 303, 1889.

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## II.—DYNAMOS AND MOTORS.

A. Witz—Pole-Reversals in Series Dynamos.—*C. R.*, vol. 108, p. 1243, 1889.

W. C. Rechniewski—Alternate-Current Motors.—*Lum. El.*, vol. 32, p. 301, 1889.

C. Reignier—Secondary Induction Phenomena in Dynamos.—*Lum. El.*, vol. 32, pp. 401, 464, 1889.

G. Richard—Details of Dynamo Construction.—*Lum. El.*, vol. 32, p. 521, 1889.

E. Meylan—Rechniewski's Dynamo.—*Bull. Soc. Int.*, vol. 6, p. 246, 1889.

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## III.—ELECTRO-CHEMISTRY AND ELECTRO-METALLURGY.

H. Pellat—Limit between Polarisation and Electrolysis.—*C. R.*, vol. 108, p. 1238, 1889.

A. Minet—Introduction to the Study of Electro-Chemistry.—*Lum. El.*, vol. 32, pp. 322, 427, 479, 530, 573, 1889.

P. H. Ledeboer—Electro-Metallurgy of Copper.—*Lum. El.*, vol. 32, p. 551, 1889.

C. E. Guillaume—Electrolysis produced by Minimum Electro-motive Force.—*Lum. El.*, vol. 32, p. 558, 1889.

Dr. T. Erhard—Electro-Metallurgy of Aluminium.—*El. Zeit.*, vol. 10, pp. 236, 262, 1889.

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## IV.—ELECTRIC LIGHT.

C. Carré—Comparison of different Methods of Illumination.—*Lum. El.*, vol. 32, p. 286, 1889.

W. de Fonvielle—Electric Light for Balloon Signals.—*Lum. El.*, vol. 32, p. 344, 1889.

E. Dieudonné—Electric Light at the Paris Exhibition.—*Lum. El.*, vol. 32, p. 351, 1889.

W. de Fonvielle—The Projector on the Eiffel Tower.—*Lum. El.*, vol. 32, p. 391, 1889.

A. Vernes—Electric Lighting of the Palais Royal.—*Bull. Soc. Int.*, vol. 6, p. 264, 1889.

O. von Miller—Berlin Central Stations.—*El. Zeit.*, vol. 10, p. 253, 1889.

## V.—ELECTRIC POWER.

G. Richard—Electric Railways.—*Lum. El.*, vol. 32, p. 207, 1889.

E. Dieudonné—Electric Lift at the Paris Exhibition.—*Lum. El.*, vol. 32, p. 516, 1889.

## VI.—MAGNETISM AND ELECTRO-MAGNETISM.

C. R. Cross and A. S. Williams—Strength of the Induced Current with a Magneto-Telephone Transmitter as Influenced by the Strength of the Magnet.—*Phil. Mag.*, vol. 27, p. 392, 1889.

J. Parker—Diamagnetism and Concentration of Energy.—*Phil. Mag.*, vol. 27, p. 403, 1889.

H. Becquerel—Effect of Terrestrial Magnetism on Atmospheric Polarisation.—*C. R.*, vol. 108, p. 997, 1889.

E. Marchand—Necessity of Introducing a Correction for Humidity in Magnetometer Observations.—*C. R.*, vol. 108, p. 1001, 1889.

— Chauvin—Rotatory Magnetic Polarisation in Iceland Spar.—*C. R.*, vol. 108, p. 1097, 1889.

J. Luvini—Terrestrial Magnetism and Sun-Spots.—*Lum. El.*, vol. 32, p. 312, 1889.

F. Larroque—Re-magnetism of Iron after Heating.—*Lum. El.*, vol. 32, p. 369, 1889.

## VII.—MEASUREMENTS AND MEASURING INSTRUMENTS.

Dr. G. Gore—Determining the Strength of Liquids by means of the Voltaic Balance.—*Nat.*, vol. 40, p. 93, 1889.

E. van Aubel—Resistance of Bismuth.—*C. R.*, vol. 108, p. 1102, 1889.

G. Richard—Horse-Power Dynamometers.—*Lum. El.*, vol. 32, p. 260, 1889.

E. Meylan—New Types of Galvanometers.—*Lum. El.*, vol. 32, p. 268, 1889.

— Guérin—Apparatus for Measuring the Resistance of Earths.—*Lum. El.*, vol. 32, p. 376, 1889.

F. Mélotte and G. Henrard—Measurement of Permeability.—*Lum. El.*, vol. 32, p. 415, 1889.

C. E. Guillaume—Use of Platinum-Iridium and other Alloys for Standard Resistance Coils.—*Lum. El.*, vol. 32, p. 451, 1889.

K. Waitz—Method of Measuring High Potential Differences.—*Ann.*, vol. 37, p. 330, 1889.

K. Strecker—Measurement of Self-Induction by means of the Telephone.—*El. Zeit.*, vol. 10, p. 289, 1889.

## VIII.—RAILWAY APPLIANCES.

G. LARMOYER—Flamache's New Block System.—*Lum. El.*, vol. 32, p. 459, 1889.

M. COSSMANN—Appliances Exhibited at Paris.—*Lum. El.*, vol. 32, p. 556, 1889.

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## IX.—STATIC AND ATMOSPHERIC ELECTRICITY.

E. L. TROUVELOT—Duration of Lightning Flashes.—*C. R.*, vol. 108, p. 1246, 1889.

M. WOLF—Disruptive Discharges in Gases.—*Ann.*, vol. 37, p. 306, 1889.

J. ELSTER and H. GEITEL—Method of Determining the Electrical Nature of Atmospheric Discharges.—*Beibl.*, vol. 13, p. 327, 1889.

S. ARRHENIUS—Effect of the Sun's Rays on Atmospheric Electricity.—*Beibl.*, vol. 13, p. 328, 1889.

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## X.—TELEGRAPHY AND TELEPHONY.

H. WUILLEUMIER—Microphonic Contacts and Telephonic Currents.—*Lum. El.*, vol. 32, p. 272, 1889.

P. H. LEDEBOER—Zigang's Electro-magnetic Telephone.—*Lum. El.*, vol. 32, p. 320, 1889.

E. ZETSCHE—New Multiple Switch-Boards for Exchanges.—*Lum. El.*, vol. 32, p. 468, 1889.

J. KAREIS—Stockholm Telephone System.—*El. Zeit.*, vol. 10., p. 244, 1889.

Dr. A. TOBLER—Wheatstone's Automatic System.—*El. Zeit.*, vol. 10, p. 266, 1889.

C. GRAWINKEL—Joints in Bronze Wires.—*El. Zeit.*, vol. 10. p. 293, 1889.

C. GRAWINKEL and K. STRECKER—Berlin Central Telegraph Office.—*El. Zeit.*, vol. 10, p. 296, 1889.

J. SACK—The Hughes Apparatus arranged for Use with Alternating Currents.—*El. Zeit.*, vol. 10, p. 311, 1889.

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## XI.—THEORY.

A. W. WARD—Magnetic Rotation of the Plane of Polarisation of Light in Doubly Refracting Bodies.—*Nat.*, vol. 40, p. 117, 1889.

J. BROWN—Helmholtz's Theory of Mercury-dropping Electrodes, and the Potential Difference between Clean Mercury and Electrolytes.—*Phil. Mag.*, vol. 27, p. 384, 1889.

H. A. ROWLAND and C. T. HUTCHINSON—Electro-magnetic Effect of Convection Currents.—*Phil. Mag.*, vol. 27, p. 445, 1889.

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A. STOLETOW—Actino-electric Phenomena.—*C. R.*, vol. 108, p. 1241, 1889.

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E. BICHAT—Actino-electric Phenomena.—*Jour. Phys.*, vol. 8, p. 245, 1889.

C. DECHARME—Differences between the so-called Positive and Negative Electricities —*Lum. El.*, vol. 32, pp. 218, 315. 366, 421, 475, 513, 563, 1889.

P. SAMUEL—Gaston Planté's Rheostatic Machine.—*Lum. El.*, vol. 32, p. 361, 1889.

J. ELSTER and H. GEITEL—Production of Electricity by Contact of Gases with Glowing Wires.—*Ann.*, vol. 37, p. 315, 1889.

## XII.—VARIOUS APPLIANCES.

G. RICHARD—Phonographs.—*Lum. El.*, vol. 32, p. 306, 1889.

G. RICHARD— Graphophones.—*Lum. El.*, vol. 32, p. 358, 1889.

E. MEYLAN—Recording Thermometer.—*Lum. El.*, vol. 32, p. 511, 1889.

P. LE GOAZIOU—Voting Apparatus.—*Lum. El.*, vol. 32, p. 568, 1889.

# JOURNAL

OF THE

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*Founded* 1871. *Incorporated* 1883.

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The One Hundred and Ninety-seventh Ordinary General Meeting of the Institution was held at the Institution of Civil Engineers, 25, Great George Street, Westminster, on Thursday evening, November 14th, 1889—Dr. J. Hopkinson, F.R.S., Vice-President, in the Chair.

The minutes of the previous Ordinary General Meeting, held on May 30th, 1889, were read and confirmed.

The names of candidates for admission into the Institution were read and ordered to be suspended.

The following transfers were announced as having been approved by the Council:—

From the class of Associates to that of Members—

Reginald John Jones.

From the class of Students to that of Associates—

Harris Henry Eley.

Donations to the Library were announced as having been received since the last meeting from the Institution of Civil Engineers; La Société Française de Physique; the Astronomer Royal; Dr. Friedrich Goppelsroeder; Messrs. Buck & Hickman;

Messrs. Saxby & Farmer; Walter R. Skinner, Esq.; Monsieur Georges Carré, Publisher; Messrs. Dawson & Sons, Publisher; *The Electrician* Publishing Co.; Messrs. Griffin & Co., Publishers; Messrs. Whittaker & Co., Publishers; Professor Dr. K. Ed. Zetzsche, Foreign Member; John Aylmer, Member; Dr. J. A. Fleming, Member; W. J. Hancock, Member; H. Graham Harris, Member; S. H. C. Hutchinson, Member; Professor A. Jamieson, Member; Dr. C. Lemon, Member; Professor Oliver J. Lodge, Member; and W. P. Maycock, Associate; to whom the thanks of the meeting were heartily accorded.

The following paper was read by the Secretary:—

# THE LIGHTING OF THE CENTENNIAL INTERNATIONAL EXHIBITION, MELBOURNE, 1888 AND 1889.

By K. L. Murray, C.E., Telegraph Engineer to the Victorian Railways, Member.

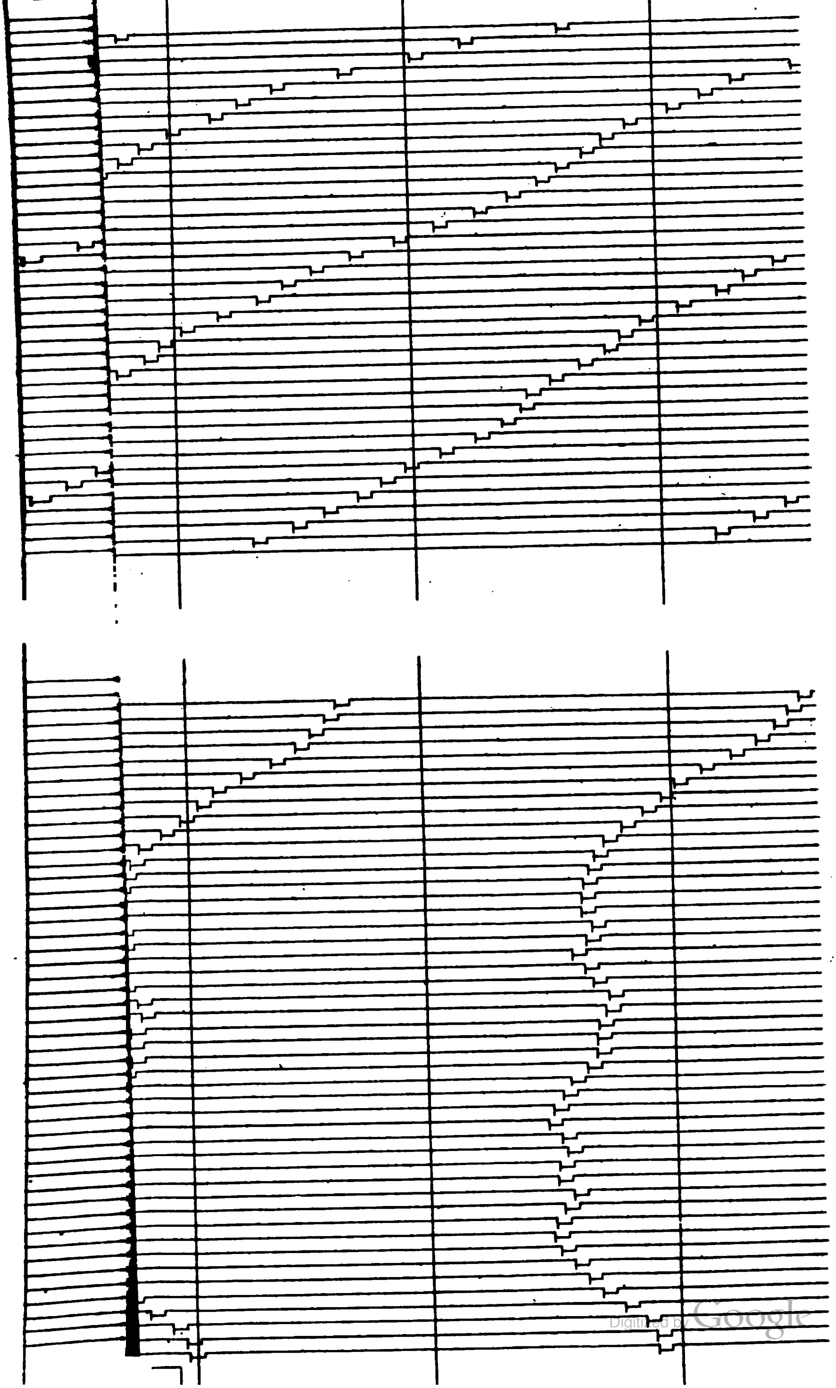
In November, 1887, the Executive Commissioners of the Centennial International Exhibition, which it had been decided to hold in Melbourne in the following year, consulted me as to the practicability, electrically and commercially, of using electricity as a means of lighting the Exhibition; and I felt that I could assure them that the electric light was the only one which could be depended on to efficiently illuminate the buildings and exhibits, and that its cost would not render its use prohibitive.

I recommended dividing the buildings into three sections, and calling for separate tenders for supplying and maintaining the dynamo machines and lamps necessary to light each part; tenderers being requested also to submit an alternative offer for the whole lighting required. My desire was to have each section lighted by a different system, if possible.

Finally, I was entrusted with the duty of drawing up a specification, and tenders were invited.

Those received all named such a high price for the work that I could not recommend the acceptance of any; but being

Pla



requested by the Exhibition Commissioners to arrange with one or more of the firms which tendered, I was able eventually to do so, and a contract was entered into with the Australasian Electric Light, Power, and Storage Company for the lighting of the greater portion of the Exhibition buildings; and an agreement was made with Messrs. Ganz & Co. to supply a transformer plant and light the Armament Court, the official reception and dining rooms, and offices.

The Australasian Company's contract was to supply, fit up, and maintain 825 Brush arc lamps, 37 Brush arc dynamos, 7 Brush Victoria dynamos, compound wound, and 2,000 incandescent lamps, with all requisite switches and fittings. The whole of the machinery and other apparatus was to be of the latest pattern and best workmanship.

As usual in such cases, the buildings were ultimately extended considerably beyond the limits originally laid down. The lighting plant had, of course, to be increased proportionately, so that the apparatus actually supplied by the Australasian Company consisted of, as under:—

40 Brush arc dynamos, known as No. 7*l*, each capable of giving a current of 10 amperes to from 23 to 25 arc lamps in series.

1 Brush arc dynamo, known as No. 8*l*, professedly capable of giving current to 55 arc lamps in series.

1 Brush arc dynamo with cast-iron armature, known as No. 6, supplying current of 20 amperes for 5 arc lamps.

7 Brush Victoria dynamos for incandescence lighting, compound wound for 110 volts at terminals.

937 sixteen-hour round-type Brush arc lamps, with alabastrine globes; 5 Brockie-Pell 20-ampere arc lamps for lighting the dome; 2 Castle arc lamps.

1,900 Swan lamps of 16 candle-power at 100 volts; 27 "sunbeam" lamps, of sizes ranging from 200 to 600 candle-power each at 100 volts.

All necessary switches, fuses, and leads, the total length of the latter being over 60 miles.

The 7*l* Brush arc dynamos were of two patterns. 1st, that in which the bed-plate and field-magnet yokes are cast in one piece; the field-magnet cores, with pole-pieces, being, as usual in this form of machine, of cast iron, fastened to the yokes with bolts. The commutator brushes and terminals are mounted on a slab of slate, which is fitted into a recess left for it in the bed-plate casting. The other pattern of arc machine has what may be termed a composite bed-plate, or no bed-plate at all. In these machines the two field-magnet yokes are cast in separate pieces, the cores, with pole-pieces, being, as in the former pattern of machine, bolted to the yokes. The two horse-shoe magnets thus formed are connected by two pieces of railway iron, which constitute whatever of bed-plate the machines have. The commutator brushes and terminals are mounted on a slab of wood fitted between the longitudinal pieces of railway iron. The first class of machine will hereinafter be called "solid-"frame," the latter "rail-pattern" machines.

The majority of the solid-frame machines had previously been used at the Adelaide Exhibition, but were fitted with new laminated armatures for usé in Melbourne. The balance were sent out direct from England. The framework of the 8*l* dynamo was imported, but the laminated armature was made and all coils were wound in the Australasian Electric Company's workshops, Melbourne. This dynamo was of the solid-frame type. The No. 6 dynamo was also of the solid-frame type, but had a cast-iron armature. Its coils were arranged for a current of 20 amperes.

Two types of laminated armature were supplied with the No. 7*l* dynamos—one having plates ·05 of an inch in thickness, the plates of the other being only ·022 of an inch thick. The former I will call thick, and the latter thin plate armatures.

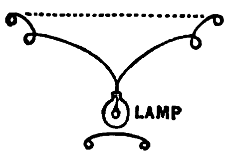
The dynamos for incandescence lighting, seven in number, were of the Brush Victoria pattern known as No. E2. The nominal output is stated to be 366 64-watt lamps at an average armature speed of 720 revolutions per minute. The machines were of the newest type, and had very massive magnetic circuits throughout. The armatures are wound with only three layers of rect-

angular wire, each section consisting of three convolutions. The laminated iron core in the V spaces between the windings is not covered with insulation, so that in these parts it is directly exposed to the cooling influence of the air.

The arc lamps were of the well-known Brush 16-hour round type, wound with the low-resistance shunt recently adopted by the makers. Alabastrine globes were used.

In the main annexes the arc lamps were arranged in a double row down each bay, one lamp being allowed for about every 1,200 square feet of floor space. The bays were about 50 feet wide, and the distance between the two rows of lamps was about 25 feet, the height of the globe from the floor being about 22 feet.

Incandescent lamps of 16 candle-power at 100 volts, to the number of about 1,900, were used for illuminating the picture galleries, beneath the galleries, in the aquarium, and in the cellars. The concert hall was lighted by 25 "sunbeam" lamps of from 200 to 600 nominal candle-power. The incandescent lamps in the picture galleries were set in a single row, 9 inches apart, on reflecting frames, the reflecting surfaces of which were painted a dead white. These frames were about 18 inches wide, made of sheet metal, with a cross section as shown. Below the lamp another reflector was placed, the primary object of which was to shade the eye from the direct glare of the lamps and keep the floor comparatively dark.




With the above arrangement of reflecting frames the walls were the only portions of the galleries illuminated by the direct light from the lamps, and the total absence of glare allowed of a very pleasant rest for the eyes.

The switch-board for the arc circuits consisted of two parallel rails of wood on which ordinary double brass binding screw terminals were fixed. The screws on the lower rail were connected to the dynamo terminals, while those on the upper rail formed the terminals of the various lamp circuits. The positive terminals of all machines and circuits were at one end of their respective rails, and the negative terminals at the other. Th

connection between the dynamo and circuit terminals was effected by means of short lengths of well-insulated cable.

The incandescence switch-board was mounted on a well-finished cedar stand. It consisted of two large slabs of black enamelled slate, on which the nickelled switches and terminals were fastened. Between the two slabs of slate a suitable recess was formed in the woodwork for the reception of a Cardew voltmeter (Goolden & Trotter's make). In the centre of a cedar panel below the voltmeter a small switch was placed, by which any one of the machines could be connected to the voltmeter by turning a handle. Suitable terminals were provided for the introduction of safety fuses, and also for enabling the machines to be coupled in parallel. When so coupled any one or more machines or circuits could be switched on or off without affecting the rest.

The arc circuits mainly consisted of 7/16s stranded cable nigrite and tape insulated, all other being of the same size of cable, but insulated with rubber and braided.

The leads were so arranged that between every two lamps getting current from the same dynamo, there were at least two lamps getting their current from some other dynamos; my object being to make certain that the building should be fairly well lighted, even if several dynamos broke down at the same time. All leads and returns ran in one direction from the dynamo room to the main buildings. But on entering these buildings the leads and returns were taken in totally different directions to the terminal lamps of their respective circuits. By this means, and careful attention to the arrangement of the circuits, the leads and returns never approached each other, except in the dynamo room. Consequently, short-circuiting in the buildings was almost impossible. The lamp circuits were run on the woodwork which carried the roof. The lamps were suspended by cords so that they could be lowered for carboning or other purpose. The wires from the lamp terminals were taken up the suspending cord and lashed to it at a point a few inches from the pulley when the lamp was at its proper height. They were then taken to the woodwork and fastened to it at such a height that the length of wire forming the cord, together with that from the point of

lashing to the suspending rope, was at least equal to the height above the floor at which the wires were fastened. This method permitted the lamps to be lowered to the floor, and at the same time left no cable hanging in festoons about the building when the lamps were in their lighting position. From the terminal lamp of each circuit the wires were taken down the wooden pillars which supported the roof, through the floor, and (bracketed to the flooring joists) to the dynamo room.

The incandescence circuits were arranged for a loss of potential of 10 volts. The installation was looked upon as being of a temporary character, consequently the electrical loss was considered to be of less moment than incurring a larger first outlay for heavier leads. Even with the above-named loss the leads for a portion of the distance consisted of two cables, each containing 37/14s; and for the remainder of the distance to the point of distribution one cable of the same size was used. The distribution was effected by means of 7/16s cable, and finally by single wire. All cables were insulated to 150 megohms per mile.

The steam was generated in 12 multitubular boilers, the pressure being maintained at 120 lbs. per square inch.

Three pairs of high-pressure engines were used, having cylinders 20 inches in diameter, with a 40-inch stroke. Normal speed, 80 revolutions per minute; and the total indicated horse-power with full working load was about 1,580. These engines were made in Melbourne, and were similar to those used for working the cable street tramways. I may say here that, recognising the very great importance of having the very best driving machinery, I urged the Exhibition Commissioners to send to England for engines specially constructed for electric light work. It was found, however, that time would not permit of this, and an order was given to a firm of local manufacturers who had engines partly made, and could guarantee, therefore, to have them ready in time.

The countershaft was in one continuous line, and measured 202 feet 6 inches over all. It was driven by ropes from the engine fly-wheels, in the face of each of which 13 grooves were formed for the reception of the ropes. The shaft was in three

equal sections, one for each pair of engines. These sections were connected by movable clutches, so that they could run either separately or together as required. To reduce to the lowest degree possible the inconvenience which would result if a pair of engines broke down, the dynamos driven by each section supplied current to lamps distributed over the whole buildings; so that if one-third of the light was taken off, no part of the Exhibition would be left in darkness.

The main body of the shaft was $6\frac{1}{4}$ inches in diameter, but the size was increased to $7\frac{1}{2}$ inches where the three rope drums were fitted. The normal speed of the countershaft was 190 revolutions per minute.

The dynamos were arranged in a double row on each side of the countershaft; the distances between the centre of the countershaft and spindles of dynamos being 28 feet and 33 feet respectively.

At one end of the countershaft an automatic speed-recorder was placed. This recorder was essentially a chronograph, and consisted of a drum on which a sheet of paper could be fastened; the drum was driven by a tangent screw working in a worm wheel, which latter could be made fast to or released from the drum spindle by means of a friction clutch. The tangent screw was connected by a flexible coupling to the end of the countershaft. A small table was arranged having a motion parallel to the drum axis. On the table an electro-magnet was fixed, the movable armature of which carried a small glass pen. The electro-magnet was put in circuit with a battery and clock having a minute contact. As the drum was mechanically driven (without slip) from the countershaft, its circumferential velocity was proportional to the speed of the shaft; and as the clock marked equal intervals of time on the paper, the distances between these marks gave the number of revolutions of the countershaft when measured on a suitable scale. The dimensions were adjusted so that the drum made one complete revolution in five minutes when the countershaft was running at the normal speed. Hence, if the speed was normal, and without variation, the marks made by the clock would be in a straight line across the paper. If the speed

did not vary, but was either greater or less than the normal, the clock marks would still be in straight lines, though making different angles with the edge of the sheet of paper. An irregular speed would show an irregular line. (Plate I., Figs. 4 and 5.)

Geared from the same spindle that drove the reading drum, and mounted on the same base, was a small centrifugal governor. The opening or closing of the balls brought a small contact lever against either one or the other of two contact stops. Two incandescent lamps, one plain, the other coloured red, were hung over the engines, the position of the contact lever determining which, if either, of the lamps should be alight. At the normal speed neither would show light, but a variation of about half per cent. on either side would place one or other of the lamps in circuit. The majority of the diagrams taken by the recorder showed a gradual increase in the speed of the countershaft as the evening advanced. This fact will be referred to later on.

Generally speaking, the lighting throughout the term of the Exhibition was satisfactory. On three or four occasions only did anything occur which would lead the public to suspect that matters were not exactly as they should be. This comparative freedom from inconvenience through irregularities in the lighting was the result of the arrangement of the circuits rather than the constancy of supply to any one circuit.

The boilers, which were designed by a Victorian railway engineer, and built in Melbourne, behaved admirably, though working considerably beyond their proper capacity. The steam pressure was kept extremely regular, the maximum variation seldom exceeding two pounds per square inch for the whole evening's run. The fuel was coke, with a little coal added at times when difficulty was experienced in maintaining the full working pressure required.

Notwithstanding the very regular steam pressure the speed of the engines varied considerably, even though they were fitted with mechanism outwardly resembling Hartnel's automatic expansion gear. With the full load on the countershaft, and the engines indicating about 1,500 horse-power, a single dynamo thrown off or on produced a very sensible alteration in the speed

of the rest of the machinery—sufficient, indeed, to necessitate the adjustment of the rheostat controlling the potential difference at the incandescence mains, and to show itself at the speed-indicating lamps.

One evening some trials were made to ascertain the effect of an alteration in the steam pressure on the speed of the engines. For this purpose the pressure was altered one or two pounds every half-hour; and it was found that a variation in pressure of one pound per square inch produced an alteration in the speed of the engines of about a revolution per minute, all other conditions remaining the same. The cause of trouble was clearly enough in the governor, and I experienced a great deal of anxiety and annoyance from a fault which should not have existed. The fact is, the makers of the engines could not be made to understand the immense importance in electric lighting of absolute steadiness in the running of the engines, or that there was any difference in the driving of dynamo machines and street tramways.

The Brush Victoria dynamos ran very well through the whole six months the Exhibition was open. In the main they kept cool, though occasionally the thrust bearings showed an inclination to heat. The commutators ran with very little sparking when the brushes were properly adjusted.

Unfortunately, the above satisfactory remarks cannot be repeated with reference to the arc dynamos. In the first place, the insulation in many of them was defective or insufficient. Consequently, "electrical fireworks" formed a not unimportant part of the evening's proceedings, till such time as the faulty armatures and field coils had either been rewound or weeded out. In many cases, the machines ran for several weeks without giving any indications of weakness, when suddenly a breakdown in the insulation would take place. Another fault, more serious (if possible) than the last, existed in some of the armatures, namely, those which have been designated "thin-plate armatures." All the laminated armatures supplied, both thick and thin plate, were built up of the so-called H pieces, bound in with a strip of sheet iron, equal in width to the intended thickness of the bobbin core. There was no indication of the H pieces having been passed

between rollers, or anything done to give them the same curvature as the armature. The horns of the H pieces were practically flat, hence their resistance to bending was simply that due to the thickness of the metal, which is insufficient in the case of the thin-plate armatures. A few weeks after the opening of the Exhibition several of the thin-plate armatures threw out plates. Two of these plates struck a door-post, and gave unmistakable evidence of the velocity with which they were thrown. On subsequent occasions the plates were not so neatly thrown out, but were caught by the field magnets, and cut the insulation of the armature coils. By the time the Exhibition closed a goodly number of plates had been thrown out. To prevent accidents perforated zinc screens were placed over the armatures.

The cause of these plates breaking off was evidently the continual bending to which they were subjected in passing through the different parts of the magnetic field in which they rotated. The force due to the rotation of the armature tends to bend the plates outwards, while that due to the attraction of the pole-pieces has the opposite tendency (at least with reference to the outer plates). Clearly, then, though the force due to rotation is constant, as the plates pass through a magnetic field which is not uniform in intensity or direction with reference to any given armature radius, the force tending to bend the plates inwards must vary in different parts of the field, and a constant bending of the plates must result. This mechanical fault was a very serious one, and might have caused damage to life and limb.

Before describing the methods used and tests made with the dynamo machines, it may not be out of place to give a few of the reasons which led to the selection of the method adopted.

In order that the tests should be equally satisfactory to all concerned, it was essential that the machines should be tested under the actual working conditions, or while they were at work on their respective circuits. It was not thought to be important that all measurements taken should be of the highest degree of accuracy. As practical engineers know, the conditions of running do not remain constant (especially in arc machines) for many consecutive moments, and the fluctuations which take place affect

all data, and the resulting efficiency from instant to instant. Hence the desire was to make tests which would be of practical rather than theoretical value.

The many more recently suggested methods of testing dynamos in which all the measurements taken are electrical, such as Cardew's and Swinburne's, or in which the mechanical is the smaller part, such as Dr. Hopkinson's, were carefully considered with reference to the special form of machine to be tested. Of these methods, that of Captain Cardew seemed to be the only one that would give satisfactory results, for by it the machines can be tested under the actual conditions of working.

Now that it is no longer thought that a dynamo is simply a generator of electricity, and as such may be used indiscriminately for any purpose to which electricity is applied, makers designing special forms of machine to meet the different requirements for which they are to be used, the importance of considering the behaviour of the machine when working under the exact conditions for which it was designed cannot be over-estimated.

In considering the matter, the many advantages, from a mathematical standpoint, of selecting one of the above-mentioned methods, or of running the machine on a closed metallic circuit, could not be overlooked. How much more satisfactory it would be to have the current almost absolutely steady, without those fluctuations caused by a faulty lamp or a bad carbon, which make the readings of the instrument used in testing so irregular, and so difficult to obtain with any degree of accuracy! But the machines were not made to run on closed metallic circuit, and, if they are to be tested fairly, must be tested with all the fluctuations of current, friction caused by pull of belt on bearings, and other modifying influences which exist under the normal ondition of working.

In some classes of machine, where the commutator has many segments, and each section of the armature winding has but few convolutions, especially if the machine is intended to light incandescent lamps or charge secondary batteries, the chances of error in any of the above methods would be a minimum. But with machines like the Brush arc dynamos, where the commu-

tator segments are few, and the number of windings per armature coil very great, the errors introduced by running the machine on closed metallic circuit instead of the ordinary lamp circuit for which it was designed may be considerable, unless the metallic circuit is arranged to have the same self-induction as the lamp circuit. With such machines as the Brush the current should be maintained as nearly as possible at the value for which the machine was designed, as small alterations in the current may produce sensible alterations in the behaviour of the machine. The tests should be made when the machines have attained their state of steady temperature. It is, of course, needless to insist that the machine shall on each and every day reach its state of steady temperature, as the atmospheric temperature may vary considerably. It is sufficient if the machine attains a temperature which would be its steady state with some known atmospheric temperature within the range of practical work.

If Dr. Hopkinson's method, where two similar machines have their armatures rigidly connected, were used, it would be needful to consider the exact relative angular position of the corresponding coils in the two armatures. If this were not attended to, the armatures might be so connected that the periodic fluctuations of the current would be either increased or diminished by the combination of the machines; hence the conditions of practical work might not be attained. In any case, with the two machines directly coupled (electrically), as suggested either by Hopkinson or Swinburne, the modifying effect of the self-induction of the circuit on the current fluctuation would be lost.

For these and other reasons the elegant methods adopted by Hopkinson and Swinburne were deemed unsuitable in this particular case. Cardew's method was also rejected as being unwieldy, necessitating as it does the removal of the machines, and so many belt transmissions.

As no other purely electrical method was thought of, it was determined to adopt the method involving the use of a mechanical dynamometer for measuring the energy absorbed by the machine, and to measure the output by the ordinary electrical method.

The chief disadvantage of this plan appeared to be in the

necessity of having all the instruments used accurately calibrated. The efficiency would not be the ratio of two sets of measurements taken by the same instrument, or even by two different sets of instruments which could be directly compared *inter se*. But as there is no great difficulty in the calibration of electrical instruments, at least within a degree of accuracy sufficient for all practical purposes, and the mechanical dynamometer can be directly tested, the increased facilities for testing the machines whilst actually working on their respective circuits was considered sufficient to outweigh the disadvantages of the method.

In all efficiency tests but one, Siemens electro-dynamometers were used to measure the current. For some other tests Ayrton and Perry's meters, of both spring and magnet forms, were used. The values of the readings of these instruments were carefully determined by comparison with the electro-dynamometers. The latter instrument was calibrated by connecting it in series with a battery, an adjustable resistance, and a fixed resistance consisting of a large number of small German silver wires arranged so that any number could be put in parallel. The massive terminals to which the small German silver wires were fastened were connected through a very sensitive reflecting galvanometer and a standard Clark cell. A key was, of course, placed in each circuit. With a given number of German silver wires in parallel, the current through the total circuit was adjusted by means of the variable resistance till the potential difference at the terminals of the German silver wires was equal to the electro-motive force of the standard cell. The exact resistance of the German silver wires was immediately measured by the aid of one of Elliot's metre bridges. A small standard resistance was constructed from a piece of the same German silver wire as that used in the fixed resistance. The exact value of this coil was found by comparison with one of Elliot's standard B.A. units. All apparatus to be used was placed in position some time before required, to allow the temperature of all parts to become equal. The temperature was taken by a standard thermometer which had been tested at the Kew Observatory. Two standard cells were used—one by L. Clark, Muirhead, & Co.;

the other made up at the time in accordance with Lord Rayleigh's instructions, whose formula for the variation of the electro-motive force of the cell with change of temperature was used. The two cells when tested were found to be almost exactly equal. No difference could be detected when they were compared by the condenser and ballistic galvanometer. But a very small difference did exist, and was shown by the deflection on a sensitive reflecting galvanometer when the cells were connected zinc to zinc, the remaining terminals being connected to the galvanometer.

In all calibrations some measurements were taken with each of the standard cells.

The results of the calibration tests of the Siemens electro-dynamometer gave the value of the constant = ·8716. The constant as given by the maker was ·87—a difference of less than one-fifth per cent. The same electro-dynamometer when compared with one of Ayrton and Perry's latest spring ammeters differed by very little more than ·01 per cent.

Two voltmeters were used—one of Ayrton and Perry's spring form, the other of the electro-dynamometer pattern. Both of these instruments were wound with copper wire, and large resistance coils of German silver wire, differentially wound, were used to reduce the potential difference at the terminals of the instrument proper. The voltmeters were calibrated in exactly the same manner as the current-meters. They were also checked by measuring the potential difference at the terminals of a known resistance by means of the condenser and ballistic galvanometer. These two methods gave exactly the same results in several tests, and the condenser method was ultimately discontinued, owing to the difficulty of obtaining a place where the ballistic galvanometer could be used without interference from locomotives moving in the vicinity. The dynamometer voltmeter was afterwards compared with one of Ayrton and Perry's spring meters: for 100 volts the difference in their readings did not exceed one-tenth of a volt.

The meter bridge was calibrated by Carey Foster's well-known method. An additional correction had to be made for the heating of the gauge wire by conduction from the binding screws.

No special means were previously employed to prevent error fro this source, as it was not anticipated that the effect would b appreciable.

The selection of a suitable mechanical dynamometer was n easy. The following conditions were laid down as essential:—

1st. It must be susceptible of a fair degree of accuracy, a show instantly any variation in the load.

2nd. It should be of such a form that errors are not likely arise from the difficulty (owing to the thickness of th belt) of accurately determining the virtual radius the pulley; and no power should be absorbed betwee the point at which the pressure is measured and th dynamo spindle.

3rd. Any springs contained must be so placed that centri fugal force will not introduce uncertain effects on th indications of the instrument.

4th. It must be portable, and easily fitted up.

The first condition simply requires that whatever the principl involved it shall be faithfully executed, and that the moment of inertia and number of moving parts shall be as small as possible. The second condition requires that the power shall be transmitted to the spindle of the dynamo at some fixed distance from its axis of rotation, and that it is the pressure at this point which is shown on the indicating mechanism.

The simplest way of fulfilling the above conditions seemed to be to give the dynamometer the form of a pulley to be directly applied to the spindle of the machine tested. It is important that the displacement of any of the parts of the mechanism shall be very small, even though the indicator passes from zero to its maximum reading.

A pulley was accordingly constructed the principle of which is as follows:—

The energy supplied to the machine is transmitted through a fixed point at a distance intermediate between the axis of the spindle and rim of the pulley. By means of levers and pistons the pressure is transmitted to a fluid filling the chamber in which the piston moves, and by it through a glass tube which

lies in the axis of rotation to a spring chamber the capacity of which varies with the pressure on the enclosed fluid. The pressure on the point through which the energy is transmitted is shown by the displacement of the fluid in the glass tube.

The pulley (see diagram, Fig. 6) was made so that the rim,

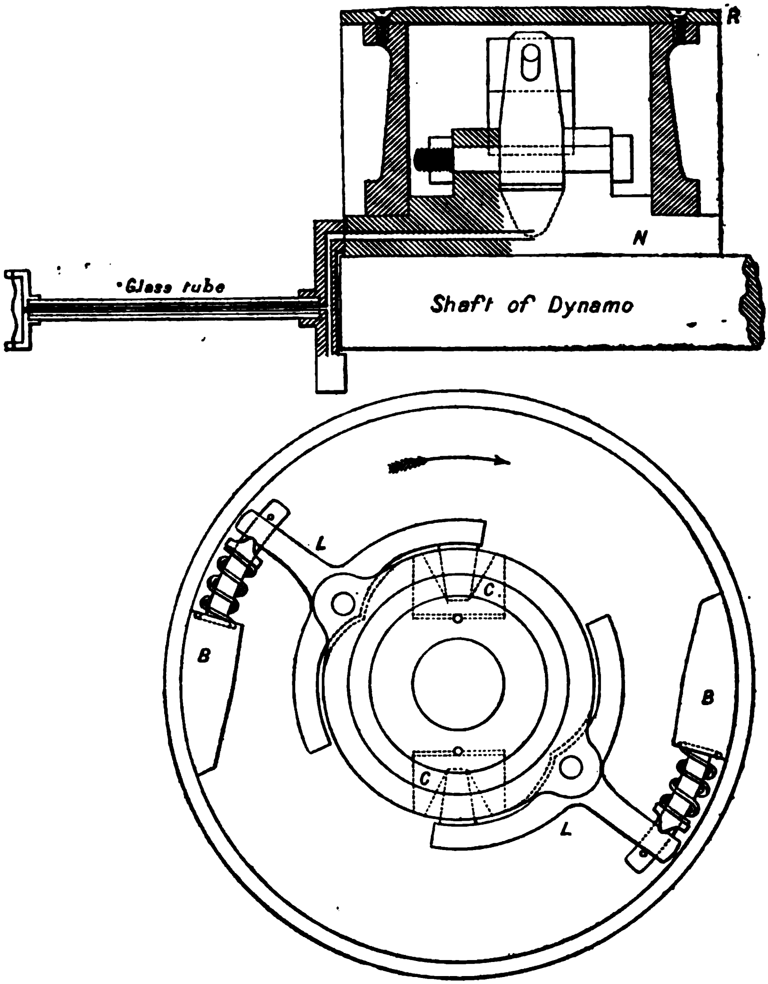



FIG. 6.

R, could turn freely on the nave, N. Two radial chambers, C, were carefully bored in the nave at the opposite ends of a diameter, and pistons were accurately fitted in them, so that

though watertight they could move freely. Two bent levers, L, were hinged at the nave, each being so shaped that when one arm pressed on the centre of the piston, the line passing through this point of contact and the centre of the hinge, was at right angles to the radius drawn through the point of contact. At the same time the other arms of the levers stood in a direction such that the surface of the lever remote from the piston coincided with the radius passing through the centre of the hinge. These arms were of such length that they just cleared the inner surface of the pulley rim. The connection between the rim and nave was made by means of two blocks of iron, B, riveted to the inner surface of the rim in such a position that they would come in contact with the projecting arms of the levers. The point of pressure was rendered definite by the use of knife edges, and any shock was relieved by the interposition of a suitable spring. Two small holes were drilled parallel to the eye of the pulley opening into the cylinders. The indicating apparatus was fixed so that it would coincide with the axis of rotation when the pulley was in use. It consisted of a glass tube, with a carefully fitted ebonite piston which served as a marker. This piston was made about 10 diameters long, and had several small grooves turned in it. The glass tube was mounted with a metallic chamber at each end, one being bolted to the pulley nave, the other closed with a circular corrugated steel spring similar to those used in some steam pressure gauges. The metallic chambers were mechanically connected by an iron tube in which three longitudinal slots were cut to enable the glass tube to be seen. The angular width of each slot was 60°. The scale was fastened on the inner side of the bars of the iron tube; consequently each time a slot turned towards the eye the glass tube and ebonite piston were seen projected on the scale that was fastened to the opposite bar. When in rapid motion the scale appeared as if drawn round the glass tube, and the end of the black ebonite piston formed a well-defined line from which to read. Every tenth division was marked in red, and the fifties were shown by a bright line on the outer surface of the iron tube.

In the pulley used, the diameter of the bore in the glass tube

was about 1-25th of that of either piston. Hence, if both pistons were in use, the motion of the ebonite piston would be about 1,250 times as great as that of either of the main pistons. The length of the scale was about 6·5 inches, so that a motion of the main pistons of 1-200th of an inch would cause the indicator to travel over the whole length of the scale. The pulley was arranged so that one or both pistons could be used. By this means the range of the instrument was doubled. Using one piston and running at 900 revolutions per minute, the maximum reading corresponded to about 30 horse-power, and with both pistons to about double that value.

It will be seen that a dynamometer of this form may be constructed to fulfil fairly well the conditions laid down as essential.

The sensitiveness is determined by the stiffness of the steel spring and length of glass tube used. In practice a very great degree of sensitiveness is not required, as the variations in the power absorbed were found to exceed two, or at times even three per cent. of the total, whereas one-half per cent. could have been easily read. The ammeter showed fluctuations in the current at least equal in relative magnitude to those shown by this mechanical dynamometer.

To calibrate the dynamometer pulley it was keyed on a fixed horizontal spindle. A long balanced arm was bolted to the rim, and at a distance from the centre of the spindle (measured horizontally) equal to $\frac{20}{2\pi}$ feet a screw was put into the lever. Various weights were suspended from the screw, and the corresponding displacement of the ebonite piston noted. It was found that the displacement was directly proportional to the load; hence the scale could be so divided that the readings would show at a glance the tangential pull of the belt in pounds at any given radius. In the pulley used the constant was calculated for a radius of $\frac{10}{2\pi}$ feet. The reading showed at once the tangential pull at this radius, and, multiplying by ten, the number of foot-pounds absorbed per revolution of the armature.



It should be noted here that the levers in the pulley were

counterbalanced for the centrifugal force of the pistons and fluid below them. The correctness of the balancing was tested by mounting the pulley on a spindle and rotating it rapidly in both directions, first of all having removed the rim. If the balancing were correct a small deflection should be produced, its direction depending on the direction of rotation, and the two deflections should be equal at the same speed of rotation. No error was detected owing to the unequal distribution of the fluid pressure on the circular spring caused by centrifugal force. This fact is not surprising when it is remembered that the effective diameter of the spring was only 1·25 inches, and that the fluid (glycerine) used was light.

The dynamometer pulley was placed on the machine which it was desired to test, in the daytime, and fresh glycerine put in. One of the bolts holding the field-magnet core was taken out, and a similar one, but with a hole bored in it to receive the thermometer, put in its place.

The electro-dynamometers were always carefully adjusted for position after all machines were in full work, so that no disturbing effect would be introduced by the action of the current in leads which ran near the testing table. Each reading noted gave the mean position of the indicator for about 15 or 20 seconds in all measurements, whether electrical or mechanical.

The speed of the armatures was determined by one of Harding's counters, in all cases a full minute being taken.

The constants of all electrical instruments were calculated for the mean temperature of the dynamo room—27° centigrade. The temperature correction for the dynamometer pulley was not determined, as only the first or last two or three readings were to be used in the results, and the accuracy of these was ensured by taking the zero of the pulley both before and after the machine was run. The intermediate readings were taken only to show if any serious alterations took place in the general working of the machines.

These tests, the results of which are tabulated (see Table A), were made primarily to ascertain the commercial efficiency of the various arc machines used. By "commercial efficiency" I mean

## A.—EFFICIENCY OF BRUSH ARC DYNAMOS (No. 7L), HOT.

| Description of Machine. | | | Maker's Number. | | Mechanical Data. | | | | Electrical Data. | | | Commercial Efficiency. | |
|---|---|---|---|---|---|---|---|---|---|---|---|---|---|
| | | | Machine. | Armature. | Time of Run. | | Revolutions per Minute. | H.P. absorbed at Pulley. | Current in amperes. | At Terminals. | | | Mean. |
| | | | | | Hrs. | Mins. | | | | P.D. in volts. | E.H.P. | % | % |
| Solid Frame. | Armature Plates. | Thin. | 457 | N.T.A. 149 | 3 | 11 | 930 | 17·64 | 9·396 | 1,149·5 | 14·47 | 82 | 78·7 |
| | | | 924 | N.T.A. 146 | 3 | 48 | 930 | 20·16 | 9·899 | 1,247·7 | 16·55 | 81·9 | |
| | | | 454 | N.T.A. 143 | 3 | 24 | 935 | 21·43 | 9·705 | 1,234·9 | 16·07 | 74·9 | |
| | | | 460 | 157 | 3 | 0 | 930 | 22·89 | 10·1 | 1,220 | 16·52 | 72·17 | |
| | | | 462 | ... | 3 | 0 | 930 | 19·95 | 9·731 | 1,261 | 16·46 | 82·56 | |
| | | Thick. | 467 | N.T.A. 184 | 3 | 35 | 930 | 23·34 | 9 861 | 1,232 | 16·28 | 69 75 | 69·75 |
| Rail Pattern. | Armature Plates. | Thin. | 1846 | 158 | 2 | 33 | 928·6 | 23·18 | 9·988 | 1,292·7 | 17·31 | 74·7 | 77·43 |
| | | | 1840 | 152 | 3 | 20 | 932 | 18·99 | 9·783 | 1,197·4 | 15·74 | 82·8 | |
| | | | 1849 | 161 | 3 | 20 | 930·6 | 24·66 | 10·001 | 1,376·9 | 18·45 | 74·81 | |
| | | Thick. | 1843 | N.T.A. 188 | 3 | 30 | 924·5 | 23·08 | 10·089 | 1,251·6 | 16·92 | 73·31 | 69·84 |
| | | | 1861 | 172 | 3 | 27 | 935 | 18·99 | 9·265 | 1,033 | 12·83 | 67·56 | |
| | | | 1856 | 167 | 3 | 0 | 917 | 26·8 | 10·129 | 1,350·6 | 18·34 | 68·65 | |

the ratio of the electrical energy available at the terminals to the actual energy absorbed at the spindle of the machine.

It will be seen that the current actually supplied by the majority of the machines was a little below the normal, the machines being professedly designed for a current of 10 amperes.

The times of run as shown in the table were not sufficient for the machines to attain their state of steady temperature, though in the majority of cases the temperature reached would be a fair average for ordinary working.

The armatures of the machines were interchangeable, so that any given armature cannot be considered a part of any specified machine; it was important, therefore, to ascertain not only the relative value of the different forms of frame, but also of the different armatures in them.

It will be seen from the table that the different forms of machine having thin-plate armatures are almost equally efficient, the solid-frame pattern having a slight advantage. The difference, however, between the two forms of machine with thick-plate armatures is still less, and the advantage in favour of the rail pattern. But as only one of the solid-frame machines contained a thick-plate armature the comparison is of little value. For practical purposes it may be said that the commercial efficiency of the two forms of machine, provided the armatures are similar, is the same. Greater alterations in the efficiency of either class may be caused by slight alterations in the conditions of working, than the actual difference between the two classes as shown in the table.

The difference in the two types of armatures is very marked. Taking the mean from the two classes of machines, the efficiency of the thin-plate armatures will be found to be about 8·27 per cent. greater than that of the thick-plate armatures. But, unfortunately, the thin-plate armatures were not calculated to withstand the mechanical strains to which they are subjected in ordinary work, so the higher efficiency obtained from them has only a theoretical existence. There is, however, no mechanical reason why the armatures should not be constructed with thin plates, and at the same time be free from the mechanical faults

shown to exist in those under review. Indeed, such armatures have been constructed in the Victorian Railways Telegraph workshops which are quite free from the faults named. One of these has been running for several years without ever giving trouble, and doing excellent work.

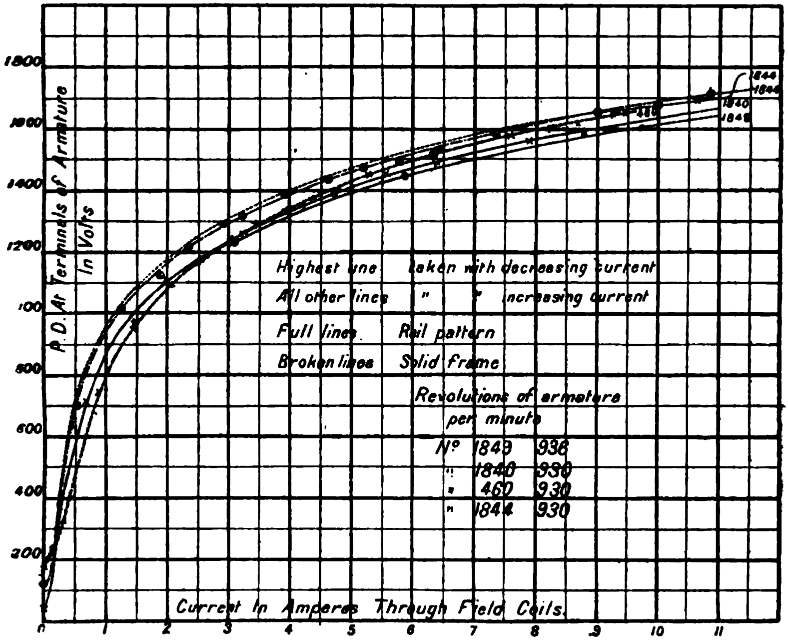



Fig. 1.

The curves in the diagram (Fig. 1) were taken to ascertain the relative value of the magnetic circuits in the two classes of frames. The method adopted was as follows :—

The commutator brush-holders were electrically disconnected from the field-magnet circuit. The ordinary brushes were taken out and replaced by others so bent that contact could be made with the commutator at the neutral point. The armature was run at the normal speed, and the field magnets were separately excited by a variable current taken from an incandescent circuit.

Simultaneous readings were taken of the current through the field coils, the potential difference at the terminals of the armature, and the speed of rotation. From these data curves were plotted as shown in the diagram.

## B.—EFFICIENCY OF BRUSH ARC DYNAMOS (No. 7L), COLD.

| Description of Machine. | | | Maker's Number. | | Mechanical Data. | | | | Electrical Data. | | | Commercial Efficiency. | |
|---|---|---|---|---|---|---|---|---|---|---|---|---|---|
| | | | Machine. | Armature. | Time of Run. Hrs. | Mins. | Revolutions per Minute. | H.P. absorbed at Pulley. | Current in amperes. | At Terminals. P.D. in volts. | At Terminals. E.H.P. | % | Mean. % |
| Solid Frame. | Armature Plates. | Thin. | 457 | N.T.A. 14. | 0 | 37 | 930 | 18·59 | 9·745 | 1,132·6 | 14·79 | 79·6 | 74·29 |
| | | | 924 | N.T.A. 146 | 0 | 30 | 913 | 22·24 | 10·495 | 1,231·9 | 17·32 | 77·8 | |
| | | | 454 | N T.A. 143 | 0 | 10 | 920 | 22·74 | 10·127 | 1,204·2 | 16·34 | 71·85 | |
| | | | 460 | 157 | 0 | 30 | 930 | 22·47 | 9·3 | 1,295·3 | 16 14 | 71·82 | |
| | | | 462 | ... | 0 | 12 | 930 | 24·57 | 10·313 | 1,251 | 17·29 | 70·37 | |
| | | Thick. | 467 | N.T.A. 184 | 0 | 15 | 930 | 25·8 | 10·423 | 1,232 | 17·22 | 66·74 | 66·74 |
| Rail Pattern. | Armature Plates. | Thin. | 1846 | 158 | 0 | 10 | 903 | 24·22 | 10·423 | 1,263·6 | 17·6 | 72·8 | 71·43 |
| | | | 1840 | 152 | 0 | 27 | 914 | 22·26 | 10·202 | 1,168·4 | 15·97 | 71·7 | |
| | | | 1849 | 161 | 0 | 25 | 934 | 25·59 | 10·239 | 1,303·2 | 17·88 | 69·8 | |
| | | Thick. | 1843 | N.T.A. 188 | 0 | 35 | 927 | 23·82 | 10·239 | 1,231·9 | 16·91 | 70·99 | 68·27 |
| | | | 1861 | 172 | 0 | 35 | 920 | 20·51 | 9·428 | 1,036·7 | 13·10 | 63·87 | |
| | | | 1856 | 167 | 0 | 28 | 917 | 26·96 | 10·495 | 1,341 | 18·86 | 69·95 | |

All the machines tested in this manner had similar thin-plate armatures.

The resistance of the voltmeter used was great, so that the potential difference as shown by it was practically the electromotive force of the machine.

As the number of windings and speed of rotation of the armatures were the same, the electro-motive forces would be directly proportional to the total induction through the armatures.

The induction through the armatures depends on the magnetising force (H) of the current in the field coils, and the goodness of the magnetic circuit.

In the machines tested, the number of windings on the field coils were equal. Hence the electro-motive forces of the different machines, when taken with the same current in the field, will represent the relative values of their magnetic circuits.

The curves may be taken either as saturation curves of the total magnetic circuit, or as curves of total induction through the armature, provided no sensible current is taken from it.

An examination of the curves in Diagram I. will show a small magnetic advantage in favour of the solid frames. This result was expected, from the fact that the bed-plate increases the effective cross area of the field-magnet yokes, but is not likely to materially increase the magnetic leakage owing to the great length of circuit.

The early part of curve of dynamo No. 460 was taken with the very greatest care. Only a few of the points are marked for clearness, as the curve passed evenly through them all. The usual concavity at the commencement is shown in a marked degree. The slow rise of this curve for low degrees of excitation seems to indicate a greater degree of hardness of the iron. All the curves approximate to the same form over that portion of their length which corresponds with the ordinary range of excitation.

Table B shows the conditions of running and general behaviour of the machines shortly after starting. Very little more time was allowed before the measurements were taken than was necessary for the lamps to assume a proper working condition.

## C.—ALTERATION IN EFFICIENCY.

| Description of Machine. | | | | Temp. Cent. F.M. Core. | | Current. | | Commercial Efficiency. | | Differences. | | | |
|---|---|---|---|---|---|---|---|---|---|---|---|---|---|
| | | | Maker's Number. | Cold. | Hot. | Cold. | Hot. | Cold. | Hot. | Temperature. | Current. | Efficiency. | Mean. |
| Solid Frame. | Armature Plates. | Thin. | 457 | 30·5° | 60° | 9·745 | 9·396 | 79·6 | 82 | + 29·5° | − ·347 | % + 2·4 | % |
| | | | 924 | ... | ... | 10·491 | 9·899 | 77·8 | 81·9 | ... | − ·595 | + 4·1 | |
| | | | 454 | 29·4° | 71·1° | 10·127 | 9·705 | 71·85 | 74·9 | + 41·7° | − ·422 | + 3·05 | 4·42 |
| | | | 460 | 38·8° | 56·6° | 9·3 | 10·1 | 71·82 | 72·17 | + 17·8° | + ·8 | + ·35 | |
| | | | 462 | 32·2° | 66·6° | 10·313 | 9·731 | 70·37 | 82·56 | + 34·4° | − ·582 | + 12·19 | |
| | | Thick. | 467 | 33·3° | 68·2° | 10·423 | 9·861 | 66·74 | 69·75 | + 34·9° | − ·562 | + 9 01 | 9·01 |
| Rail Pattern. | Armature Plates. | Thin. | 1846 | ... | ... | 10·423 | 9·983 | 72·8 | 74·7 | ... | − ·435 | + 1·9 | |
| | | | 1840 | ... | ... | 10·202 | 9·783 | 71·7 | 82·8 | ... | − ·419 | + 11·1 | 6·0 |
| | | | 1849 | 28·3° | 65·16° | 10·239 | 10·001 | 69·8 | 74·81 | + 36·86° | − ·238 | + 5·01 | |
| | | Thick. | 1843 | 28·8° | 47·1° | 10·239 | 10·089 | 70·99 | 73·31 | + 18·3° | − ·15 | + 2·32 | |
| | | | 1861 | ... | ... | 9 428 | 9·265 | 63·87 | 67·56 | ... | − ·168 | + 3·69 | 2·44 |
| | | | 1856 | 40·5° | 68·8° | 10·495 | 10·129 | 69·95 | 68·65 | + 28 3° | − ·366 | − 1·3 | |

At times, however, the tests had to be postponed till the machinery attained its normal speed. It will be seen that the current in most cases was above 10 amperes.

In Table C the more important values given in Tables A and B are compared, and their differences, with their proper signs, are added, to enable the alterations which take place when the machines and lamps are heated, to be more readily detected.

The current in every case but one, fell as the evening advanced. This reduction is mainly due to the increase in resistance (due to heat) of the shunt circuit in the lamps. The effect of this is to lessen the action of the fine wire in the differential coils and allow the carbons to be separated a little farther; that is, to increase the potential difference required at the terminals of the lamps.

In ten out of twelve of the arc machines tested, the commercial efficiency was found to increase with the reduction of current that took place from the above cause. In the other two machines the current and efficiency rose or fell together. A comparatively small increase of current above the normal is found to produce a sensible diminution in the efficiency. Hence in practical work it is important to ascertain the most efficient current to take from the machines, and see that it is always maintained at that value. The increase in the efficiency may be partially due to a reduction in the losses caused by Foucault currents in the armature and field magnets, brought about by an increase in the specific resistance of the metal by heat. The loss due to magnetic viscosity in the armature core may also be lessened by the same agency.

It will be seen that the average output of the machines is much about the same whether they are hot or cold.

| | | | |
|---|---|---|---|
| Total output of 12 machines, cold | 199·42 | electrical | horse-power. |
| „ „ hot | 195·94 | „ | „ |
| Difference... ... | 3·48 | „ | „ |

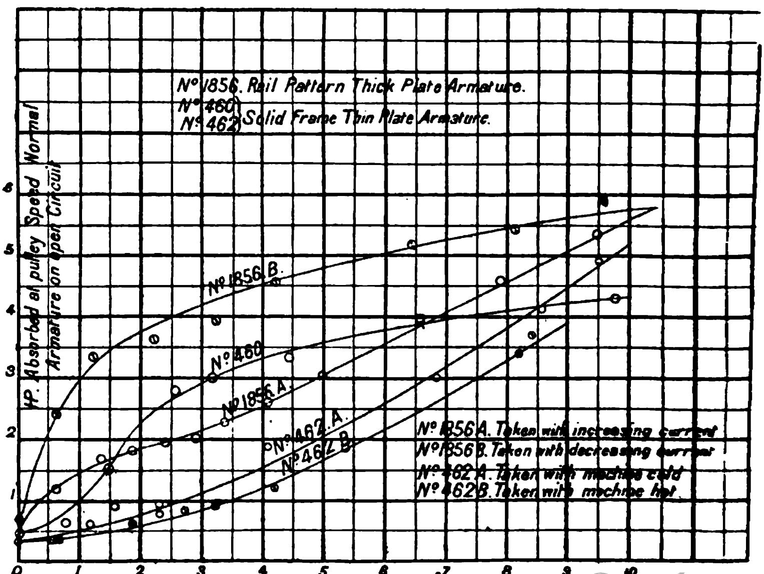
| | | |
|---|---|---|
| Total power absorbed at pulleys by 12 machines, cold | 279·77 | H.P. |
| „ „ „ hot | 261·11 | „ |
| Difference ... ... | 18·66 | „ |

The power absorbed by the machines is much less when they are

hot; the total reduction in the twelve tested being 18·66, or 1·5 horse-power per machine, while the diminution in the output was little more than a quarter horse-power per machine. This fact accounts in some measure for the gradual increase in the speed of the engines (which has been previously mentioned) as the evening advanced, though the steam pressure and number of machines driven remained constant.

As there is no direct method of ascertaining the power absorbed by Foucault currents and magnetic viscosity while the machines are at work (the usual method being to find the value of all directly measurable quantities and subtract their sum from the total power absorbed by the machine, the balance being debited to the above causes), an attempt was made to find approximately the alteration in the magnitude of these losses corresponding with given variations in the total induction through the armature, by the following method:—

The dynamometer pulley was placed on the machine to be tested, the field magnets were arranged so that they could be separately excited by a variable current, and the commuta-




*Current in Amperes Through Field Coils.*

Fig. 2.

tor brushes were removed from the machine. The zero of the pulley was noted, and the machine then run at the normal speed, the deflection produced on the pulley indicating the power absorbed by mechanical friction. The field magnets were then gradually excited and simultaneous deflections on the amperemeter and pulley noted, the speed of rotation of the armature being also taken. From the data thus obtained the curves on the diagram (Fig. 2) were drawn.

It must not be forgotten that the value obtained for the waste power, corresponding with a given degree of field excitation, does not necessarily represent that which actually takes place when the machine is at work with the given degree of field excitation. The modifying influence of the current in the armature cannot well be estimated. The reduction produced by it in the total induction through the armature core would tend to lessen these losses. At the same time the losses in both armature core and pole-pieces would be increased, owing to the forward rotation of the field by the armature current crowding up, as it were, the lines of force into a smaller space, so increasing the electro-motive force of the induced currents, the value of which is always $\frac{dn}{dt}$. Another source of difficulty in the Brush machine is the uncertainty of the effect produced by the great oscillation of the armature field (about 40°) owing to the small number of commutator segments. The curves shown on Diagram II. present some striking peculiarities.

The points from which the several curves start on the vertical line for $C = 0$ may be regarded as their respective origins. The power indicated by the portion of the ordinates below this point is due to mechanical friction, and is constant for all degrees of excitation of the field. The curves Nos. 462A and B were taken with the machine cold and hot respectively. It will be observed that less power is wastefully absorbed when the machine is hot than when cold, and that as the two curves start from the same point the difference is not due to mechanical causes. These curves show in the early part a very slow rise in the waste power for a given increment in the field excitation; but as the excitation is increased, the lost power rises with an increasing rate. On

the same diagram the curves marked Nos. 1856A and B were taken with increasing and decreasing currents in the field coils respectively. By reference to Table C it will be seen that dynamo No. 1856 is one of those in which the efficiency rises and falls with the current in the circuit. The curve taken with an increasing current is the only one of interest. The early portion is peculiar, and I can see no satisfactory explanation of the peculiarity. From the point where the exciting current is 4 amperes the curve is nearly a straight line, which, if produced downwards, would pass through the origin. In this machine, then, the power absorbed, driving the armature (on open circuit) through the magnetic field increases directly as the current at least between the values C = 4 and C = 10. But the power required to overcome mechanical friction is constant. Hence, if the resistance of the circuit remains constant, as it practically does in this case, the efficiency of the machine will rise and fall with the current taken from it. Curve No. 460 was taken from the other machine, in which the efficiency and current rise and fall together. The power absorbed by the armature on open circuit increases at first very rapidly with the current; but when the current has reached 3 amperes the rate of increase is not so great, and it ultimately tends towards a small constant rate of increase.

The curves given in Diagram II. express the horse-power absorbed in driving the armature (on open circuit) through the variable magnetic field as a function of the exciting current. This method of representation is purely arbitrary, though convenient when it is simply desired to ascertain the alteration in the power absorbed for a given change in the exciting current.

The power required to drive the armature on open circuit through a magnetic field clearly will not depend on the value of the exciting current, but on the number of lines of force which the given excitation can force through the magnetic circuit of the machine. Those lines of force only which pass through the armature produce any useful effect. Consequently, if it is desired to ascertain the rate at which the lost power increases with the useful induction, the curves given in Diagram II. should be

expressed as a function of the total induction through the armature.

The curves in Diagram I. may be taken as curves of induction through the armature, expressed as a function of the exciting current.

The curve of induction through the armature of dynamo No. 460 is shown in the diagram (Fig. 3), and is marked No. 460A.

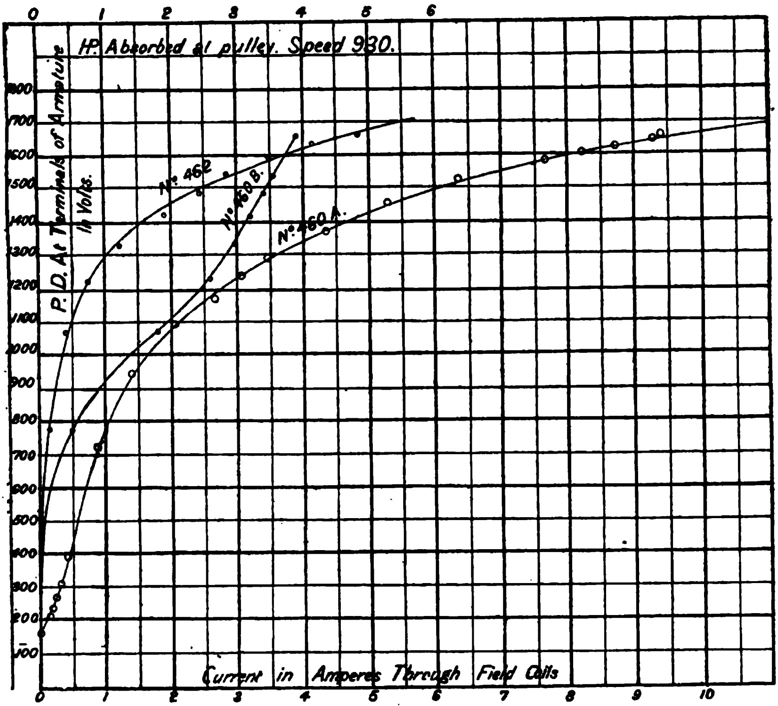



Fig. 3.

The ordinates of curve No. 460B represent the power required to drive the armature (on open circuit) where the induction through it is proportional to the abscissa.

A similar curve from dynamo No. 462, in which the power wasted is also expressed as a function of the induction through the armature, is added for the sake of comparison. The same peculiarities which have been already noticed are now shown in a more marked degree. Curve 460B tends towards a constant ratio between the waste power and induction through the armature.

## D.—DISTRIBUTION OF ENERGY SUPPLIED TO ARC DYNAMOS (No. 7L).

| Maker's Number. | EXPRESSED IN WATTS. | | | | | | | | Remarks. |
|---|---|---|---|---|---|---|---|---|---|
| | Accounted for. | | | | | 6 Absorbed at Pulley. | Not Accounted for. | | |
| | 1 Mechanical Friction. | 2 $C^2 R$. | 3 False Induction, &c. | 4 Available at Terminals. | 5 Total. | | 7 6 – 5. | 8 Per cent. of Total, 6. | |
| 1856 | 338·2 | 1,376·8 | 4,011 | 14,703 | 19,799 | 20,108 | 309 | 1·53 | Machine cold. |
| 460 | 458 | 1,029·2 | 2,720 | 12,046 | 16,253 | 16,765 | 512 | 3·05 | |
| 462 | 230 | 1,223 | 3,796·4 | 12,902 | 18,153 | 18,331 | 178 | 0·97 | |

## E.

| Machine. | | | | | | Armature. | | | | Remarks. |
|---|---|---|---|---|---|---|---|---|---|---|
| 1 Maker's Number. | 2 Area of F.M. Core. | 3 Ampere-Turns, F.M. | 4 H. | 5 B. | 6 $\mu$. | 7 Number. | 8 Area of Core. | 9 Total Induction. | 10 B. | |
| 1844 | 442 | 43,200 | 122·8 | 4,882 | 36·5 | ... | 78 sq. cm. | 2,158,000 | 27,666 | Solid Frame Machine cold. |
| 460 | 442 | 43,200 | 122·8 | 4,825 | 39·3 | 157 | 78 „ | 2,132,600 | 27,841 | Solid Frame |
| 1849 | 442 | 43,200 | 122·8 | 4,647 | 37·8 | 161 | 78 „ | 2,054,000 | 26,333 | Rail Pattern |
| 1840 | 442 | 43,200 | 122·8 | 4,602 | 38·2 | 152 | 78 „ | 2,078,700 | 26,650 | Rail Pattern |

In dynamo No. 462 the waste power increases enormously, with high values of induction through the armature.

The distribution of energy supplied to three of the machines is tabulated on Table D.

The figures given in column 3 are taken from the ordinates corresponding to the given current on Diagram II. They do not necessarily represent the true value of the loss when a current is taken from the armature. The figures given in the other columns are the result of direct measurements while the machines were actually at work.

The data for the curves in Diagram II. were taken before the machines were started on their respective circuits; they would therefore be comparatively cool. For this reason the distribution of the power absorbed by the machines when cold was selected, so that the values from Diagram II. would be taken as nearly as possible under the same conditions.

It will be seen that the difference between the measured total and the sum of the energy absorbed in the circuit and various parts of the machine is very considerable, especially so in the case of dynamo No. 460. In all three cases the power absorbed at the pulley is greater than that which can be accounted for.

Probably this difference is due to an increase in the power absorbed by useless induction when a current is taken from the armature.

In Table E a few approximate data are given with reference to the general condition of the magnetic circuit of these machines.

The values given were obtained with the armature running at the normal speed of 930 revolutions per minute on open circuit, the field magnets being separately excited. In columns 9 and 10 respectively the total induction through the armatures, and corresponding values of B, are given. These and the dimensions constitute the only useful data. In columns 5 and 6 the corresponding values of B and $\mu$ for the field cores are given, assuming no leakage to exist. This, of course, is not the case.

The figures simply indicate a low useful value of B in the field cores.

When the machines are generating a current all the values given will be altered. The actual value of B for the armature of dynamo No. 460 would be 23,028 when the machine is giving a mean current of 10·1 amperes.*

The effect of the fluctuations in the current given by the machines was observed by carefully measuring the current given by the machine and the potential difference at the terminals of the field coils; immediately afterwards the resistance of the field coils was taken.

The product of the measured resistance and current gives the potential difference requisite to maintain a steady current of the given value through the field coils. Subtracting this quantity from the potential difference actually measured, the remainder represents the mean E.M.F. due to self-induction.

Dynamo No. 460, hot.

(1) ... ... ... ... Measured P.D. = 144·04 volts.
C = 11·09 amperes, R = 8·2 $\omega$ ∴ C R = 91·00 „

Difference ... ... 53 „

(2) ... ... ... ... Measured P.D. = 139·0 „
C = 9·82 amperes, R = 8·2 $\omega$ ∴ C R = 80·5 „

Difference ... ... 58·5 „

Dynamo No. 460, cold.

(1) ... ... ... ... Measured P.D. = 140·5 volts.
C = 11·33 amperes, R = 7·06 $\omega$ ∴ C R = 80·0 „

Difference ... ... 60·5 „

(2) ... ... ... ... Measured P.D. = 138·27 „
C = 10·85 amperes, R = 7·06 $\omega$ ∴ C R = 76·6 „

Difference ... ... 61·67 „

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* Armature E.M.F. = terminal P.D. + C R + back E.M.F. in field coils
= 1,220 + 126·25 + 57·2;
Total = 1,403·45 volts.

From these data it appears that the current fluctuates about 1·5 per cent. on either side of the mean, or a total fluctuation of 3 per cent.

If the current were steady the gain in the field coils only would enable the machine to feed one extra lamp with the same armature speed; but the efficiency of the machine would not be much affected.

Of the Victoria Brush dynamos (No. E2 used for incandescence lighting) only four were tested to ascertain their commercial efficiency.

Much more complete details of the behaviour of this class of machine are given in the various journals than can be found of the Brush arc dynamo. For this reason it was not considered worth while to multiply tests of the Victoria Brush machines.

The method of testing was exactly similar to that used for the arc machines, excepting that both pistons in the dynamometer pulley were used, whereas in testing the arc machines only one piston was required.

VICTORIA DYNAMOS, HOT.

| No. of Machine. | Current in amperes. | P.D. at Terminals, in volts. | $\frac{EC}{746}$ | H.P. absorbed at Pulley. | Commercial Efficiency. |
|---|---|---|---|---|---|
| | | | | | Per cent. |
| 1823 ... ... | 192·96 | 108·49 | 26·06 | 36·28 | 77·34 |
| 1822 ... .. | 236·66 | 106·85 | 33·89 | 41·82 | 81·03 |
| 1587 ... ... | 90·013 | 97·66 | 11·78 | 17·71 | 66·52 |
| 1825 ... ... | 246·2 | 110·6 | 36·5 | 42·33 | 86·2 |

The very small current taken from dynamo No. 1587 will account for the low efficiency shown, as the losses are almost independent of the output.

As a whole, the efficiency of these machines is less than was expected.

To conclude. More complete tests, to ascertain the magnitude of the various losses which take place in the machines; the values of H, B, and $\mu$, for both fields and armatures; the exact magnitude and form of the fluctuations in the current from the arc

machines, and its effect in lessening the output from the machines, are much to be desired.

It would also have been instructive to have made a long series of tests obtaining data from which curves similar to those given in Diagrams II. and III. might be drawn, so ascertaining practically the law in accordance with which the power uselessly absorbed varies.

The very considerable differences in the efficiency of the various dynamos, working as nearly as possible under the same external conditions, and having to all appearance similar armatures, is apparent, and the question is, To what are these differences due?

It would seem that they must be caused by differences in construction, by which the deleterious reactions in the machines are altered; but if the tests which I have recorded in this paper could have been extended, and made under various conditions, some light would probably have been thrown on the point. The close agreement of the curves in Diagram I., and the great dissimilarity of those in Diagram II., seem to indicate differences in the quality or arrangement of the iron forming the armature cores.

There were no spare machines at the Exhibition, the whole number obtainable being in constant use; hence the tests made had to be confined to the shortest possible time, so that the lighting of the building should not be interfered with. This is the reason why further tests were not made, and also for the meagre and somewhat unsatisfactory nature of many of the data given.

Such as it is, however, I submit this paper to the members of the Institution of Electrical Engineers with the hope that there may be something in it which will draw forth remarks from some of them whose great experience makes anything they say valuable.

At the Spencer Street Railway Station, Melbourne, I have had for some years a small installation of about 60 Brush arc lamps at work lighting the station yard, and I am now arranging for a central lighting station from which about 800 arc and 4,000

to 5,000 incandescent lamps will be lighted. Thirty of the Brush arc dynamos used at the Exhibition have been purchased and will be utilised, most of them being fitted with new armatures, which I will have made upon lines that I hope, and believe, will prove satisfactory. At any rate, I will avoid the evident errors in construction of the armatures sent out from England with these machines.

If considered of sufficient interest, I will be glad at some future time to give the result of the work done, and also the details of some experiments made with a view to test the propriety of laminating the field-magnet cores as well as the armatures.

TELEGRAPH ENGINEER'S OFFICE,
MELBOURNE, 30*th April*, 1889.

The CHAIRMAN: Is Mr. Mordey here?

Mr. W. M. MORDEY: First I must congratulate the author Mr. Mordey. of this paper on the great care that he has taken to investigate a matter that perhaps has not been carefully studied before out of the works of the manufacturers. There are one or two little temporary faults alluded to—faults discovered by us as soon as by Mr. Murray, and remedied before we knew that they had been discovered in Australia. The throwing out of small pieces of the Pacinotti projections of the Brush armatures was a fault that did not occur for a considerable time in the machines that we made, and was one that might easily escape the attention of the manufacturers, but we took steps as soon as we found it out to remedy it. The actual number of pieces thrown out was quite small; there were many thousands in the machines, and only two or three dozen were thrown out, so that perhaps Mr. Murray has dwelt with unnecessary emphasis on this really unimportant matter.

I am sure I need not say anything more about this cause of complaint Mr. Murray alludes to. Probably most of us know that the same class of machines exactly were used at the Colonial Exhibition for two or three hundred lamps, at Manchester for five or six hundred lamps, at Glasgow for five or six hundred

Mr. Mordey. lamps; and they ran practically without any trouble at all. I think at the Colonial Exhibition we had some of these machines running under a very heavy penalty clause—viz., a fine of 1d. for every minute for every lamp that was out; and out of a possible exceeding 2,000,000 pennies which we might have been fined, we were fined 35s. only, and I believe the fines were strictly enforced.

The paper is a very useful one, but its usefulness might have been still greater if it had covered wider ground. It scarcely fulfils the promise of its title, for it only really deals with one or two matters in connection with the lighting of the Exhibition. The great amount of information which Mr. Murray doubtless possesses as to the fuel consumption, the efficiency of the engines, the cost of labour, and so on, in connection with running a large installation of that sort, would, I am sure, be very useful to many members of this Institution; it is not often that an opportunity occurs of running an installation of a thousand arc lamps for a sufficient time to get the necessary figures as to cost.

Then there was an alternating-current plant in use there, we are told, but nothing more is said about it: many of us would like to have had some useful information about that.

There are a good many facts, tests, and particulars given in the paper which, I think, are possibly not of general interest; but an old question is raised nearly at the end of the paper, where something is said about the discontinuity of the current from the Brush machine. That question has often come up before this Institution. I remember—in 1882 I think it was—Professors Ayrton and Perry described what they called a "discon-"tinuity meter," the object of which was to find out how discontinuous was the current from any dynamo. That discontinuity meter consisted of a sort of Ruhmkorff induction coil, with two windings, one of which was to be in circuit with the machine, and the other was connected to some instrument. When Professors Ayrton and Perry described that apparatus I made some similar tests, and I referred to them before this Society at the time. I took the field magnet of a machine that happened to be wound with two wires, and put one winding in a Brush arc light circuit,

and put a Siemens dynamometer in the other winding, but I could not get any result: the Siemens dynamometer did not take any notice of it. I even put the two wires to my mouth, and could not feel anything at all, so I came to the conclusion that the current was not very discontinuous. Mr. Mordey.

Professor W. E. AYRTON: Was there an iron core in that?

Mr. W. M. MORDEY: An iron core? Yes.

Professor W. E. AYRTON: That makes the difference.

Mr. W. M. MORDEY: Had Professor Ayrton no iron core?

Professor W. E. AYRTON: No.

Mr. W. M. MORDEY: I had one. It was an open magnetic circuit transformer. I always use iron cores in transformers, because I get a better effect in that way.

In 1887, I made another test with the same object, but I made it in rather a different way. I took the armature of a Victoria dynamo and sent the current of a Brush dynamo through it, and took the difference of potential. Then I sent the same current through it from an accumulator, and again took the difference of potential, and got but a very small difference; in fact, the coil seemed to require a higher difference of potential with the accumulator—a difference of 0·5 per cent.—but that was, no doubt, an error in reading. We made another test with transformers quite recently, and, in fact, since I have seen the proof of this paper, that seems to me to settle the point, by taking half a dozen transformers, each made to transform from 2,000 volts to 100 volts, and putting all the 100-volt coils in series and all the 2,000-volt coils in series. I sent the current from a Brush dynamo through all the low-tension coils, and then, with a Cardew voltmeter, attempted to take the difference of potential of the secondary or fine-wire coils; but the Cardew voltmeter showed nothing at all, although an equal alternating current would have given more than 50,000 volts. With the wet hands against the terminals of the whole series no shock whatever could be felt. That was a transformer with an iron core—a laminated iron core. Perhaps Professor Ayrton will say that is the reason why it did not act as a discontinuity meter, but it showed zero discontinuity.

With very small machines I think the discontinuity may be

Mr. Mordey. noticeable: it will be greater than with a large machine running over a long circuit. I have entirely failed to find any sign of discontinuity; but I do not think this discontinuity, even if it existed, would be such a very bad thing. It simply means, if Mr. Murray is right as to the effect amounting to 3 per cent., that an alternating current of 0·3 ampere is laid on the Brush current; and some of us think that an alternating current is not uch a very dreadful thing after all.

At page 742, where Mr. Murray refers to the discontinuity of the current, there is one point as to the method of measurement to which I wish to draw attention. "The effect of the fluctuations in "the current given by the machines," we are told, "was observed "by carefully measuring the current given by the machine and the "potential difference at the terminals of the field coils; immediately "afterwards the resistance of the field coils was taken." Then we have figures given of the tests. The product ($C \times R$) was 91, and the measured potential difference was 144 in one case; and about in the same order all the way through. Then Mr. Murray says: "From "these data it appears that the current fluctuates about 1·5 "per cent. on either side of the mean, or a total fluctuation of 3 "per cent." I do not quite see how that value 1·5 is arrived at, but, in any case, I would like to say that that method certainly cannot be taken as accurately or necessarily giving the discontinuity of the current at all. Suppose, for instance, we had an absolutely smooth current sent through the field magnet of the machine. If the armature were rotated, the effect of the Pacinotti projections would be to make the fields a sort of alternating-current generator, and they would tend to raise the potential difference slightly at the field terminals. Assume, in this supposed case, that the steady electro-motive force propelling the current is such that the current is actually absolutely steady: still the difference of potential at the terminals of the field would be greater than that due to the ohmic resistance to the current flowing through it. I do not know whether I am followed in this, but I wish to point out that the method employed does not necessarily give any measure of the unsteadiness of the current; it may give something—it may give some indication of

an unsteadiness of the magnetic field—but it does not give the unsteadiness of the current in the main circuit. Mr. Mordey.

The Victoria machines are alluded to, and some tests were made which gave efficiencies varying from 77 to 86. Well, Mr. Murray says the efficiency of these machines is less than was expected. I think probably the efficiency was rather higher than appears. You, Sir [*referring to the Chairman*], have made tests of similar machines, and, I think, have obtained a higher result than that. But the efficiency of a machine ought to be taken, of course, at the full load. In this case the highest result was got with the largest load, but the machines were capable of running a good deal higher. They were sent out capable of working really actually higher than what was supposed to be the normal output. I have here a private letter from Mr. Fletcher, the engineer who, under Mr. Murray, carried out the actual work, and I will just quote his words as a proof of the efficiency of the machines. He says: "The Victoria machines worked very "nicely, and were all that could be desired. I had one of them "working for about two months with 30 per cent. above what is "supposed to be its normal output: it ran very nicely, and only "a little warmer than the others." I do not wish to question the accuracy of the test, which, of course, rests upon the accuracy of the dynamometer; but I think that possibly a careful examination of this instrument will show that there were some causes of friction that did not appear, except as reducing the apparent efficiency of the machine.

Professor W. E. AYRTON: It appears to me that if through the primary circuit of an induction coil there be sent a strong steady current on which is superimposed a comparatively small intermittent current, the latter will produce but a small effect in inducing a current in the secondary circuit if the induction coil be furnished with an iron core the magnetic circuit of which is *closed*. In other words, a transformer with a closed magnetic circuit, which is admirably adapted for producing a secondary current by *alternations* in the direction of the primary current, is unsuitable for giving a secondary current by simple variaions in strength, without alteration of direction of the primary Professor Ayrton.

Professor Ayrton.

current. And this was the explanation I gave the late Comte du Moncel, in 1878, when he mentioned to me that he had found that completing the magnetic circuit of an ordinary Ruhmkorff coil actually diminished the effect of the coil. I further pointed out to him that if he combined with his closed magnetic circuit the *reversing* contact-maker that I devised for Ruhmkorff coils when in India in 1870, he would obtain far larger effects than were usually obtained with a Ruhmkorff coil.

Mr. Mather, who made the experiments for Professor Perry and myself in 1882 to which Mr. Mordey referred, has kindly looked up his note-book, and he informs me that the following were the details of the arrangement:—The induction coil, or "discontinuity meter," had in its centre a bundle of soft-iron wires 7⅝ inches long, and the bundle had a diameter of 1 inch. On this were coiled some 10 or 12 layers of double-cotton-covered No. 16 copper wire, bringing the diameter to 3 inches; and over this two separate layers of insulated No. 10 copper wire, their ends being left out so that these two layers could either be joined in series or parallel; the diameter of the finished coil being 3⅝ inches. The current from either a Brush or an Edison machine was sent through the layers of No. 16 wire, which acted as the primary coil, and a Siemens dynamometer was put in the secondary circuit. The Brush machine had eight separate coils on its armature, and the Edison machine many coils, the exact number not being recorded.

The following results were obtained:—

BRUSH MACHINE.

| Revolutions per Minute. | Primary Current, in amperes. | Total Resistance in Secondary Circuit, in ohms. | Coil of Dynamometer Employed. | Secondary Current, in amperes. | REMARKS. |
|---|---|---|---|---|---|
| 434 | 10 | 0·084 | Fine wire | 2·08 | Secondary coils of induction coil in series. |
| 500 | 11 | 0·084 | ,, ,, | 2·2 | ,, ,, ,, |
| 500 | 11 | 0·084 | ,, ,, | 2·2 | ,, ,, ,, |
| 500 about | 11 | 0·054 | ,, ,, | 1·6 | ,, ,, in parallel. |
| 500 ,, | 11 | 0·0126 | Thick ,, | 5·24 | ,, ,, ,, |

Professor Ayrton.

## Edison Machine.

| Revolutions per Minute. | Primary Current, in amperes. | Total Resistance in Secondary Circuit, in ohms. | Coil of Dynamometer Employed. | Secondary Current, in amperes. | Remarks. |
|---|---|---|---|---|---|
| 680 | 10·5 | 0·0126 | Thick wire | 0 | Secondary coils of induction coil in parallel. |
| 680 | 10·5 | 0·054 | Fine „ | 0 | „ „ „ |
| 680 about | 11 | 0·0426 | Thick „ | 0 | „ „ in series. |
| 682 | 11 | 0·084 | Fine „ | 0 | „ „ „ |

There is no record of what kind of resistance was introduced into the primary or dynamo circuit to bring the current to the given value; but, as our object was to ascertain whether there was any difference between the constancy of the currents produced by a Brush and an Edison machine, and not whether the difference, if any, could be wiped out, it is most probable that the resistance employed was a *non-inductive* one.

In this same note-book of Mr. Mather's I find the results of a long series of experiments that we made at that time on the efficiency of a Brush machine when used as a generator and as a motor; and as these experiments, like the ones given above, have, as far as I remember, never been published, it may be useful to give here a short extract from our tests of the Brush dynamo when used as a generator, in order that a comparison may be made between the results now obtained by Mr. Murray and those obtained by us in 1882. It must not, however, be forgotten that the armature of the Brush machines we used was made of malleable cast iron, whereas in the Brush machines tested recently by Mr. Murray the armature core is made of thin soft iron ribbon.

The transmission dynamometer employed was the one devised by us, and which is described in the *Journal of the Society of Telegraph Engineers* in 1881.

Professor Ayrton.

*September* 15*th*, 1882.

BRUSH DYNAMO AS GENERATOR.

| Revolutions per Minute. | Current in amperes. | P.D. in volts. | Horse-Power Developed. | Horse-Power given to Dynamo by Belt. | Efficiency per Cent. |
|---|---|---|---|---|---|
| 722 | 11·3 | 238·3 | 3·61 | 5·37 | 67 |
| 708 | 11·1 | 234·7 | 3·49 | 4·74 | 73 |
| 711 | 11·3 | 227·5 | 3·45 | 4·98 | 69 |
| 729 | 11·3 | 234·7 | 3·56 | 5·07 | 72 |
| 708 | 11·5 | 231·1 | 3·56 | 5·24 | 70 |

[NOTE.—After a reference to his notes, Professor Ayrton has amended his original remarks, and made some valuable additions thereto.—ED.]

Mr. Raworth.

Mr. J. S. RAWORTH: I did not intend to rise at present, Sir, as I thought it would be rather an unfortunate circumstance if the criticism on this paper fell entirely into the hands of representatives of the Brush Company. It is not well for scientific subjects to be treated from a trade point of view; and although we are ready enough to sell the Brush machines, and to convince everybody that they are good ones, I do not think this Institution would be satisfied if we were to speak of the subject of this paper without regard to scientific considerations. I think many of us owe a great deal to Mr. Murray for bringing this subject before the Institution. There is no machine, I think, at present before the public which has been so largely illustrated, or generally described, and, perhaps, so little understood, as the Brush machine. Very few people have had information as to what its efficiency was,—what its capabilities were,—excepting that it burned a considerable number of arc lamps in series, and that it took very little notice whatever of distance. I think Mr. Mordey boasts that, with an ordinary No. 8L Brush dynamo, he could maintain an arc lamp burning in York Station from our works at Lambeth; and I think that another calculation is that a No. 8L machine will work 40 lamps in series, placed at intervals of one mile, on something like a 7/14 wire. The scientific points which are raised in this paper are such that we cannot very well discuss them except generally.

What strikes me as being most peculiar is that the tests of

the various machines differed very considerably indeed—so much so that I think Mr. Murray himself was astonished with the results he got. All that I can say is that the tests we have been in the habit of making at the Brush Company's works have been so uniformly regular, there having been scarcely ever more than a half or one per cent. difference, that finally we ceased to make them. We now make the two tests that are necessary for the manufacture—that is, the insulation test and the general output test: the efficiency tests have been made so often and with such close uniformity that we have ceased to repeat them. Mr. Raworth.

With practical exhibition work I have had a considerable amount of experience—at the Colonial Exhibition, at Manchester, and at the Glasgow Exhibition; and although there were one or two little accidents there, they were clearly traceable, in at least two cases, to leaky roofs. Anybody who has had electrical work to do at an exhibition knows very well that when the dynamos are first brought into the shed the roof is generally in an incomplete state: you have to put up with everything that comes through, be it snow, rain, or hail; and you are very clever indeed if you can prevent some portion reaching the armatures of the dynamos. The Melbourne dynamos—about 40 in number—were made in about three months from the date of order; they were shipped off as rapidly as possible, had a long sea trip, were put to work in the Exhibition, and the lighting was carried out satisfactorily—so much so that Mr. Murray has, I understand, bought the dynamos. I think, therefore, that he has given us very good testimonials; and I have no doubt he will become a constant customer.

With regard to the Victoria machine. Mr. Mordey has pretty fully dealt with that question, and I reminded him when I read the proof of this paper that about the only outside test we have ever had of our dynamos is a very careful one that you, Sir, made some eight months or a year ago, when, if I remember correctly, you put the efficiency at something over 90 per cent.

I do not wish to take up any more of the time of this meeting, as I think there are many members here who may like to ask questions and give us their views; and if there is anything

Mr. Raworth. further the Brush Company can add in the way of explanation upon any general questions, I shall be most happy to reply. Of course, Mr. Murray is not here to explain to us all those little details one would like to know, especially about the dynamometric pulley, which appears to me to be the crucial point. Mr. Mordey remarked this morning, and I think very truly, that Mr. Murray's method of taking the speed was somewhat defective: he took it by a form of continuous recorder. I think the better plan would have been to have used a tachometer, so that the exact speed might have been taken for each measurement made.

I notice in the diagram at the end of the paper giving the full record of speeds, that even in the "fairly steady" diagram there are considerable fluctuations, and I take it that with a small load the fluctuations would have been considerably more than are shown by the experimental records.*

Professor Ayrton. Professor W. E. Ayrton, F.R.S.: In view of the very valuable services that Brush machines have rendered to electric lighting, might I be allowed to say one word more to assure Mr. Raworth and Mr. Mordey that I was not finding fault with the Brush machines? I only spoke as to a matter of fact regarding the measurement of the discontinuity, and I did not express any opinion as to the advantage or disadvantage of this discontinuity. May I suggest a reason for suspecting that the accuracy of Mr. Murray's tests are not as great as the figures in his paper would entitle one to imagine? No one is less likely than myself to find fault with praise of Ayrton and Perry ammeters and voltmeters; but when a person says that an Ayrton and Perry spring ammeter is correct to ·01 per cent.—that is, 1 in 10,000—it shows that the experimenter must possess a microscopic power of reading the ammeter that I do not understand at all, and it shows an amount of accuracy in a commercial instrument which is equally surprising. You must remember that the scale of such an instrument is about 5 or 6 inches long, and to read to a ten-thousandth part of the deflection when the actual deflection

* Some further observations contributed by Mr. Raworth will be found on p. 767.—Ed.

is perhaps only 3 inches means reading that deflection with a microscope; and you know you do not supply with a £4 or £5 electric meter a £20 microscope to read its scale with. Professor Ayrton.

Professor S. P. Thompson: One really ought to make a very clear, fine distinction between machines for arc lighting and machines for incandescent lighting in judging of their merits. They are designed upon different lines; they necessarily have a different commercial efficiency, because the degree of saturation is different. As arc lighting machines are made at the present time—because they are designed with drooping characteristic curves—they cannot possibly show under test as high an efficiency as machines made as incandescent lighting machines are made at the present time. I am leaving alternating-current machines entirely out of comparison, and am dealing only with continuous-current machines of the two kinds. The existing arc lighting machines are, for the most part, of two types, one of which has a very small number of sections on the armature, and with, consequently, a very small number of parts in the commutator. The necessary consequence of that will be that the electro-motive force generated in the revolving armature, assuming the magnetic field to be constant, will necessarily have very considerable fluctuations; but it does not at all follow that the current which results will fluctuate in the same degree as the electro-motive force fluctuates; for everything that there is in the circuit that possesses self-induction—any coil, especially any coil with an iron core, most of all such large coils as one gets in the series windings on the field magnets themselves—everything of that sort in the circuit tends to steady the current. The fluctuations of the current are necessarily, therefore, very much less in percentage value than the fluctuations of the electro-motive force. I do not doubt for a moment that Professor Ayrton six years ago got a very considerable discontinuity in the particular current that he measured, but I doubt whether he was measuring exactly under the circumstances that would be fairly representative of the current in an arc lighting system. I do not know whether the current was running through a series of arc lamps every one of which had an electro-magnet in it, and running round the field Professor Thompson.

Professor Thompson.

magnets of the machine, which would greatly steady the current. And whether the arc lamps were many or few, Professor Ayrton does not say. I am in the habit every day of using a small Brush machine which lights only one arc lamp. I sometimes light an arc lamp, which is on my lecture table for projection purposes, from this Brush machine, sometimes from other dynamos which are intended for incandescent work; and I always know when the Brush machine is on, because I hear a humming: there is sufficient fluctuation to produce a humming sound. But if I had half a dozen arc lamps, or 50 lamps in the circuit, fed by a larger machine with more massive field magnets, I certainly should be very much surprised if I heard the same humming. I do not think that the current from the large Brush machines has anything like the amount of fluctuation—I cannot call it discontinuity—that the current from the small Brush machine has. The machines made on the Thomson-Houston plan ought to show more fluctuation than the machines made on the Brush plan, because they have fewer portions in the commutator and fewer sections on the armature. But in the case of the Thomson-Houston currents I have never heard of any complaint of want of continuity, or of amount of fluctuation, because the field-magnet coils of these machines possess an unusual amount of self-induction. But this I know to be the case in the Thomson-Houston machines: if you separately excite the field magnets, and try to work, say, 40 or 50 arc lamps so that the current goes through nothing but the armature and the arc lamps, that machine refuses to work. It requires the self-induction of the field magnets to be thrown into the main circuit for that machine to do its duty. I should not be surprised if something of the same kind occurred with the Brush machine. I should not be astonished if it showed more fluctuation than under ordinary circumstances.

Is there not some mistake in the relative values of the magnetism and the permeability? I am very much surprised to find there is any kind of iron used in a dynamo which only gives 4,800 lines to the square centimetre when the magnetising force is 122. There would seem to be a mistake of 10 times; that is

to say, I think the number given for H is 10 times too great, and the permeability, which is put at 36, is 10 times too small. Professor Thompson.

Mr. R. L. Cousens: There are one or two remarks I should like to make upon the paper, Mr. Chairman. Mr. Cousens.

I have had considerable Colonial experience with the Brush machine; and although I have not come here for the purpose of praising it, I can say that, so far as my experience has gone, it appears to me, from a Colonial point of view, that it is the best high-tension machine in the market.

There are one or two points which I do not quite agree with in the paper. On page 719 Mr. Murray says that the reason the H plates of the armature broke off was from the continual bending to which they were subjected in passing through the different parts of the magnetic field in which they rotated. For several years past I have had under my supervision Brush arc machines for street lighting, the armatures of which were in 1884 replaced by the laminated thin-plate type, and which were, I believe, the first made to order.

I have never found any portion of these fly out, and in my opinion the faulty ones alluded to by Mr. Murray were not built up sufficiently strong, and the plates would have flown out if the armature was simply rotated, with no magnetic field through the same.

The insulation of the armatures was certainly insufficient, and frequently flames, alluded to by Mr. Murray as "electrical fire-"works," were to be seen issuing from them; but this was remedied by continually improving the insulation whenever any of the machines broke down.

With regard to the apparent decrease in the current as the evening advances, I have found the same to be the case, the precise reason for which I am unable to give, excepting that there is a greater resistance throughout the entire circuit from rise in temperature. From what Mr. Murray states to be the reason—viz., the lessening action of the fine wire in the differential coils of the lamps, and therefore the increase in the length of the arc—he would seem to imply that the lamps did not then regulate so well.

I have found, on the other hand, that the lamps seem to burn

Mr. Cousens. better as the evening advances, and even two or three lamps can be added in a circuit with, say, 16 lamps without practically diminishing the light emitted from these, the revolutions of the machine being kept constant. This is more particularly noticeable on a cold evening than on one when the temperature is high. I have also found it to be the case with Brush lamps of the old form of winding, the shunt resistance of which is considerable, viz., about 350 ohms. This being so, I scarcely think that the reason given by Mr. Murray can be the correct one.

With regard to machines of other types, I have found them to give far more trouble, and to be more liable to be damaged by lightning, than the Brush, although the latter has been by no means infallible. Some series machines for arc lighting of the drum type, with Gramme winding, which were sent out for arc lighting purposes, gave an exceeding amount of trouble from the breaking of the wires round the commutator; and when these portions were thickened, they broke within the coils themselves. This fault, I believe, has been alluded to by Professor S. P. Thompson in his paper on "Diseases of Dynamos," but no reason was assigned for this, nor have I heard of any.*

The armatures of these machines were replaced by others which were wound with stranded wire instead of solid, which to a great extent remedied the defects.

Mr. Esson. Mr. W. B. Esson: Mr. Chairman and gentlemen,—Though I have never made Brush machines, I can appreciate thoroughly this paper of Mr. Murray, which is exceedingly useful and of very great service to us, since it describes the results actually obtained from an installation of considerable magnitude. In all there are about 900 arc lamps, and 2,000 incandescent lamps, the total indicated horse-power being about 1,500.

There are several anomalies, however, in the figures which

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* Since the meeting of the Institution I have consulted with others, and have heard different opinions expressed as to the reason of this fault in this type of machine, as follows:—

(1) The wires break from air resistance when the armature is rotated.

(2) From stray lines of force leaking from pole to pole.

(3) From the shaft spindle being to a certain extent flexible, the wires being therefore strained, and thus frequently breaking.—R. L. C.

Mr. Murray gives. For instance, in Table C, the efficiency of the machine hot is compared with the efficiency of the machine cold; and in one case we find that the rise during the run is from 70·37 per cent. when it is cold to 82·56 per cent. when it is hot—a very great jump. We are quite prepared for a difference of efficiency like from 79·6 to 82, or from 77·8 to 81·9, but the extraordinary difference between 70·37 and 82·56 requires some explanation. Mr. Esson.

It is a pity that when Mr. Murray went to such great trouble in taking the efficiency of these several dynamos, he did not go a little farther and give us simultaneous readings of the indicated horse-power of the engines, and the average electrical power appearing at the terminals of the dynamos. Of course it would be very difficult—one might say impossible—to measure simultaneously the total electrical power being given by 50 dynamos; but we might have been given some rough idea of it, and we might have been told at the same time something about the fuel consumption.

Fortunately, however, electric light engineers are able to state pretty definitely the ratio between the electrical horse-power given at the terminals of their dynamos and the horse-power indicated in the cylinders of their engines. In the present installation, if we assume—and the assumption is rather over the mark than under—that the power actually delivered from the countershaft pulleys is 80 per cent. of the indicated horse-power of the engines, the electrical horse-power furnished by the Brush machines comes out at about 60 per cent. of the indicated horse-power, while for the Victoria machines it comes out at from 67 to 68 per cent. Well, from actual trials which we have made with engines driving Phœnix dynamos through countershafting, without any trimming or extra precaution to ensure the best results, we have found that we get from the terminals of our arc machines 68 per cent. of the horse-power developed in the cylinders of the engine; while with our incandescence machines we get from 72 to 73 per cent. That is, driving from countershafting. Driving with the dynamos belted directly to the fly-wheels, we should get a considerably better result. I hope Mr.

Mr. Esson. Murray will give us the further results which he promises in the last paragraph of his paper. We cannot have too many reliable figures from actual working.

With reference to the general question of constructing arc machines, there seems to be an impression abroad that such machines can only be made in two ways—that is, according to the Brush or Thomson-Houston design. There never was a greater mistake. Speaking from experience, I may say that it is quite easy to construct arc machines giving from 1,000 to 1,500 volts, having ordinary Gramme-wound armatures connected up to Gramme commutators the bars of which are insulated with mica in the usual way. If the machines are properly designed there is not the slightest trouble with the commutators, and past failures have simply resulted from ignorance of the proper methods of construction. Professor S. P. Thompson has just said that in consequence of the commutators having so few sections in the Brush and Thomson-Houston machines, these are necessarily inefficient. Very well; the best way to secure efficiency is to give up those machines having few sections, and to employ only Gramme-wound machines, which have a large number of sections and a high efficiency.

Mr. Kapp. Mr. Gisbert Kapp: Mr. President and gentlemen,—I shall not follow the previous speakers in their attempts to show what efficiency can be obtained with different machines. We all know that every inventor or designer of dynamos thinks his own particular machines the best. I should, however, like to point out that the efficiencies given in the paper, apart from any interest they may have comparatively with that of other machines, are very instructive in themselves. In Table A we find for the solid-frame machine the average efficiency of five tests, 78·7 per cent. with the thin-plate armature. With the thick-plate armature, in the solid-frame machine the efficiency is 69·75 per cent., or a drop of 9 per cent., simply due to the fact that the armature plates are 50 instead of 22 mils. thick. In the other (rail) machine we have a similar ratio—77·43 to 69·84—say about 8 per cent. Now 8 per cent. additional in efficiency is very well

worth having, even at the extra cost and trouble of using thin plates for the armature core. Most dynamo makers have already, by actual experience, come to the conclusion that the iron in the armature cannot be laminated too much. Most makers have begun with rather thick plates—about 16 to the inch—and have gradually come down to something like 40 to the inch. There are, however, several objections to the use of thin plates. They are not only more expensive, but also mechanically weaker, as is shown by the throwing out of the Pacinotti projections; and the amount of space wasted by insulating the plates is greater. Mr. Kapp.

The objection of expense is not a very strong one, for the cost of the material in the armature is not very great in proportion to the cost of the material in the rest of the machine; and to make up against this slight increase of cost, there is the consideration that if we have thin plates we are sure of getting good iron, because bad iron cannot be rolled very thin. The objection as to mechanical weakness I am glad to hear Mr. Mordey say has been got over, but he has not told us how. Then as to insulating space, this can, by varnishing the plates, be reduced to something like 4 or 5 mils. for each alternate, or say 2½ mils. for each plate; so that only 8 per cent. of the whole space need be lost by insulation. Lately some makers have given up insulating the plates altogether.

Regarding the question of the fluctuation of current, there seems to be some diversity of opinion. Professor Ayrton says there is a distinct fluctuation; the amount of it can roughly be tested by an instrument he and Professor Perry had constructed for the purpose. On the other hand, Mr. Mordey has made tests with an instrument supposed to be of the same construction, and found that there was no fluctuation in a Brush current. This result was probably due to complete saturation of the core of his transformer. The theory generally advanced in support of the view that the current from a Brush machine is steady is that the mass of the field magnets is sufficient to smooth down the attempts of the armature to produce fluctuation. The field may be compared to the fly-wheel of a steam engine; and although it is supposed that the fly-wheel makes the motion absolutely uniform, a

Mr. Kapp. moment's consideration will show that it cannot be so. The office of the fly-wheel is to help the engine over the centre. Whatever resistance against turning there is from the belting or other machinery which the engine drives, that resistance has to be overcome by the fly-wheel when the crank is going over the centre. Everybody knows that a body in motion can only overcome resistance by being slowed down, and there must be a certain retardation of angular velocity in order that the fly-wheel may give out work and get the engine over the centre. The action of the field magnets of a Brush dynamo is similar. As long as the current remains of absolutely constant strength the field cannot push it on, and we are therefore forced to the conclusion that there must be a certain fluctuation in order that the field may have a tendency to prevent fluctuation. Since there must be some fluctuation, no matter how heavy the field, it is obvious that the determination of the electrical output by simply multiplying current and pressure is not absolutely correct. Such a determination is in the same manner incorrect as it would be with an alternating current, but not to the same extent. The error thereby introduced may possibly be very slight, but for very accurate measurement it ought to be taken into consideration.

Mr. Addenbrooke. Mr. G. L. Addenbrooke: I may say that I happened to be in Melbourne about three days after the Exhibition closed, and had an opportunity of seeing all the machinery. I did not see it running, but I saw it as it was, and therefore I may perhaps be able to supply from memory a few of the details which some members have asked for.

The number of boilers was 12. They were all made in the Colonies; and I may say that nearly the whole of the boilers in the Colonies are made in the Colonies. The type has no doubt been evolved partially from marine boilers and partially from locomotive boilers. It is a fire-tube boiler of the marine type, only rather different in detail, being much longer, and the tubes are returned as in a marine boiler. The flue is carried back from the front by thin iron coverings riveted down on to the side faces of the boiler. The boilers were mounted on wrought-iron supports, so that they required no brickwork at all: they

were simply stood in their place; and I may say that the boilers and their arrangement made a very neat-looking job, and gave satisfactory results. I did not hear whether any tests of evaporative power or anything like that had been tried with the boilers, but the general tests of such boilers are fully up to the standard. The engines spoken of in the paper were an exact copy of the well-known type of Messrs. Marshall, of Gainsborough. I understand that the measurements were taken from an engine belonging to the Australian Electric Company, the valve gear being carried out on identical principles, which are, I think, well known to most people. Mr. Addenbrooke.

The engines, as far as workmanship went, appeared to me to be well turned out. They were not compound—simply paired—and therefore, of course, not very economical. I had no opportunity of finding out how the valves, &c., worked, but to all appearances they were fair engines, and such as would have been creditable to English manufacture. I may say that I think, on the whole, Colonial workmanship is very good: they know the value of good work.

In the engines there were two cylinders, one on each side, and in the centre a large fly-wheel, boxed in with wooden boxing; it was grooved, as the paper says, for 13 pulleys. These fly-wheels were, I think, 20 feet in diameter, and they weighed 20 tons; they were, of course, cast and made in the Colonies. On the Melbourne cable tramway system 30-ton fly-wheels, nearly 30 feet in diameter, are in use. These large driving wheels drove all the pulleys, which formed the most portentous-looking line of shafting that I ever saw; it was, as stated in the paper, 200 feet long, divided into three parts by pulleys with clutches, so that it could be driven from either engine; and I think that usually two sections were driven from one engine: thus there were something like 140 feet usually driven from one engine. The loose pulleys were on the countershafting, so that the belt was not running at all when the dynamos were thrown out, and of course this necessitated a lot of extra work, and made the countershafting look much heavier. The dynamos were arranged between the engines and the countershafting in the usual way. The whole

Mr. Addenbrooke. was certainly carried out in a ship-shape and nice form, and altogether looked neater than anything we saw at the exhibitions in London. This, however, is no doubt accounted for by all the machines being of the same pattern, and designed to work together.

Unfortunately, as I have said, I did not see the lighting, and therefore cannot say much about it. But from what I heard from outside electrical engineers it was, on the whole, satisfactory. This installation was one of the largest which have been put up. Perhaps you are not all aware of the fact that the area of the covered portion of the Melbourne Exhibition was somewhat larger than any of those lately held in London—the Colonial, Healtheries, and so forth; and, as far as I could see, the thing was as well done as a whole. I do not mean only with regard to electric lighting, but everything else. The lighting of the picture gallery, which Mr. Murray alludes to, was very good. The shades had been nicely arranged so that the lamps just threw the light down on to the edge of the floor below the pictures, and the lamps were not visible to the public. I do not know whether I can give any further details. I may say that there was a transformer plant of Messrs. Ganz working in the Exhibition: it did its work very well during the Exhibition, and was, I believe, afterwards bought by a Melbourne firm for a theatre installation. The Thomson-Houston people also had a transformer plant at work, which also did well.

Of Mr. Murray himself, I may say that he has been, I believe, electric engineer of the Victorian Railways from the first, and telegraphy in Victoria may be said to have risen under his care. He has the electrical connections to look after for something like 2,200 miles of railway; and while he has a reputation for carrying out everything satisfactorily and efficiently, he is at the same time a most impartial man. I am sorry to say that when I last saw him he was in poor health.

Mr. Preece. Mr. W. H. PREECE, F.R.S.: I would like merely to remark, Sir, that Mr. Murray sent his paper over to England to my care, and desired me to look after it for him; and I should have been perfectly prepared to have supported any of his views if they had been severely handled or severely criticised.

I am quite certain that Mr. Murray himself will feel very pleased that his paper has been so well received, and we as an Institution are very much indebted to him. We are, indeed, glad when our members in distant parts supply us with papers of this description, and communicate to us what has been done in other lands. Mr. Preece.

I am sorry Mr. Murray has omitted one important point: there is little or nothing about the behaviour of the arc lamps, and little or nothing about the amount of the illumination. What we really do want to know is the experience of managers of exhibitions, and of the lighting of large spaces, of the amount of illumination that is given by the distribution of arc lamps at various distances apart.

You will pretty well know that I have had a great deal to do with exhibitions—the Inventions, Colindéries, Liverpool, Manchester, and others—and I have always laid it down as a law to totally disregard the nominal light-power given by arcs, or any other indication of candle-power, and have taken only the power consumed in the arc lamp. The power consumed in the Brush arc lamp is 500 watts, and by allowing a watt to 2 square feet of surface area, I have found that you get a brilliant illumination; by giving 3 square feet to each watt you get a little less; and so, by regulating the surface to be illuminated to the number of watts, you are able to regulate your illumination as you please. This rule has been frequently followed, and it is most desirable that we should have information as to the working of some such rule, so that we really all may be guided in the illumination, say, of railway stations. In Paris, where 13 acres were covered with glass and illuminated, I found, in the Galeries des Machines, that the amount allowed was less than that I have named: it was something like 5 square feet per watt; and I think that if those who have charge of arc lights will bear that in mind, and give us some information on that point, they will help us very much.

On behalf of Mr. Murray, I am sure I am only expressing his views when I say that he is extremely indebted to the Institution for receiving his paper so kindly.

Dr. Hopkinson.

The CHAIRMAN: Mr. Raworth, I think, referred to a test I made of the Victoria machine some little time ago. As the results of the test were satisfactory, I think that the Brush Company are entitled to them. I should state the machines were specified to have an efficiency of 92 per cent., and they came so near in the test that I did not feel myself justified in taking the slightest exception to them. Those machines had an efficiency of about 89 per cent. at the maximum load.

On the motion of the CHAIRMAN, a hearty vote of thanks was unanimously accorded to Mr. Murray for his interesting paper.

The meeting then adjourned.

# MELBOURNE EXHIBITION.

## NOTES ON MR. MURRAY'S PAPER.

Dynamos Nos. 454 to 467 were not made by the Brush Company, and as I have no means of ascertaining their exact construction and windings, I shall deal only with the second series, covering dynamos 1846, 1840, and 1849 with *thin* plates, and 1843, 1861, and 1856 with *thick* plates. These dynamos, excepting in the thickness of the armature plates, are known to be practically identical in construction.

*Dynamos Test.*

| | | |
|---|---|---|
| Highest efficiency—thin plates ... | 82·8 | per cent. |
| Lowest " " " ... | 74·7 | " |
| Difference ... | 8·1 | " |
| Highest efficiency—thick plates ... | 73·31 | " |
| Lowest " " " ... | 67·56 | " |
| Difference ... | 5·75 | " |
| Average efficiency—thin plates ... | 77·43 | " |
| " " thick " ... | 69·84 | " |
| Difference ... | 7·59 | " |

The variations in the above results in each set are very remarkable.

In the case of dynamos 1840 and 1849, their efficiencies are given (hot) at 82·8 and 74·81: difference = 9·99 per cent.; but on referring to Fig. 1, we find that the values of the magnetic circuits for similar currents are practically identical. Any actual difference in efficiency must therefore be sought for in the armature. Unfortunately, no experiments bearing on this point in reference to these two dynamos are recorded; but one cannot resist the conclusion that a loss of power in one armature exceeding that in the other by 7·99 per cent., equivalent to about 2 horse-power, would have produced such a marked incease of temperature as to have excited special attention.

One of the most remarkable recorded results of the experiments is the improvement in efficiency as the machine increased

in temperature. This was, of course, to be expected to a slight extent, owing to the increased resistance to the generation of eddy-currents in the iron of the armature, as suggested by the author; but on referring to the tables the figures given appear quite inexplicable.

Referring to Table C, we find the average increase in efficiency, when hot, is given as—

| | | |
|---|---|---|
| Thin plates | ... ... ... | 6·0 per cent. |
| Thick „ | ... ... ... | 2·44 „ |

The maximum increase is given—

No. 1840, *thin plates* ... ... 11·1 per cent.;

whereas there was an actual decrease in the case of

No. 1856, *thick plates*, of ... 1·3 per cent.

Assuming that the difference was due to the variation of the eddy-currents in the armature, one would have expected to find the greatest variation in the thick-plate armatures, in which the eddy-currents would undoubtedly be more serious than in those with thin plates; but the facts as disclosed by the experiments are quite in the contrary direction. The thin plates show, on the average, an improvement of 6 per cent., while the thick plates show only 2·44 per cent.

The maximum improvement in the thin plates was 11·1 per cent., and one of the thick-plate armatures actually deteriorated 1·3 per cent.

It is clear, therefore, that the variation cannot be accounted for in the armature. The only other place where the variation can be looked for is in the tips of the pole-pieces, but the mass of these is so small that it is practically certain that any excessive loss of energy concentrated in them would have raised their temperature within the 30 minutes of the "cold" experiment, and thereby have nullified the variation.

From the last series of experiments with the Brush dynamo of which details are given, Mr. Murray concludes that the current was not steady, and that the total fluctuation was about 3 per cent.; but it is evident that the difference between the observed loss of potential and current-resistance may have been due either

to fluctuation in the current, or fluctuation in the resistance of the magnetic circuit. Mr. Mordey's experiments show that the fluctuation of the current from No. 7L working on a full load of lamps is extremely small, if not entirely inappreciable by any instrument less sensitive than a telephone; but we know, from the construction of the machine, that the resistance of the magnetic circuit is being varied constantly by the revolutions of the armature, and that the distribution of magnetic density in the field magnets varies in a still greater degree. These variations in the field magnets are reflected on the armatures, where they produce opposing forces, so that it does not at all follow that variations in the field produce equal or corresponding variations in current.

*21st Nov.*, 1889. JOHN S. RAWORTH.

The One Hundred and Ninety-eighth Ordinary General Meeting of the Institution was held at the Institution of Civil Engineers, 25, Great George Street, Westminster, on Thursday evening, November 28th, 1889—Sir WILLIAM THOMSON, F.R.S., President, in the Chair.

The minutes of the Ordinary General Meeting held on November 14th, 1889, were read and approved.

The names of candidates for admission into the Institution were announced and ordered to be suspended.

The following transfer was announced as having been approved by the Council:—

From the class of Associates to that of Members—

Clement Joachim.

The following paper was then read:—

## ELECTRICAL ENGINEERING IN AMERICA.

By G. L. ADDENBROOKE, Associate.

The last eighteen months or two years in America have been much more characterised by commercial and industrial progress with already existing types of plant and apparatus, than by a number of new inventions. Inventive ability seems to have been more concentrated on improving and elaborating already existing inventions, than on the evolution of new forms and ideas.

The leading types of machines and apparatus are all well known to you, and have been so fully described and illustrated that to enter into a minute account of them now would only be to traverse old ground. I therefore propose to devote myself this evening to central station, construction, and internal work, and the application of electricity to tramway and other industrial purposes. Finally, I propose to make a few remarks on the tone, policy, and aims of American electrical engineers, and those commercially interested with them in advancing the application of electricity.

My visit to America was not quite of the ordinary character, as I came through on my way from Australia, and consequently had to traverse the whole breadth of the continent before reaching the better known towns on the Eastern Coast. I was thus able to make a general, if rapid, survey of what is going on in the far West and in towns remote from the manufacturing and engineering centres, as well as in New York and some of the larger cities.

### Overhead Work.

Throughout the continent, from shore to shore, the main thing that forces itself on the observer is the prevalence of arc lighting. Hardly any what we should call moderate-sized village outside the oil region appears to be without its arc lamps.

Arc lamps, when used for public lighting, are usually fixed on poles from 20 to 40 feet high, according to the caprice of the parties interested in their erection; or they are slung from the corners of intersecting streets, in which case they usually hang low down; or the tower system is adopted. When posts are used, they are of rough pine, never painted, often out of the straight, and warped or bent. If the lamps are fixed, iron spikes are usually driven at intervals on each side of the pole, starting about 8 feet from the ground, and forming a sort of ladder by which to reach the lamp. During my whole journey from San Francisco to New York, I can hardly recollect seeing one ship-shape, neat, and smart-looking post, whether for carrying arc lamps or for any other electrical purpose. Everything has a temporary and expedient look about it, which is very offensive to English eyes. Creosoting does not seem to be practised, and the only protection applied to the poles which I saw anywhere, consisted of boards fixed round the base and reaching about 6 feet high; but this protection was, I think, only applied to large poles carrying circuits or telephone wires.

The lamps themselves are always entirely without ornamentation, and are usually very roughly fixed on the poles. If the globes are not clear glass they are usually only lightly frosted, so that the light glares in one's eyes in all its nakedness.

When the lamps are hung over the centre of intersecting

streets, four wires are brought down from the corners of the neighbouring houses, the lamp hangs from the centre, and is lowered and raised by a pulley and cord. The wires from which the lamp hangs, the cord, and the leads all festoon about in accordance with the chapter of accidents, and look, as we should think, both slipshod and untidy.

The only relief to this state of things is afforded by the towers. These towers are usually erected at the intersection of streets, with the four legs at the four angles of the pavement. They are constructed of exceedingly light ironwork. Tapering gracefully upwards to a great height, they are at any rate neat and not uninteresting-looking objects in themselves, and certainly form very definite landmarks. Three or four arc lamps hung round the top of one of these towers shed a sufficient light over a great area where the houses are not high. With this method of distribution a 20-light machine can be employed to great effect; especially as in country towns and villages, where these towers are chiefly employed, the object is rather to make darkness generally visible, if I may so express it, than to provide bright illumination in well-defined places.

By using the tower method one's eyes are not dazzled by the brightness of the light, which is so much the case with lamps at a low level.

In my own case I used sometimes at night to get quite an irritated feeling when walking about, if at all tired, from the incessant glaring in one's eyes of naked arc lamps, and the impossibility of finding rest anywhere. I mentioned this effect to two or three Americans, but they did not seem to notice it.

Important, however, as the public lighting is, in the large towns it occupies a comparatively inconspicuous place in comparison with the numbers of lights used for illuminating stores, saloons, hotels, restaurants, and all sorts of public buildings. Arc lamps are so lavishly used in these places, both inside and out, that the principal streets are often literally a blaze of light. There were, as far I could ascertain, about 2,000 arc lamps in nightly use in San Francisco, 3,000 in Chicago, and I understood that 3,500 were shut down in New York while I was

there, owing to the accidents which had taken place, and of which so many accounts have been published.

The majority of the arc lamps used outside are fixed by means of light iron rods projecting from the buildings, though some, of course, are hung.

The circuits feeding lamps hung either inside or outside of a building, either run direct from the nearest poles on each side of it, thus making an angle over and across the pavement which is very unsightly, or a sort of barrel-shaped glass insulator is used. This insulator has a hole through the centre and a groove round the middle of the outside. The wire coming in one direction is passed through the centre of the insulator, turned back on itself, and made off; the wire in the other direction is passed round the outside, and also made off on itself; the two ends of the loop are then carried to the lamp, through which the circuit is completed. Nearly all the wire I saw was insulated with "under-"writers' insulation." Where lamps are used internally, this is often carried through wooden window frames and such other like places without any further protection or care. Once inside, the wire is carried along ceilings and walls quite exposed, but separated about 3 inches by small wooden cleats. Sometimes instead of cleats small white porcelain insulators are used.

As far as I could learn, it is the invariable custom for all companies to run, fix up, and entirely look after the lamps they operate. The customer simply pays a rent for the use of the lighted lamp of so much a week, month, or year, as the case may be, and has no further trouble of any sort. The rates differ according to the time the lamps are to be in use each night. To see that the lamps burn properly, linemen promenade the streets all night, and are supposed to see every lamp on their circuits at certain intervals.

The circuits themselves are almost invariably carried on poles erected along the edge of the kerbstones. A fair estimate of the average height of these posts is, I think, about 30 feet; of course some are much more, and many less. Long wooden cross arms are fixed near the top of the poles, carrying from two to six, and even eight, insulators on each arm. These arms are not so strong as we

should make them for the purpose. Owing to the length of the arms, and to their not being very securely fixed to the poles, the weight of the circuits often pulls them out of the horizontal. To see a warped and bent pole, set crookedly in the ground, and with the arms at various angles with each other, and the wires on it all hanging in different curves, seems about as dismal and woe-begone a piece of engineering as can well be imagined.

I must make some exception, however, in the case of the Thomson-Houston Company in New York, who, at any rate, had got light iron stays attached to the poles and arms, at an angle so as to give more support to the arms and keep them in their places.

The insulators are supported on the arms by wooden pegs, which have a screw-thread cut on the part that projects. On this one of the ordinary green glass insulators is screwed. Green glass insulators are used for every purpose in America, whether for electric light, telegraphs, or telephones. They are very cheap, and answer the purpose well. Still, the effect of green glass insulators on arms painted a dull red, fixed on bare poles from which the bark has just been stripped, is not æsthetic.

The majority of the poles for circuits have no steps provided for ascending them. On the frequent occasions when it is necessary to go up to the top to make alterations or add new circuits, the linemen use climbing irons. The tearing of the wood by the spikes of these climbing irons gives the poles a jagged and frayed-out appearance from top to bottom. One could fancy they lead to the habitation of some immense squirrel.

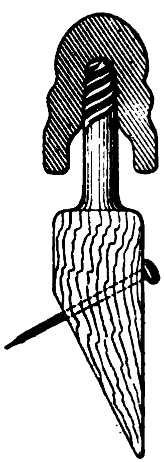
The Americans nearly always use solid wire for running their circuits, covered, as I have mentioned already, with "underwriters' "insulation." Little trouble is taken to keep the various circuits on one pole in regulation with each other, as long as they do not touch. Indeed, the whole construction of the poles and arms is so flimsy that no great strain could be put on the wires. The poles themselves are fixed at short intervals, often not more than 30 yards apart, and seldom above 60.

The circuits are rather hung up on the poles than run, in

the sense an English lineman attaches to the word. Separate suspenders I never saw used, except for telephone cables. For alternating incandescent circuits of 1,000 volts several separate cables of small size are also used, instead of running a large one with suspenders, as has been the custom here.

What I have just said about short spans for electric light work also applies to telephone and telegraphic work: the spans used in this work are generally also short. In the latter case, since the wires are light, they are run pretty taut without putting much strain on the arms, so that there is not much sag. Wires run in this way are of course not so liable to sway about as long spans are in high winds, and make intermittent contact or get overlapped. Further, the wires, being run along the streets, are protected from high winds in a great measure, and are not particularly exposed to them, as our over-house work is. This method of erection, therefore, rough as it is, and notwithstanding the enormous number of wires which are often suspended from one pole, gives an immunity from contacts and accidents that an English engineer at first can hardly understand or appreciate.

For leading in wires into houses, or for putting on extra wires on a pole—in fact, for general purposes of the sort—a block of wood is used the lower end of which is cut off at an acute angle, the upper being cut away and turned down, and a screw-thread cut on it to carry an insulator. When the face of the angle is fixed against the side of a house or a pole, and a spike or screw put through it, the insulator projects somewhat, and forms a support for making off the wire. The whole thing is of the cheapest description but, nevertheless, seems to answer its purpose.



Wires are led in from the outside with much less care than is generally used in England; and this applies also to telephone and telegraph work. It often puzzled me how they worked at all after they had been put up a few months. But the fact that the climate is much drier, and fine rain not so frequent as here,

has no doubt a great deal to do with it. In the case of arc light circuits entering and coming out of houses, and worked in series—as they always are—it is, of course, a material point that although the difference of potential between the circuit and the earth may be 3,000 volts, yet the difference between the outgoing and incoming wire is only some 50 volts; so that the tendency of the current to break across the leads is really small. Again, the chance of the current going to earth on dry ceilings and walls is, when one comes to look into it, not great. Thus, notwithstanding the rough character of most American work, and the poor insulation of the wires, in this class of work there is a greater freedom from faults and a higher margin of safety than one would at first imagine. The only place where the margin of safety is really low is where the wires enter buildings.

I have now, I think, gone over all the main points in arc lighting construction work. Construction work for alternating currents is generally carried out on much the same lines as for arc lighting; the only difference is that in the latter case converters are stuck against the outside of buildings instead of arc lamps, which, of course, are in parallel instead of in series. A rough hole is knocked through the brickwork just under the place where the converter is fixed, for the secondary leads, which are slipped through a couple of rubber tubes and carried inside. But little attempt is usually made to execute this part of the work in what we should call a neat or workmanlike manner.

Let us now make a *resumé* of this branch of the subject.

There are in America now, I believe, more than a quarter of a million arc lamps in constant use. In the large towns these arc lamps are fed by machines equal to keeping going 50 or 60 lamps each.

Outside a central station, supposing it to be for 1,000 lights, we shall find tall and untidy-looking poles 30 or 40 feet high. At the top these poles will have four or five cross arms, each carrying six or eight insulators. These poles will be continued along the neighbouring streets at intervals of 30 to 60 yards, according to circumstances. Every here and there a circuit will leave the rest for the side of a neighbouring house, supply one or more lamps,

being run in the latter case along the face of the houses, and then return again at the next pole. Or a circuit may jump across the street, supply a lamp or two, and then return again to the nearest pole on the side it first started from.

Here and there one or two circuits will leave the main line of poles altogether, and turn up a side street: in this case the poles in the side streets are generally smaller. It is surprising the length some of the arc circuits extend in this way—not unfrequently over a dozen miles.

As all the machines in a station are usually of the same size, and are run at the same speed, when the circuit is long the number of lamps fed off a nominal 60-light machine may perhaps have to be reduced to 45 to allow for the resistance of the leads. As you are aware, some of these arc machines are now used on power circuits in the daytime, which forms a very welcome source of extra revenue.

I trust I have not wearied you by a too minute description of this, a not very interesting part of electrical engineering in America; but I was anxious to convey as clearly as I could on what a remarkably simple and crude basis almost the whole fabric of American electrical engineering in this particular at present rests. For I think I am not overstating facts in saying that at least nine-tenths of all the electrical work in America—putting aside isolated plants—is carried out, as far as the construction work is concerned, in the manner I have described. In all this portion of the work I saw next to nothing which showed particular ingenuity, skill, or brilliancy of idea. Simple and crude means were everywhere used to effect in a rough and crude manner the end desired. In the means employed there is little to excite either admiration or interest. The interest lies in the fact that in such a simple and elementary manner so much has been done.

## UNDERGROUND WORK.

Turning now from this branch of the subject to underground work, I must confess that I had a good deal of difficulty in getting really accurate and reliable information on this subject—at any rate, of a comparative character. The Edison Company

have, of course, buried their wires all along, and their three conductors enclosed in an iron tube, run solid, with insulating material inside, are well known. As far as I could ascertain, the various systems of conduits have not proved very successful so far, and, since the proof of the pudding is in the eating, the fact that the Subway Company in New York are, I believe, laying down iron pipes entirely—at least, they were in every place I saw work going on—points to this conclusion.

The system of construction for underground work at present being carried out in New York is as follows:—

Manholes at convenient distances are made in the roadway—not, as a rule, in the pavement, but just on the rise of the road, and clear of the gutter. These manholes are circular, and are covered by a heavy iron lid. Below the manhole is a bell-shaped pit 8 or 9 feet deep, and bricked round, and of sufficient size at the bottom for a couple of men to stand fairly comfortably to work. Iron pipes such as are used for water or gas are laid in the ground from one manhole to the next. I understand that each electric company will, as a rule, have a separate pipe, so that if there are several companies running along the street, they will each require a separate line of piping opening into the manhole. There will, therefore, often be several pipes entering at each side of the manhole. These pipes enter the manhole at such a height as, roughly, to come between the hips and breast of a man standing on the bottom—at a height, in fact, to bring them in a fairly convenient position for working. The cables are drawn through the iron pipes in the ordinary way. Branches are taken from the manholes under the roadway and pavement to any house between the manholes requiring current.

As regards the class of cable which is being largely laid down in these conduits, the conductor consists of seven strands of No. 16 copper. These are insulated by a layer of a hard but somewhat flexible material, containing, I believe, some rubber in conjunction with other materials, but the complete material is cheaper than rubber used alone. The thickness of this material surrounding the conductors is one-twelfth of an inch. It somewhat resembles ebonite, and is of a consistency to stand

rough usage and hard wear. It will be interesting to see how it lasts in practice. I must say it seems to me a very suitable material for the purpose for which it is intended; and as far as insulating qualities go, I was able to see the record of tests of a good deal that has been laid, and they were excellent, going up to several thousands of megohms per mile. The insulating material is covered with a lead pipe about one-sixteenth of an inch thick, and containing, I believe, a percentage of tin. These lead-covered cables are usually jointed at each manhole, and a little slack left on them there. The jointing is done by a special system, and when it is complete electrically the two lead tubes are drawn together over a small bit of brass tube, which protects the point of junction; they are then "wiped" together with a hot iron like an ordinary plumber's joint. No doubt this work is heavy and expensive, but it looks as if it should last when once put down.

### Internal Work.

Coming now for a moment to the internal work in buildings, most of what I saw had a rough, temporary look about it, evidently being put up as cheaply as possible. I do not say that no good, solid work is done, but the proportion of it to the remainder is, I fancy, a good deal smaller than in this country.

Gas fittings are freely utilised, and I think I am correct in stating that in the arrangement of lights the gas-fitting way of managing things is adhered to much more closely than in this country, where we have rather tried to avoid imitating the ordinary ways of arranging gas lights.

Turning aside for a moment, it appears to me that one of the great wants all over the world now is, if I may so term it, a "science of illumination." Up to and as far as *providing* light we are exceedingly scientific. But in *utilising* it when we have got it, we proceed in an elementary and haphazard manner.

Mr. Preece has, I know, from the first paid a great deal of attention to this point, and appreciated its value. It seems to me that it would really be of great practical benefit if a committee of this Institution, or some other responsible body, would

accumulate some accurate and authoritative data on the subject. When once started properly, these data would increase of themselves, and would soon form a guide which would be exceedingly useful in drawing up specifications and arranging to the best advantage the lighting of either open spaces, public buildings, or private houses.

We all have our own indefinite notions on the subject as the result of experience, but if they could only be put into definite shape—and I think the subject does admit of being put into definite shape—it would, I feel sure, be a great advantage.

Of course there are innumerable patterns of switches and cut-outs and fittings in America, as here, but none came to my notice showing any very brilliant conception in design. Lamp sockets and fittings are generally somewhat larger and clumsier than with us.

The chief difference I noticed was that a switch is almost invariably included in each lamp socket. Americans, as a rule, do not go to the same amount of trouble and expense that is general here to fix switches in handy places near doors. If you want the light on or off, you turn it on or off at the burner as you would do gas.

### Central Stations.

Coming now to central stations, those I went over were the new Arc Light Station in San Francisco, the Edison Station in Chicago, the Westinghouse Station in Pittsburg, and the Thomson-Houston Station in New York; and I may add that since my return to London I have had an opportunity of going carefully over the new Westinghouse Station in Sardinia Street, Lincoln's Inn Fields, the plant in which is entirely of American manufacture and pattern, and arranged in the American manner.

I might easily have seen many more stations, in some cases perhaps larger and better arranged than those I did see. But those I did see are, I think, all typical of the latest American work, and all were in the process of making large additions to their plant, so that they were quite up to date.

The station in San Francisco is near the railway station. It is a large brick building, erected entirely for its present purpose, about 250 feet long by 80 feet broad, with a ground and two

upper floors, and has been finished about eight months. It is intended to provide accommodation for about 2,000 arc lights when filled up. At present it is furnishing current for about 600. It is arranged in the usual—I was going to say invariable—American plan of placing the engines and boilers on the ground floor, and arranging the dynamos, switch-boards, and electrical portion of the station on the first floor. In this instance the second floor is used as a storehouse, and for general purposes.

The boiler-house forms one end of the building, and there was nothing above it. It is arranged for two lines of boilers, one on each side, one side only being filled up at present. They face towards the centre of the building, with a large space between for coals and general working purposes. The boilers were of the well-known water-tube pattern. This type, varied slightly in detail by each maker, seems to be in general use everywhere in the States. From the boilers the steam is led to a 650-horse-power single-cylinder Corliss engine, which, by rope gearing, drives a single line of countershafting. I must say I was surprised to find that, in a new station so large as this, and one in which expense evidently had not been spared, the engine was neither compound nor condensing, particularly as there was water within a moderate distance. There was only this one engine in the station driving all the dynamos, though space was provided for three more as the amount of lighting increased.

The working of the station was therefore dependent on the single engine and line of shafting keeping in good order. The engine drove this line of countershafting as I have said, the loose pulleys being on the shafting. At the stations I visited where countershafting was in use I was struck with the care and completeness with which means were provided for adjusting the plummer blocks and bearings in any direction. The general idea was somewhat as follows:—The plummer block, or upright, stood in a cast-iron shoe with a flat planed bottom, through which the holding-down bolts projected. The holes for the holding-down bolts in the plummer block were bored large so as to permit a certain movement in the block. The edges of the cast-iron shoe were turned up all round, and through these edges projected screw

bolts, the points of which touched the plummer block. By loosening, then, the holding-down bolts at any time, and screwing or unscrewing the screw bolts as required, the block could be slewed slightly or adjusted to line with the greatest nicety and ease. Besides this, the under bearing worked in guides, being kept in its place by a strong screw projecting up from underneath. By working on this screw, and on the cap of the upper bearing, the height of the bearing could, of course, be adjusted with great exactness. With these means the brasses are also quickly and easily replaced; while if they get warm when the shaft is running, the bottom bearing can be slacked down. The Thomson-Houston Company in New York told me they did this, and their dynamos are on the same floor as the line of shafting. It is, of course, a still simpler and safer matter to do this when the dynamos are situated almost right above the shafting, as is the general practice in American stations. Leather belting of the ordinary description was in use, except for driving, where ropes are used, as I have mentioned. I found leather belting employed at each of the stations I visited.

These long lines of countershafting must, of course, be wasteful of power; still, it is worth pointing out that such shafting runs under the best conditions when doing work right overhead, since both the dead strain and the running strain on the belts to the dynamos tend to lift the line of countershafting, which, therefore, if the weight is properly adjusted, must run very lightly in its bearings.

The method of driving from underneath is also economical in belting, since the weight of the belt hanging on the dynamo pulley causes it to have a good grip, and permits of a short length being used. The belts pass up to the dynamos through holes in the floor, the holes being elongated to permit the belt being tightened, if necessary, by screwing the dynamo along on its bed rails.

In each of the stations I visited in America, the upper floor carrying the dynamos was entirely of wood, strongly made, and supported from underneath at intervals by stout wooden uprights, but still of wood.

The weight of the floor in the San Francisco station is

entirely supported by the wooden uprights, being only steadied at the sides. I was informed that at the old station in San Francisco, worked by same company, and which, I understood, was supplying about 1,200 arc lights, a good deal of trouble was experienced from the floor, which was fixed to the walls, and which, vibrating with the motion of the machinery, shook the brickwork. I found the vibration on these wooden floors less than I anticipated, considering the amount of machinery on them; but, of course, the motion of a good dynamo is very even, and if the joints on the belt are neatly made there is no jerking.

In this San Francisco station all the machines were Brush 60-lighters. From 50 to 60 lights in series on one circuit seems to be about the general rule for lighting in America when any number of lights is required. As all the machines are run from the same countershafting, and at the same speed, whatever work they may be doing, it is necessary to provide an automatic regulator to adjust the current through the field magnets for each machine when doing less than its normal output. On the other hand, if the circuit is a very long one—and I was told some of the circuits at San Francisco are as much as 13 or 14 miles—the number of lamps on the circuit must be reduced to compensate for the extra resistance. These Brush dynamos were all mounted on short iron columns about 9 inches high, so that they were lifted entirely from the floor. I was told that this arrangement was found very handy for keeping the machines clear and free from dirt, and for inspecting them. I did not notice much difference between the machines and those turned out by our friends on this side of the water, and the style of the workmanship seemed to be about the same. A light overhead trolley, with lifting gear, ran overhead so that it could quickly be brought over any machine, and I was informed that should a machine go wrong they could replace an armature in half an hour. In the older station in San Francisco a large number of Thomson-Houston machines are used, but Brush machines only are employed in the new station, as they find the Brush armature is more easily replaced, and is so much the easier of the two to

repair if anything goes wrong. It is worthy of note in this connection that the latest advices from America inform us that the Thomson-Houston Company have bought the Brush Works at Cleveland, Ohio, and therefore presumably the Thomson-Houston Company will in the future control this interest as well as their own.

From the dynamos the mains are led to a long narrow shelf, where are fixed the resistances and indicating instruments for each machine. The mains next proceed to the switch-board, which is of the plug pattern—brass plugs with boxwood handles united by a flexible insulated cord.

All the flooring being of wood, as it is in each of the stations I saw, and being kept clean and dry, the risk of getting a bad shock is not great, except by shunting the current through oneself, or getting in series with a main, which are not ordinary accidents.

At San Francisco the motor business is steadily increasing, and forms a welcome addition to the revenue of the station, without a very great additional expenditure.

I will now go on to the Edison Station in Chicago. There is no necessity to describe it at length, as the plans on which Edison stations are constructed have been published over and over again.

The boilers, which are situated on the ground floor, are of the water-tube pattern—Heine arrangement. In this pattern, which seems to be a good deal used, the tubes are much more nearly horizontal than in the well-known Babcock & Wilcox type. All the tubes come into a large flat box at each end, and there are small manholes in the boxes opposite the ends of the tubes, for getting at them. These flat boxes are prevented from expanding with the pressure by tubular stays, which pass right through them, and are expanded in their places. These small tubes serve as blow-holes for directing a jet of steam on the upper side of the tubes to clear them of ashes. They are kept plugged up with a stopper, under ordinary circumstances, to prevent the gases coming through. These boilers had a damper projecting downwards in the furnace opening, for directing the

draught over the surface of the coals when firing, to cause more complete combustion and prevent smoke. This damper may have been of some use for the purpose, but it certainly made it a difficult operation to fire properly.

The engines were also in the basement, in the next room to the boilers. They were of the ordinary Armington-Sims pattern, single cylinder, and neither compound nor condensing. There was one to each dynamo, the dynamos being situated overhead on the floor above. The dynamos were of the ordinary Edison pattern, for, I think, 1,500 lights each. Everything was very neatly arranged in the dynamo room, the current being conveyed about by large copper bars. The walls were simply lined with resistances and indicators for the different circuits. In fact, the place gave one rather the impression of a wholesale clock store, though the attendant informed me they did not have much difficulty in keeping their resistances properly adjusted. All the mains are underground, on the three-wire system. Indeed, there is a good deal of underground work in Chicago, the Telephone Company also having a large number of cables buried.

The next station I visited was that of the Westinghouse Company at Pittsburg. It is situated in a large building of the warehouse type, quite in the centre of the town.

The boilers here are of the water-tube type, but heated by natural gas instead of coal. This natural gas is brought in pipes to the town from a distance of 35 miles. The company who own the line of piping—of which, by the way, Mr. George Westinghouse is also President—sell this gas to manufactories, &c., at a rate which is nearly equivalent to the cost of coal; so that, although the convenience is great in using it, the economy is not so large as might at first be anticipated.

Work at the Pittsburg station was in the first instance begun with two Corliss single-cylinder engines of the ordinary type, driving on to a line of countershafting; but this method of working has not been extended, and one of these engines has since been removed. The next additions to the station consisted of Westinghouse Standard engines of 250 indicated horse-power, driving 2,500-light alternating dynamos. These engines

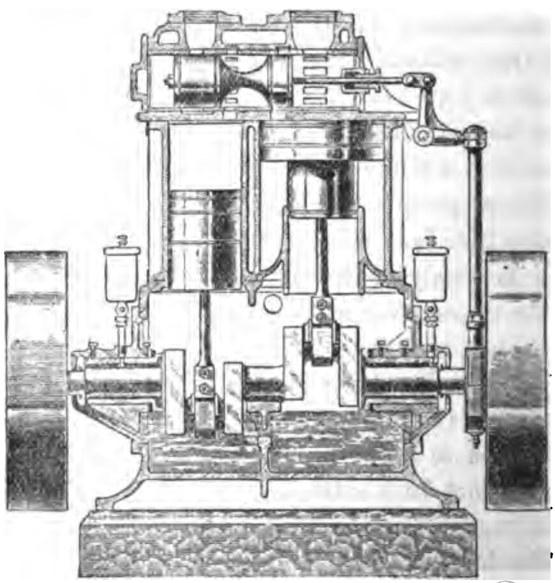
stand on square blocks of concrete and masonry; the blocks for this sized engine are about 7 feet by 5 feet. The last additions are of the new type of Westinghouse Compound engine. From a power point of view, as well as from an electric lighting point of view, the station has therefore something of historical interest.

While in Pittsburg I had an opportunity of going carefully through the Westinghouse engine factory with Mr. Ralph Bagaley, the vice-president and manager, and of seeing all the details of construction of each of the different types of engines.

The Westinghouse Company make three styles of engine, each worked by a piston valve and single eccentric.

The Standard type is so well known that I will not say anything about it. In it, as you are probably aware, the steam valve is between the cylinders, and set at a slight angle to them, while the cylinders are set rather over their work, and not directly in a vertical line above the crank shaft.

The Junior and the new Compound engines are on somewhat different lines to the Standard, while the main principles involved



WESTINGHOUSE COMPOUND ENGINE.

are nearly the same. In these two later types the piston valve lies horizontally along the top of the cylinder heads. It is operated by a single eccentric working outside the engine case in a vertical direction, the motion being changed to a horizontal one by means of an L piece attached to the engine case by a hinge. The whole movement is of simple character, and works exceedingly well. In fact, there are practically no more working parts in the Compound engine than in the Junior or the Standard two-cylinder patterns, and the one is as simple as the other.

The Standard type has now stood the test of several years' work very satisfactorily, and there is no room to doubt that the Compound engine, constructed in the same shops and by the same method as the Standard engine, will wear at least as well. The makers' tests show exceedingly good results for this Compound engine—viz., from 18½ lbs. to 19¼ lbs. of water per brake horse-power hour with condenser, and from 24 lbs. to 25½ lbs. without condenser. I saw the testing apparatus in the makers' shops, and engines undergoing their test preparatory to delivery, and feel perfectly confident that the figures given by the makers are thoroughly reliable.

I will not describe this engine more fully, although to do so would probably elicit an interesting discussion, as it would be wandering from the subject of my paper. On the other hand, the remarks I have made are not intended to compare the Westinghouse Compound engine with any other engine of at all similar type, but rather to draw the attention of electrical engineers to the fact that there are now, at any rate, two large firms of the highest repute turning out this class of engine, and ready to guarantee economy and efficiency on a par with Corliss engines.

Returning to the Pittsburg station, five Westinghouse engines of 250 indicated horse-power drive five alternating-current dynamos of 2,500 16-candle-power lights capacity each, besides the machine driven from the Corliss engine. The exciters are run from smaller engines. The engines driving the alternating dynamos make 250 revolutions per minute, the dynamos making 1,050 revolutions per minute. The power is conveyed

overhead by belts to the first floor, where the dynamos are placed. Each of these dynamos is on a sliding bed, so that the belt can be tightened, and, as I have already said, the exciters are run separately from the machines. The machines are so well known it is hardly necessary to describe them. I may, however, perhaps with advantage say a few words on the construction of the armature, which is the least easy part to understand from written descriptions or diagrams. In Professor Silvanus Thompson's book on dynamo machines the diameter of the armature is given at nearly twice its breadth. While this is true of the smaller machines, in the 2,500 machine—which is the standard size for large stations, and is the size employed in the new station in Sardinia Street in London—the length of the drum is considerably greater than the diameter. The drum itself is formed of thin iron plates, just like an Edison armature, only of larger diameter; and as the iron near the centre is useless, holes are punched in the sheets, which lightens the armature and secures ventilation. Round the outside of this iron core, and over the two ends, insulation is laid. As regards the coils of wire in which the current is generated, they are made in this way:—Beginning at the inner side of the coil, the wire is wound on an oblong frame to correspond with the shape of the field magnets, one turn on the top of the other. The coils are only one wire thick;—that is, there is only one layer of wire between the drum and the field magnets. When these thin flat coils are laid on the drum, the round ends of the coils are turned down at right angles over the edge at each end of the drum, and the connections from coil to coil are made. A small piece of wood screwed into the side of the drum, and projecting over the end of each coil, keeps it in its place. When all the coils are fixed in their places, sheets of mica are laid over them all round the armature, and bands of fine wire with a little thicker insulation underneath them are bound round the whole armature at short intervals, in exactly the same way as is usual with the armatures of continuous-current machines. An armature made in this way has perhaps rather an amateurish look about it, but there is no doubt that they work well in practice and give excellent results. It is

further worth noting that as the field magnets are long and narrow—a ratio of at least 10 to 1 in the large machines—they cannot be very economical as far as exciting current is concerned. Still, since the exciting current, anyhow, bears such a small ratio to the energy which the machine will transform into electric current, this is not a very material point.

The mains are run from the dynamos to the switch-board in troughs cut in the joists supporting the floor, which is made to take up over these troughs.

The switch-board at Pittsburg consisted of a wooden frame boarded over, about 9 feet high and 12 or 14 feet long, and standing about 3 feet from the wall, so as to give room to go behind and make the connections. This, I may say, is the usual way of arranging switch-boards in America.

The general type of switch used in America for alternating-current work—and it is not confined to the Westinghouse Company—is exceedingly simple in construction and operation, as well as efficient. In general principle it is like an ordinary tobacco chopper. To the top of a small upright a lever is hinged, terminating in an insulated handle. The metal part of this lever has a hatchet-shaped projection cast on it, projecting in the same plane as the motion of the lever. When the lever is pulled down the hatchet-shaped piece sticks in between two spring pieces set up to receive it, and so makes contact. If the lever has two hatchet-shaped pieces attached to it opposite each other, and engaging in spring pieces on either side, according as the handle is thrown over in one direction or the other, we have a two-way switch. Unite two of these levers together by a cross bar of insulating material, with a handle in the middle, and we have a double-pole two-way switch. Place a spring in the columns holding the hinges on which the levers turn, to throw down the levers when nearing the spring contact-pieces on either side, and we have the complete article. These switches were all mounted on wooden bases. In fact, I saw no slate used in America anywhere for such purposes, except at the Electric Club in New York. In the new Westinghouse Station in Sardinia Street, however, the whole of the switches are mounted on enamelled slate, and very nice they look.

One exciter is used for several machines; each exciter has a resistance in its field magnets, which can be varied. There is, moreover, a separate resistance in the field-magnet circuit of each alternating machine, so that the E.M.F. of the machine can be varied while its speed remains constant.

Double throw-over switches are connected so that each alternating-current machine can be connected to either of two exciters, and so that any main circuit can be run from any machine. The general principle followed is to keep the machines at work as fully loaded as possible. Thus in the day many circuits will be run off one machine, but as evening comes on they will one by one be taken off and put on other machines. The throwing over is so quickly done as to only just cause a perceptible flicker in the lights. All these switches were bare and unprotected.

I did not hear or see much of the method of running two separate machines parallel on the same circuit. Whatever may be done in isolated instances, it does not appear to be practised generally; indeed, with the American system of running a number of small circuits instead of fewer heavy ones, the desirability of doing so does not often come in.

I should like to endorse here what I see Professor Geo. Forbes said in his paper on foreign central stations with regard to feeders. The area which can be economically supplied from a central station has certainly been greatly increased by the adoption of 1,000 or 2,000 volts instead of 100. But at the same time our notions of the area which can be supplied from one centre have experienced a corresponding growth.

If, then, such an increased area is supplied on a high-tension system from one centre, we have as great, or nearly as great, falls of potential to deal with as if the low-tension system were used over a correspondingly smaller area.

To secure the highest class of lighting, therefore, together with the flexibility desirable in an installation lighting a large area, the whole of the outside work should be based on as complete and carefully designed a set of feeders as from our present knowledge we should employ with a low-tension system.

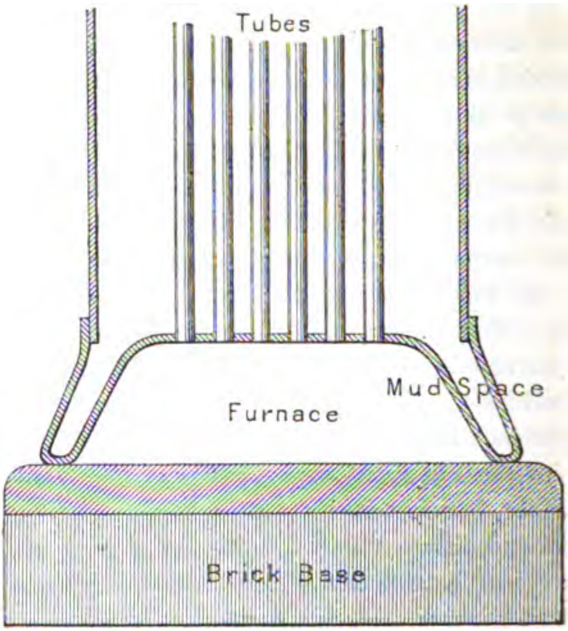
On many of the Westinghouse feeders lighting at great

distances, there is a fall of potential of as much as 10 per cent. To avoid pilot wires an indicator is used, which, I think, Professor Forbes has already described here. There is no regular voltmeter for driving the machines by, but an indicator is used with but one single mark on the dial. When the machine is giving the voltage which is required at the further end of the feeder to keep the lamps at their proper brightness, the hand of the indicator will be right over the mark on the dial. The loss in the feeder will, of course, differ according to the current flowing through it; so, to allow for this, a compensator is attached to the indicator. The main current flows through the compensator, which is arranged to cause a slight back-pull on the needle of the indicator; this pull becomes greater as the current increases. As the current increases, therefore, a slightly higher voltage on the machine will be necessary to keep the needle in its place. The instruments have, of course, to be made so that they can be adjusted to the calculated requirements of each feeder. When once set right, however, their operation is simple enough.

The Westinghouse Company also use a piece of apparatus containing an adjustable converter, which enables them to supply current from the same machine to two feeders having a different fall of potential.

The Thomson-Houston Station in New York is situated in Twenty-Fourth Street East, near the river, and occupies what was formerly a large sugar refinery. It is, therefore, a building of the warehouse class, but very strongly built. I was told that this station was neither so large nor so well-arranged as their station in Brooklyn, but the latter, I regret, I had not time to see. Two boilers struck me here as out of the ordinary patterns. The simplest way to describe them is, I think, to consider them as large locomotive fire-tube boilers set on end. The tubes are about 15 feet long, and the water line is about 10 feet up the tubes. This gives a considerable steam space above, through which the fire tubes pass, superheating the steam generated, and thoroughly drying it. At first sight one might suppose this an uneconomical pattern, as the heated gases pass so quickly into the flue. But the gases appear to give out their heat well,

for Mr. Foster, the station superintendent, told me the temperature of the gases going into the flue was not above 550 degrees Fah. The lower portion of this boiler is, however, the




BOILER WITH TUBES.

interesting part. I cannot very well describe it without the aid of a diagram. The outer shell of the boiler is expanded out and prolonged about a couple of feet below the tubes. It is turned up at the bottom, and united all round to the crown plate of the boiler, into which the lower ends of the tubes are expanded. This prolongation of the shell then forms a circular hollow, or chamber, about 2 feet deep. The internal space enclosed by this hollow chamber forms the fire-box or furnace, and is about 6 feet in diameter. Any sediment collects in the hollow chamber, and can be taken out at manholes. There were two of these boilers in use, and it was in contemplation to increase their number.

The engine—and there is only one—which furnishes power to this station is a large compound condensing Corliss engine; the two cylinders being in tandem. It is a very fine piece of work of its class. It drives on to a large fly-wheel with a smooth

face of about 60 inches breadth. This fly-wheel transmits its power to a line of countershafting, which is arranged on the floor above, by means of a large leather belt 58 inches broad and of great thickness. I think, in all, seventeen machines are run from this line of countershafting—fifteen 55-light arc machines and two alternating-current machines; in this case the machines are on the same floor as the countershafting. The general construction of the Thomson-Houston alternating-current machines is on the same lines as those of the Westinghouse Company. The main difference is that instead of the ends of the coils on the armatures being turned over the edge of the core, as in the Westinghouse machine, the armature has a sort of prolonged lip all round the outside at each end, and the ends of the coils lie flat on this instead of being turned over. To their smaller machines, also, the Thomson-Houston Company frequently add a commutator, and commute part of the current to excite the field magnets, as Messrs. Zipernowski, Deri, and Blathy are in the habit of doing. The switch-board here is on much the same lines as I have already described. It was all quite open and exposed; but I was told that, in view of an accident which had happened there a short time before, they proposed to cover the exposed parts up.

This way of leaving the working parts of high-tension alternating-current switch-boards exposed is not confined to American manufacturers: a good deal of the Continental work is even worse. Yet all the different patterns of switches and converters could be protected at a moderate cost, and by the exercise of a very moderate amount of skill and ingenuity. I cannot help thinking that not to do so, to save money in this direction, is false economy.

## Isolated Plants.

Let us now turn for a few moments from central stations to what are called "private installations" in this country and "isolated plants" in America. I have already spoken of internal construction work, or wiring and fittings, in connection with central stations. With isolated plants there is not much difference. Of course the character of the work differs greatly in different places, as it does here; but, taking it as a whole, I don't think it is equal

to our standard, whether from an engineering or artistic point of view. I noticed, by the way, that there is a much greater tendency to turn incandescent lamps upwards in fittings, like gas jets, instead of downwards, as with us the majority of lamps are turned. Isolated plants are driven by steam and gas engines, located in cellars and basements, exactly like they are here. As you are aware, accumulators are not so generally used; I only saw the set at the New York Electric Club. With respect to dynamos, the Americans have been fully alive to the developments, both theoretical and practical, in our knowledge of the magnetic circuit which have taken place during the last four or five years, and all their modern machines appeared to me fairly up to date in this particular. What I may be permitted to call the Edison-Hopkinson type, the Kapp type, the Manchester type, and the Weston type—the latter with two magnets, placed horizontally—appear to be the favourites, just as they are here. The construction and workmanship of the machines appeared to be much on a par with our own; there is little to choose between in one or the other. As to the way the installations are run, judging by that crucial practical test, the look of the commutator and brushes, it is much the same as here; certainly, I think, on the whole, not better. The lamps, as a rule, I did not find any steadier, nor are they kept nearer their proper voltage than they are here. This applies to arc lamps as well as incandescent: there is no magic about arc lamps over on the other side of the water; they are subject to the same vicissitudes and uncertainties as they are here, and do not burn steadier than those in any moderate-sized station here where the appliances are such that the lamps get a fair chance.

The switch-boards in these private installations are nearly always mounted on wood, and there is even a disposition to cut things fine in the strength and solidity of these wooden base-boards, somewhat, as it seemed to me in one or two instances, at the expense of efficiency and lasting qualities.

### Electrical Tramways.

Such very full accounts have been published of everything that has been done in this direction, that there is really little I

can add to them. In the *Electrical World* for October 19th there is published a table of the electrical tramways in actual operation *and under contract* in the United States. The total mileage of both is 1,260 miles, with 1,884 cars, on 179 roads. Of this, 716 miles were, it appears, actually being operated by electricity at the date of the return, and 544 miles were under contract. Of this total, 21 miles were worked by cars with storage batteries, and all the rest, with the exception of two or three miles, by overhead conductors.

Nothing can be simpler than the way these overhead conductors are put up. Two lines of posts, about 20 feet high, are set up opposite each other, one on each side of the street; light steel wires are run across from one to the other; a small iron holding-piece is attached to the centre of the steel suspender, and this holding-piece carries a bare hard-drawn copper wire, along which the current flows, returning by the rails. Where feeders are used, they run along one side of the street, and connections are made at intervals with the central wire. The way the conductor is carried round corners is on a par with the rest of the work. Posts are set at the further corners of the street, and from each of these two, three, or four steel wires are run at an angle with each other, meeting corresponding wires run from posts on the inner side of the curve. In this way the wire is made to take the form of part of a rough polygon, while the horizontal and vertical movements of the fishing-rod connector on the car are sufficiently ample to keep the contact-wheel on the wire when the car is going round a corner.

This method of erection does not look very nice, certainly, but it appears to answer the purpose well. Such work could, I think, be more solidly carried out here for about £70 a mile. In a few places standards with projecting arms placed in the centre of the street have been used for carrying the overhead conductor. If neatly constructed, there is no reason why these should be unsightly, and they can be used for the further purpose of lighting the streets.

A great point about these electrical tram-cars is the way in which their speed can be varied. They can be run at three or

four miles an hour in crowded streets and while turning corners; or in broad roads in suburbs where there is not much carriage traffic, the pace can be increased to 16 miles an hour. The cars are stopped very quickly and reversed, and will, of course, run either way on the same track.

The companies claim a reduction of 50 per cent. in the cost of working over the employment of horses, putting aside all the extra facilities which the electrically propelled car gives. A very noticeable point about all electrically propelled cars is the quiet and gradual manner in which they start and come to rest, and the evenness of their motion while running. In these points they greatly surpass both cables and horses. I took the opportunity of talking with a good many members of the general public in the train and hotels about the working of these cars. There is no doubt that the outside public thoroughly appreciate them and understand their value. At present electrical tramway work is the most "live" branch of the electrical industry in the States, and everyone connected with it was full of buoyancy and hope at its future prospects. It is really sad to think that, after all the skill and care which has been lavished on electrical tramway work here, it should be dragging along in such a half-hearted and pottering fashion.

## Conclusion.

While in America, through the kindness of Mr. Lockwood I had an opportunity of going over the telephone exchange system of New York with him, and of seeing the Courtland Street Exchange, which contains more than 3,500 subscribers, and is capable of extension to double that number. I also visited the Western Electric Company's factories both in Chicago and New York, where all the telephonic apparatus for use in the States is made. Although a description of the present state of this industry would properly come under the heading of my paper, seeing that the paper has already extended to such length, and that, owing to circumstances, telephony is a matter of comparatively limited interest here, however important in itself, I will not say more about it, but will conclude with a few general remarks.

In the early portion of this paper I endeavoured to convey an idea of the roughness and crudeness of American outside construction work. After reading over the proofs of the paper, I do not think I have overdrawn the picture; rather it is underdrawn, if anything. At the same time, I do not wish it in any way to be understood that American engineers are necessarily rough and untidy in their ideas, and incapable of different work. Putting aside their electrical plant, the numbers of beautiful tools, crowned by the latest pattern of the phonograph, which come over here from America, point to an entirely different conclusion. In accuracy of workmanship and careful finish, when they consider it suits their purpose, the Americans are our equals, if not our superiors: the difference lies in the fact that our ideas often differ from theirs as to where this accuracy and careful finish should come in. English engineering would hardly dare to appear before its fellow-citizens as the responsible owner and designer of such a terrible eyesore as American outside work is; but the American engineer appears to look at it from a different point of view. It is not his business to think of his fellow-citizens. Each of his fellow-citizens, he considers, is quite competent and capable of taking care of himself. It is the business of the electrical engineer to forward and extend the applications of electricity. If in doing this he does anything disagreeable to his fellow-citizens, it is for them to protest when it becomes intolerable to them, not for him to think of it beforehand.

And I would like you to try and think for yourselves, and really feel the enormous facilities which American engineers have in the past enjoyed, and still enjoy, in extending their business, by simply being allowed to erect open wires everywhere through the streets. Even where wires are now being put underground, they are largely put underground in order to carry on existing work and to extend it, not merely to take advantage later of what may chance to turn up on the route.

Besides this, we have all had it drilled into us by this time that it does not matter how perfect, how desirable, how useful a thing is likely to be, before it can come into general use the public have got to be educated up to it, to see it, and feel its

desirability and utility, and its practicability—to understand it each man for himself. Now the American system of overhead wires and cheap line construction, bad as it may be in itself, has from the very first brought electricity home to everybody, right to their very doors, not so much as a luxury, but as a practical desirable factor of everyday life, an attraction to public resorts and a good advertisement for business. Consequently electricity is in America no more a wonder, but a fact of everyday life to an extent that it is not yet here.

Looking at their plant, looking at their apparatus, and looking at the facilities which Americans have enjoyed for extended practical experiments on a large scale, it does not appear to me that their actual achievements, simply from an engineering and scientific point of view, have been greater than ours have, in spite of the apathy on the part of the public and investors with which we have had to contend. But the fact remains that they have done more business than we have, even considering the difference in population.

Of what work we have done so far, Englishmen may feel justly proud, and with better times I trust we may still be able in friendly rivalry to show that we are competitors worthy of America's best mettle.

The President.

The PRESIDENT: The great extent of the subjects touched upon in this paper is such that I am sure we all feel that more than what remains of this evening will be required for its discussion, and it is proposed to continue the discussion at our next (the Annual) Meeting, on December 12th. Before that date a complete print of Mr. Addenbrooke's paper will be in the hands of the Secretary, ready for distribution, upon application, to those desiring copies. We have some time still for discussion this evening, and I hope the members of the Institution will use it by giving their views upon the communication which has been read.

Professor Forbes.

Professor G. FORBES: The feeling which I have had during the whole time of listening to this interesting paper has been one of admiration of the realistic way in which Mr. Addenbrooke has brought before us a thoroughly good idea of the way in

which electric light work is done in the United States. I must say that one can see in the face of it that it is a very exact representation—anyone can see that—and I can bear witness to it as recording exactly the condition of affairs when I was there last, which is nearly eighteen months ago; and apparently there has been but very little change in the general way of working from the time when I was there. It seems to me to be very much the same; and in that part of the paper which deals with the general modes of construction and working of electric systems, whether it be arc or incandescent, I must say that I thoroughly endorse the opinions that Mr. Addenbrooke expresses. It is a very clear and good account of the work. It is undoubtedly true what he has said in his concluding remarks—that their general modes of construction are rough-and-ready, and are an eyesore, and may sometimes lead to dangerous results. But although they are rough-and-ready in manner, the country is one which feels that they want to extend these modern developments as quickly as possible, and that if they waited to do it in the expensive way in which we are accustomed to consider necessary, it would be a long time before the same extension would be made to outlying districts; it exists at the present moment even in the far West of America. As Mr. Addenbrooke has pointed out, it is just the same as regards the American railways: when we travel on them we are accustomed to say that an English engineer would not build such a railway—such light work, and generally such bad permanent way, comparatively; but an American immediately answers you: "Look at the thousands of miles that "we have to cover, the enormous distances that we have to go; "if we had put down the whole of our railways in the same "substantial and expensive manner as you have in England, we "should not have one-fourth of the facilities, and the trade of "the country would not have reached the present position that "it has." Professor Forbes.

In the whole of this paper I do not see that there is anything to criticise, or to add to, in the matter; and I can only congratulate the Society on having the condition of things so forcibly brought to their mind. I think we are on the right track certainly,

Professor Forbes.

but we do want our outdoor work certainly more substantially, securely, and permanently erected, and we shall be all the better in the end for the reminder. We have been very slow in beginning our central station work on a large scale, but now that we have begun it, we are doing it pretty thoroughly, and I do not think it will be long that the United States can say that they are far ahead of us in this branch of the practical applications of electricity.

I have said that in the descriptions which Mr. Addenbrooke has given us I have seen very little difference from the condition that things were in when I was last in America, and yet, in the correspondence I have with friends over there, I am continually hearing of very great strides that are being made; and I had hoped that I should have seen in the paper some account of some further advances, though, however, I must say I did not expect much new information in the allusion to electrical tramways. When I was over there, there were very few electrical tramways, and, curious enough, I was more often in an electric tramway driven by accumulators than I was on electric tramways driven otherwise. On three separate occasions—in Boston, Philadelphia, and New York—I travelled on tram-cars driven by accumulators, and I was only on one driven otherwise. Those were all mostly experimental, whereas now it is totally the other way.

I had hoped that Mr. Addenbrooke would have been able to give us some information on the advances in America in certain details which are interesting us at the present moment very much indeed. I would specially mention the question of motors, and particularly alternating-current motors. We have all heard a very great deal of what is being done, but the accounts that come to us are generally filtered through various media, and we would have liked to have had some direct description from the United States of what is being done by means of the alternating-current motor. I see that lately M. Zipernowski has started his large station for motor work, and that really seems to be a more practical thing than any of the alternating-current motor work that we have accurate information of from America.

Professor Forbes

Also, I should have liked to have heard something more about the practical use of meters in the United States, especially for alternating-current work. The use of the Edison meter we are perfectly familiar with, and we know how very largely it is used in America; but the alternating-current meters have only lately been used, and I would have been glad if Mr. Addenbrooke would have told us to what extent they are being used, and how the difficulties of using meters are being met.

One of the chief difficulties in using meters which we always come across when we are thinking about putting them in, is their small range; and if the meter has only a range of 10, or 12, or 15, as is generally the case, there is a difficulty. One meter will register from one lamp to 12 lamps, but what are you to do between that and 100 lamps? Are you to begin with registering only eight or nine, and leave the first eight or nine without record? or what is to be done? These are the points on which I think it would be interesting, if there is any information from America, to have it given us.

There is one feature in American factories which I noticed when I was over there, and from what I have heard from friends in America since, I know has been extended still more, about which also I think some information might well have been given us, and that is about the enormous amount of time and money that has been spent in the factories upon tests.

The laboratories of all the large manufacturing companies are splendid establishments, and I know that since I was there in most of these the system of accurate testing has been increased very much indeed; and it would have been useful to draw attention to this fact, because in this country it is too often the case that it is not done—there is not sufficient testing. I will mention one point in which I have noticed the difference very much indeed, and that is in the testing of converters. I do not think any converters in this country have been put through a really thorough set of tests for all the different sizes of converters that are made, and for different outputs, and I know that it is due to the careful tests that have been made upon the other side that the converters which are in

Professor Forbes.

use over there are so much more efficient than many of those which are in use on this side.

I am glad that Mr. Addenbrooke has drawn attention to the importance, which has been overlooked so much, of having our mains, even on high-tension alternating-current systems, supplied by feeders. I believe there is not a single central station in this country of high-tension alternating-current distribution in which feeders are employed. In America, I think that with every large central station feeders are employed as uniformly with them as they are on the low-tension system.

There is only one other question I would like to have had some information from Mr. Addenbrooke about, and that is with regard to one class of underground wiring, which I am not quite able to recognise from the description he gave. I should be glad if he would tell us what is the type of cable which he has described: probably I shall know it by some other name.

Mr. G. L. Addenbrooke: The Calder.

Professor G. Forbes: I do not recognise it.

Mr. Donovan.

Mr. H. C. Donovan: I hoped that Mr. Addenbrooke would have dwelt more fully on the atmospheric conditions of the American climate. The extraordinary dryness of the air naturally renders certain precautions as to insulation utterly unnecessary in the northern parts of the United States and Canada. A common blue glass insulator stuck on to a piece of deal, and that nailed on to a pine post, would, in this country, and more especially in London, be soon a source of very great leakage; for be it remembered that the conduction to earth would not be through the glass and wood, but over the surface when such surface is damp and dirty. Therefore, in a dry, clean atmosphere, blue glass on wood is quite as serviceable as the finest porcelain and ebonite. It is quite natural to see that a dry climate is a premium on carelessness and rough-and-ready work.

What Mr. Addenbrooke has so graphically described with regard to electric lighting work, I have myself observed with regard to telegraphic intallations some years ago in the States, and have heard many others remark on the same subject. The accepted opinion is that the superior brain-power of the

Americans enables them to disregard insulation, &c.; but, from a process of observation and reflection, I have come to the conclusion that it is not superior brain-power that favours electrical enterprise in America, but climate. When on the introduction of telephonic and electric lighting enterprises in this country, most of the installations put up by American electricians soon proved, on account of faulty insulation, inefficient, the fault was not in the brain-power of the Americans, but due to the English climate, which, one need hardly say, is at times damp. Mr. Donovan.

Mr. W. P. Granville: Although, perhaps, a little apart from the immediate subject under discussion, I should like to call attention to the unsatisfactory way in which the insulation resistance of electric light mains is usually expressed—that is, by simply stating the number of megohms per knot, irrespective of the diameter of the conductor. Now that we have insulated wires of such widely different dimensions, it seems necessary that the dielectric resistance should be expressed in terms of the *diameter* as well as of the length of the conductor, because its insulation varies not only with its length, but also with its diameter. Mr. Granville.

Professor S. P. Thompson: Though it is four or five years since I was in the States, Mr. Addenbrooke's paper brings out so forcibly the style of work out of doors there that I felt as if I were back again in the States. I am disappointed in that fact, because I had hoped that five years would have made a great difference in the style of outdoor work that was in vogue then in America. One thing, of course, is new since, and that is the use of alternating currents with transformers. In 1884, when Professor Forbes lectured in Philadelphia on alternating currents, he could not find in the whole of the United States an alternating-current machine wherewith to illustrate his lecture. Professor Thompson.

I should like to refer to the glass insulators. I brought one back with me from the States, and will have it here next meeting. Though alike in other respects, mine differs from Mr. Addenbrooke's in being even more primitive: the piece of wood is simply chopped to a rough point with a hatchet, and the glass insulator is forced upon it; there is no screw on the wood. The

Professor Thompson. back of the piece of wood is hollowed a little so as to fit against any convenient tree trunk. Incidentally it furnishes an interesting illustration of the way in which the patent laws of the United States encourage small inventions; for there was a special patent taken out in the States for the manufacture of a wooden arm to hold an insulator, the back of which arm is hollowed out so as to fit against a tree trunk!

Mr. Addenbrooke has referred to the long circuits on which arc lights are used in the evening as being applied now in the daytime for the distribution of power. I wish he had been able to say a little more, because I want to know what kind of motors are used running on long circuits of this kind. Are they constant-current motors? How have they been made to work as constant-current motors? Or are they simply machines applied as dynamos—Thomson-Houston or other dynamos—and used as motors?

Mr. Addenbrooke makes the remark that the lamps are made as roughly as, or more so than, the average arc lamps in this country, and yet one hears so much about the steadiness of electric lights on the other side. I am inclined to think that the difference is, after all, not in the machinery—either in the engines or dynamos—or in the rough-and-ready ways of wiring, or in the design and construction of the mechanism of the lamps; I am inclined to think that the main difference lies rather in the *men*—that in America the electrical engineer who has the charge of a station, or who is perhaps only a lineman, takes a great deal more personal pride in keeping his machines, his wires, and his lamps in good order than the average engineer whom we can manage to grow in this country. The working engineers in this country never seem to me to have exactly the same amount of personal pride as the American. The American engineers will tell you and enforce upon you that their system is the best in the world, and that there is no equal to it. Every lineman, every fitter, believes devoutly in the supreme excellence of the system upon which he is at work, and will not hear a word against it, or in favour of anybody else's; he has a personal pride in his own. It is a difference of temperament, perhaps, and may be due to their climate—that splendid climate by the side of which ours is so very dismal.

Mr. J. SWINBURNE: One point Mr. Addenbrooke has not mentioned is the difference in the commercial management, or the organisation of American electric light businesses. In England a firm is usually run with one man as a head, who is partly an electrician, partly a business man, partly an engineer, and partly a commercial traveller. In America they separate all these. The electrical man has nothing to do but to design apparatus: he has assistants under him; and all the costs of testing, and of everything made experimentally, are kept separate, and are allowed for in a separate account. When he is designing machines, they are not for special orders, but simply for work that is going to be put in hand and made wholesale. He has to perfect his dynamo, and when it is designed he has nothing more to do with it; it goes into the hands of the superintendent of the factory, whose business it is to keep to the design exactly, and make large numbers of the machines without any variation whatever. Whenever work can be done in that way the Americans can beat us, and the reason is obvious. Mr. Swinburne.

I should like very much to ask what sort of thing the Zipernowski motor is that we hear about. We have seen accounts and pictures of it, but the pictures are always carefully arranged so that you cannot see anything.

Referring to the insulation of cables, I wish—and I daresay a great many other people wish—that the way of measuring insulation and conductivity of cables in megohms, per mile or per knot, was given up. If resistance is to be used at all, it must be in megohm-miles; but I do not see how the diameter can be taken into account, as the leakage does not vary as the surface. It would make it rather complicated, and I think it would be better to specify the megohm-miles for each particular conductor and insulation.

As regards central lighting generally in the States, I think we are a good deal misled in England by continually forgetting the price of gas in America; I do not think it really is that the Americans are so very much ahead of us in electricity, but that they are lamentably behind us in coal gas.

The PRESIDENT: If they cannot get their coals at a

moderate expense, they cannot get their gas at a moderate expense.

Professor G. Forbes: There are two kinds—gas coal and steam coal.

The President.

The President: I am sorry that I shall not be able to be present this day fortnight. I should like very much to have heard Mr. Addenbrooke's reply, but I trust I shall see it all in print. For myself, I must say that I have listened to his paper with exceedingly great interest. I am sure it will do much good to us in this country; it certainly is of a highly stimulating character.

The very description of what he called "rough-and-ready" American methods should be carefully weighed in our minds, so as to get whatever good we can out of the "rough-and-ready" method. The business of the world—in railway engineering, and a great many other things in commerce and colonising—has been done by rough-and-ready methods, and electrical engineering has certainly been promoted very largely in that way. The very beginnings in electrical engineering were rough-and-ready, and if we stop to attain too much perfection in common details we shall lose in the race, or be further behind in the race than we need be. But when danger is involved in rough-and-ready methods they must be restrained, and instead, every practicable precaution, and every attainable perfection of appliances to ensure safety, must be demanded of the constructor.

I quite agree with Professor Forbes as to the difficulty in understanding how these currents could be used for motors.

As to the question which Mr. Swinburne put to Mr. Addenbrooke, I think it a very important one. We certainly are very anxious to know, and to know as soon as possible, what practical methods there are for distributing power by electricity. We have hung back a great deal too long; we ought to have sewing machines, lathes, and machines in workshops, all over every place that is supplied by the electric light, now driven by electricity. As to electric propulsion for carriages, the overhead wires that Mr. Addenbrooke has described will be a very great difficulty indeed

in this country in the way of electric traction. I would like to ask him the question, How is it possible, in the towns, to have the wires low enough to give the contact to the electric car, and yet high enough to be out of the way of things that are often carried through the streets, such as highly loaded waggons, fire escapes, and many other things that reach up much further than would be convenient for the current-catcher of the electric tram-car? The President.

Mr. G. L. ADDENBROOKE: I would have preferred answering these numerous questions at the end of the discussion next meeting. I hope you will not expect me to be a sort of walking dictionary on American engineering of all sorts; but as far as further advances go, I rather put the question on one side. It has been really, perhaps, Professor Forbes's special line to have told us hitherto all about American advances, and I was not myself so anxious to hear the very latest thing that was done as what was really the practical position of affairs altogether. We know perfectly well that, however good an invention is, it takes two or three years as a rule before it works out to be of public use, and therefore I did not go into those parts of the subjects perhaps quite so much as I might otherwise have done. Mr. Addenbrooke

With regard to motors on arc light circuits, I cannot tell you very much, as I did not actually see any of the motors at work, but I will try to supply some information later. I saw other motors at work driving printing machines and so forth, and giving great satisfaction.

As far as testing and experimenting go, I quite bear out what Professor Forbes has said. There is no doubt that the Americans are beginning to be careful on this point, and to appreciate the value of testing. But you must understand that different localities differ very much. America is not one place any more than Europe is; and because they do so and so in one place, it does not follow that the same is done everywhere else.

Of course the dryness of the climate has an enormous influence on their work; and not only the dryness of the climate, but when they have rain it comes down heavily, and they do not get the drizzling days which we get here, and which ruin all

Mr. Addenbrooke. insulation. As most people know, a good heavy shower will improve insulation after it is over, but it is the constant damp and drizzle that affect us here.

As regards the insulation of wires. It is perhaps a little out of the subject, but I must say I think that in talking about the insulation of electric light wires the absolute thickness of the insulation should always be mentioned. Of course, if the insulation is expressed in terms of the diameters of the core and insulation, we can get at it, but it is very important we should know the absolute thickness of the insulating material: if it is a first-class material, and *so* thick, we know it is likely to answer our purpose.

It was remarked by Professor S. P. Thompson that there does not appear, from my paper, to be any very great difference in the overhead work since he was in America five years ago. Well, as I have pointed out in my paper, there is no doubt that from an engineering point of view the thing answers its purpose fairly well, and could not possibly be cheaper; consequently, until the public protest, the character of the overhead work is not likely to be greatly altered.

The interest which men take in their work was also referred to by Professor S. P. Thompson. There is no doubt that that is very much so. For one thing, their wages are of course better, employment perhaps more constant, and the chances of getting on better; and these are things which always make men take a greater interest in their work.

With regard to the President's question as to the crossing of streets for the electric tram-car connection, the fishing-rod connection extends about eight or nine feet above the top of the tram-car. This leaves a good deal of room for traffic to pass under. If a cart of hay, for instance, were piled up very high, and the wires were not at a sufficient height for it to pass under, I suppose it would have to go round.

The PRESIDENT: How about the short-circuit between the two wires?

Mr. G. L. ADDENBROOKE: I did not hear anything about that, but in large towns they use this [*drawing sketch*]. Posts are fixed with double cross-arms upon them, and the wires are run so. It

is an exceedingly convenient way of doing it, and lamps can be put on the top. Mr Addenbrooke.

The PRESIDENT: All other carriages keep out of the way?

Mr. G. L. ADDENBROOKE: They must take care also to keep out of the line of posts. They can go on the line of tramway as long as they keep clear of that part of the road where the posts are placed.

The PRESIDENT: The discussion will now be adjourned until this day fortnight, when the Annual General Meeting will be held.

A ballot took place, at which the following were elected:—

*Foreign Members:*

| | |
|---|---|
| Edward J. Hall, jun. | Axel Hultman. |

Masayuki Otagawa.

*Members:*

| | |
|---|---|
| William Henry Allen. | William Alexander Bryson. |

*Associates:*

| | |
|---|---|
| George Carnegie Alexander. | George S. Hooker. |
| William John Alexander. | C. Frewen Jenkin. |
| Arthur J. Arnot. | Hugh Haweis Paynter. |
| Francis Gibson Baily. | Peter Theodor Raaschou. |
| William St. John Beale. | William John Harrison Ryder. |
| Henry W. Bowden. | Charles Sheldon Thomson. |
| Vernon Keble Cornish. | Hubert Edwin Tutte. |
| Herbert John Dowsing. | Thomas Savile Watney. |
| Henry Llewellyn T. Foster. | William Wynn Williams. |

*Students:*

| | |
|---|---|
| Sidney Hopwood Blake. | William Rowland. |

The meeting then adjourned.

# ABSTRACTS.

## W. KOHLRAUSCH and C. HEIM—EXPERIMENTS WITH ACCUMULATORS FOR STATION WORKING.

*(Elektrotechnische Zeitschrift, Vol. 10, p. 303, 1889.)*

The two cells on which the experiments were carried out had formed part of a battery which had been in daily use from October, 1832, till December, 1887. Each cell contained three positive and four negative plates; the surface of each plate was 36 square dm., and the liquid was dilute sulphuric acid of specific gravity 1·115, the quantity being 8·4 litres. The cell, when ready for working, weighed about 20 kg. The capacity of each cell was 45 ampere-hours, the normal charging current was 5 amperes, and the discharge current 6·5 amperes.

With the normal current the E.M.F. rises on charging from 2·09 to 2·1 volts at the end of half an hour, then falls again to 2·085, rising at the end of four hours to 2·09, and then increases steadily till the end of the eighth hour, when it rises rapidly, finally touching 2·34 volts. On discharging with a normal current the E.M.F. first rises slightly from 1·922 to 1·928, and only again reaches its former value after about one and a half hours; it then falls off very regularly till the end of the discharge. The remarkable rise of both curves should probably be attributed to some kind of secondary action. The resistance was very carefully measured, and was found to be proportional to the charge and to the discharge respectively. The density of the acid was taken regularly, and found to be directly proportional to the state of charge of the cell; it varied from 1·115 to 1·147. This value agrees extremely well with the value arrived at from theoretical calculations, these giving 1·146, from which the observed value differed only by the third decimal place.

A considerable number of further experiments were made with currents exceeding the normal, the detailed results of which are all given in the paper.

## W. WEDDING—ARC LIGHT PHOTOMETRY.

*(Electrotechnische Zeitschrift, Vol. 10, p. 337, 1889.)*

The experiments were mostly carried out with a Siemens differential lamp, worked with a continuous current of 14 amperes, and taking a potential difference of 47 to 52 volts. The standard unit of light employed was the German normal candle, burning with a flame of a height of 45 mm. This may be taken as equal to the English sperm candle. As an intermediate standard of comparison, were used two Albert burners of 25 to 30 candle-power. It would, perhaps, have been preferable to use the amyl-acetate normal lamp of von Hefner-Alteneck. The arc lamp was suspended above the photometer bank, so as to admit readily of the light being measured in all directions, and under various angles.

The following table contains some of the results with a naked light:—

| Angle. | Left. | Angle. | Right. |
|---|---|---|---|
| — | 109 | — | 136 |
| 46° – 26′ | 1,992 | 46° – 25′ | 2,009 |
| 61° – 8′ | 863 | 60° – 42′ | 804 |

The E.M.F. varied between 46·7 and 47·1 volts.

From an inspection of the curves, it appears that the light measured in a horizontal direction is very small; it increases rapidly up to 20 degrees, and then more slowly, till it reaches a maximum at about 42 degrees, and then once more falls off. Soon after passing below 60 degrees the light almost disappears, owing to the shadow cast by the lower carbon, while still further below the lamp some light is thrown down from the crater of the upper carbon. The author gives a formula for calculating the mean spherical illumination from the light measured under the several angles all round the lamp. Experiments were also carried out with three different kinds of globes, and it was found that the mean spherical illumination lost respectively 41, 40, and 53 per cent. of that given by the naked light. By the use of reflectors the diminution of the illumination was reduced to about 32 per cent. The author finally treats of the mathematical calculations for arriving at the best disposition of lamps for illuminating any given area.

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## W. NEGBAUR—EXPERIMENTS ON THE PERMEABILITY OF DIFFERENT SORTS OF IRON AND STEEL.

(*Electrotechnische Zeitschrift, Vol.* 10, *p.* 348, 1889.)

The experiments were carried out on rods of the various metals 80 mm. long and 5 mm. in diameter, twenty-two different kinds being experimented on in all. The observations were made by Gauss's method, with the rods in the first position. The action of the magnetising coil on the magnetometer was nullified by placing a second coil on the opposite side.

Let M equal magnetism in c.g.s. units.
,, H ,, horizontal intensity of the earth's magnetism.
,, R ,, distance of the rod from the magnet in cm.
,, N ,, number of turns on the coil.

If the sectional weight of the rod is taken as about 13 gr., and we assume a specific magnetism of 180 c.g.s. units for the best kinds of iron, the maximum moment of the rod will be about 2,400 c.g.s. units. From Müller's formula

$$M \text{ max.} = 14{\cdot}4 l\, d^2 \text{ arc tan. } \frac{n\sqrt{l}}{5{,}300\, d^{3/2}} \,.\, i\,;$$

and with Waltenhofen's constants the magnetising force for the above value of moment comes out as

$$ni = 7{,}500 \text{ c.g.s. units.}$$

The author's results show that soft iron gives the best results, that steel comes next, and that cast iron takes the third place. A notable exception is Bessemer iron, of which the permeability is at least as great as that of soft iron; it seems, therefore, to be specially useful for the construction of dynamos and of transformers. Amongst the several kinds of steel the best appeared to be Bessemer steel. The greatest measured difference of the kinds tested was 31·6 per cent. of the highest value. Magrini has found that, with a magnetising force of over 50 c.g.s. units, the magnetic moment is independent of the kind of iron. In my experiments I have come to a value of about 60 c.g.s. units. The results have also shown, what has already been remarked by Ewing, that the course of the curve for the magnetism is dependent on the individuality of the iron.

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## Dr. V. WIETLISBACH—THE INDUCTION COILS OF MICROPHONES.

(*Elektrotechnische Zeitschrift, Vol.* 10, *p.* 378, 1889.)

The experiments were carried out on two coils, the particulars of which are tabulated below:—

| | Core. | | Primary. | | | Secondary. | | |
|---|---|---|---|---|---|---|---|---|
| | Length mm. | Diameter mm. | Diameter mm. | Turns. | R. Ohms. | Diameter mm. | Turns. | R. Ohms. |
| Coil No. 1 ... | 67 | 0·4 | 0·5 | 185 | 9·54 | 0·15 | 4,200 | 260 |
| Coil No. 2 ... | 110 | 0·5 | 1·25 | 130 | 9·12 | 0·22 | 4,200 | 128 |

In such coils the most interesting thing is the course of the magnetic induction in the primary coil. In a coil with an iron core the induction may be considered as made up of the electro-dynamical mutual induction of both coils and the magnetic induction of the iron core on the secondary coil. The former being very small, in comparison with the latter, may be neglected. In the experiments the primary coil was connected up in circuit with a battery and an adjustable resistance. The secondary coil in like manner was connected up with a galvanometer and an adjustable resistance. Any alteration in the resistance in the primary circuit caused a corresponding change in the magnetic induction of the core, and hence in the induced current in the secondary coil. Although the primary winding of coil No. 1 had more than half as many turns as, and three times the resistance of, the coil No. 2, yet for equal diminution of the current its induction is $\frac{1}{4}$ to $\frac{1}{3}$ less than that of the coil No. 2. Each ampere-turn of the coil No. 2 induces, therefore, about twice as much as an ampere-turn of coil No. 1, this being on account of the more favourable arrangement of the iron core. Again, the greater resistance of the coil No. 1 makes the microphone less sensitive. Hence on all accounts the coil No. 2 appears to be the more suitable.

## Dr. LEONHARD WEBER—ATMOSPHERIC ELECTRICITY.

(*Electrotechnische Zeitschrift, Vol.* 10, *p.* 387, 1889.)

The article deals with the results obtained during a year's observations. More generally the observations were made by means of kites, but occasionally a balloon was sent up. A great number of tables and curves are given of the measurements of the currents observed.

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## C. GRAWINKEL—CONNECTION OF ACCUMULATORS FOR TELEGRAPH WORK.

(*Electrotechnische Zeitschrift, Vol.* 10, *p.* 446, 1889.)

The article deals with the best arrangement which can be adopted for supplying the very various wants of a telegraph line by means of secondary batteries, so that both open and closed circuit working can be carried on successfully. Without the diagram which accompanies the article, it would be difficult to make the arrangement clearly understood.

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## E. BOUTY and L. POINCARÉ—CONDUCTIVITY OF MOLTEN SALTS.

(*Journal de Physique, Vol.* 8, *p.* 368, 1889.)

## L. POINCARÉ—IDEM.

(*Journal de Physique, Vol.* 8, *p.* 373; *Comptes Rendus, Vol.* 109, *p.* 174, 1889.)

In working at such high temperatures as 350° to 500° there are two great difficulties to be contended with: one is, that if a glass tube is used to contain the salt under experiment at such temperatures, its walls become more or less conductors; the other, that thermo-currents are set up at the junctions of the salt and the electrodes used. The first is more particularly troublesome when an alloy with a specific melting point is used as the source of heat. This defect was got rid of by the simple expedient of using an air-bath as the heating medium. A special arrangement of electrode served to eliminate the second: this consisted of an insulated vessel containing a solution of the salt under experiment, the bottom of the vessel being stopped by a plug of fibrous asbestos in such a manner that while the upper part of the plug was in the solution, the lower part was immersed in the molten salt.

A considerable number of experiments were made with potassium nitrate. Its conductivity may be calculated from the formula

$$c = 0{\cdot}7241\,[1 + 0{\cdot}005\,(t - 350)],$$

and the calculated values agree very well with those obtained from experiment. Working with mixtures of potassium nitrate and sodium nitrate, it was found that the joint conductivity as measured agreed closely with the value calculated from the conductivities of each salt taken separately.

Having observed that silver which had been exposed to the action of a bath of fused nitrate of silver remained unpolarised in fused nitrate of potassium, it

became simple to replace the asbestos electrodes by others of silver without introducing any disturbing influence. The specific conductivity of fused nitrate of silver is found from the formula

$$c = 1{\cdot}233\,[1 + 0{\cdot}0025\,(t - 350)];$$

that is, for temperatures between 280° and 370°. It is seen in this formula that the temperature coefficient is different from that for potassium nitrate and for sodium nitrate. Experimenting on ammonium nitrate, the formula was found to be

$$c = 0{\cdot}4\,[1 + 0{\cdot}0073\,(t - 200)],$$

which once more gives a different coefficient. If, however, the temperature coefficient be multiplied in each case by the density of the fused salt, a constant number is obtained, as shown in the following table:—

| Name of Salt. | Temperature. | Density. | Coefficient. | Product. |
|---|---|---|---|---|
| Potassium Nitrate... ... | 350° | 1·84 | 0·005 | 920 |
| Sodium ,, ... ... | 350° | 1·84 | 0·005 | 920 |
| Silver ,, ... ... | 350° | 3·9 | 0·0025 | 975 |
| Ammonium ,, ... ... | 200° | 1·36 | 0·0073 | 971 |

Taking into account the great difficulty there is in measuring exactly the densities at these high temperatures, it will be found that the coefficients are in inverse ratio to the densities of the corresponding salts. It is also to be remarked that the molecular densities—0·0397 for potassium nitrate, and 0·0420 for ammonium nitrate, supposing them both brought to a similar temperature, *e.g.*, 350°—are very close to each other; these two quantities differ, however, considerably from the molecular conductivities—0·0537 and 0·602—of silver nitrate and sodium nitrate respectively, which do not differ much. It is known that in the case of solutions the two former salts are normal, while the two others are abnormal.

There is a limit to these experiments, when the temperatures have reached that of the fusion of glass; this may be got over by the use of tubes of porcelain. Experimenting on chlorides, the author made use of such tubes, and the silver electrodes before alluded to; these did not polarise to any extent, and, moreover, the amount of polarisation is quite regular. The temperatures were measured by means of a platinum-rhodium thermo-couple. The following formulæ represent the variations for potassium and sodium chlorides respectively:—

$$c = 1{\cdot}80\,[1 + 0{\cdot}0066\,(t - 750)] \text{ between } 700° \text{ and } 800°;$$
$$c = 3{\cdot}15\,[1 + 0{\cdot}0064\,(t - 750)] \text{ between } 715° \text{ and } 800°.$$

It will be noticed that the coefficients of variation are the same for both salts. The densities being measured at about 750°, it was found that they were practically the same, viz., 1·65. The product—1,070—is not very different from that of the nitrates given above, viz., 920. The molecular conductivities are respectively 0·0813 and 0·112 for potassium chloride and sodium chloride; and it is interesting to remark that the ratio of the conductivities is almost the same as the ratio—0·67—of the molecular conductivities of the nitrates of potassium and sodium.

When solidification takes place in the electrolyte, the resistance increases

regularly. At the moment when the whole of the salt becomes solid the conductivity falls to 1-500th of that of the fused salt at the same temperature; then it decreases very rapidly, the coefficient of variation being 100 times what it was before. In order to gain some approximate idea of the effect of the porcelain tube, some measurements were taken of its conductivity, the results being given by the formula

$$c = 10\,(0{\cdot}0573\,t^2 + 0{\cdot}0000125\,t - 16{\cdot}30).$$

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## M. DEPREZ—ELECTRIC TRANSMISSION OF POWER AT BOURGANEUF.

(*Comptes Rendus, Vol.* 109, *p.* 455, 1889.)

The source of power is a waterfall situated at "Les Jarrauds," on the Maulde, a distance of 14 kilometres from Bourganeuf. The water actuates a horizontal turbine which is capable of giving out a maximum power of 180 H.P. at a speed of 150 revolutions per minute. From the turbine the power is transmitted to the dynamo on the floor above by means of two belts, there being two pulleys on the dynamo. The generator has two armatures, both on one axle; they are wound with wire 2·2 mm. in diameter, and have each a resistance of 2 ohms. The section of each wire is 3·8 square mm., thus giving a section of 7·6 square mm. for the current, which may be 35 amperes without excessive heating. The two armatures are joined up in series, and can give an E.M.F. of 5 to 5·5 volts for each revolution per minute.

The electro-magnets are separately excited by a current of 20 amperes and 90 volts.

The line is metallic, and consists of a siliceous bronze wire 5 mm. in diameter, carried on porcelain insulators. The total resistance of the line is 28 ohms; the insulation is found to be very perfect, even in wet weather.

The motor is of exactly the same type as the generator, and is also separately excited, current being taken either from one of the lighting dynamos or from a battery of accumulators. The motor is used to drive the dynamos which supply current for lighting the town; these are Gramme dynamos giving an output of 250 amperes and 120 volts.

At some preliminary experiments made with the generator and motor, before they were installed at Bourganeuf, with an artificial line of 30 ohms, the lighting dynamos gave collectively an output of 385 amperes and 112 volts when the generator was developing 22 amperes and 3,750 volts. In a second experiment, with 25 ohms resistance in the line, the lighting dynamos gave 376 amperes and 115 volts when the generator was giving an output of 20 amperes and 3,550 volts. The lighting dynamos used having a commercial efficiency of 80 per cent., whilst the Deprez dynamos are said by the author to have a commercial efficiency of 90 per cent., the results actually obtained might be improved upon; and there is no doubt that a power of 60 H.P. might be obtained at Bourganeuf with 100 H.P. at the turbine at "Les Jarrauds."

# LIST OF ARTICLES

RELATING TO

# ELECTRICITY AND MAGNETISM

**Appearing in some of the principal Technical Journals during the Months of JULY, AUGUST, SEPTEMBER, and OCTOBER, 1889.**

## I.—BATTERIES AND ACCUMULATORS.

E. Dieudonné—Gendron's New Battery.—*Lum. El.*, vol. 33, p. 27, 1889.

B. Kolbé—Potential Difference of Galvanic Cells.—*Beiblätter*, vol. 13, p. 396, 1889.

C. Heim—Effect of the Density of the Acid on the Capacity of Accumulators.—*Beiblätter*, vol. 13, p. 409, 1889.

W. Kohlrausch and C. Heim—Researches on Accumulators for Station Work.—*El. Zeit.*, vol. 10, p. 327, 1889.

C. Grawinkel—Coupling up Accumulators for Telegraph Work.—*El. Zeit.*, vol. 10, p. 446, 1889.

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## II.—DYNAMOS AND MOTORS.

R. V. Picou—Various Dynamo Armatures.—*Lum. El.*, vol. 33, p. 7, 1889.

G. Richard—Details of Dynamo Construction.—*Lum. El.*, vol. 33, p. 519, 1889.

C. Reignier—Secondary Phenomena of Induction in Dynamos.—*Lum. El.*, vol. 33, p. 605, 1889.

E. Meylan—Rechniewski's Dynamo.—*Bull. Soc. Int.*, vol. 6, p. 288, 1889.

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## III.—ELECTRO-CHEMISTRY AND ELECTRO-METALLURGY.

S. Arrhenius—Electrolytic Dissociation *v.* Hydration.—*Phil. Mag.*, vol. 28, p. 30, 1889.

E. Duter—Electrolysis of Distilled Water.—*C. R.*, vol. 109, p. 108, 1889.

N. Piltschikoff—Variations in Current Strength during Electrolysis.—*C. R.* vol. 109, p. 135, 1889.

L. Poincarré—Conductivity of Electrolytes at High Temperatures.—*C. R.*, vol. 109, p. 174, 1889.

G. Richard—Electro-Metallurgy of Aluminium.—*Lum. El.*, vol. 33, p. 151, 1889.

H. Ponthière—Energy Absorbed in Electro-metallurgical Processes.—*Lum. El.*, vol. 33, p. 251, 1889.

S. Arrhenius—Modern Theory of Electrolytes.—*Lum. El.*, vol. 33, pp. 401, 458 513, 563, 1889.

E. Cohn—Absorption of Electric Oscillations in Electrolytes.—*Ann.*, vol. 38, p. 217, 1889.

W. Ostwald and W. Nernst—Free Ions.—*Beiblätter*, vol. 13, p. 393, 1889.

M. Loeb and W. Nernst—Conductivity of some Silver Salts.—*Beiblätter*, vol. 13, p. 395, 1889.

A. Sokoloff—Electrical Oscillations in Electrolytes.—*Beiblätter*, vol. 13, p. 402, 1889.

B. Nebel—Electro-Crystallisation of Copper.—*Beiblätter*, vol. 13, p. 536, 1889.

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## IV.—ELECTRIC LIGHT.

E. W. Smith—Shunt Transformer.—*Phil. Mag.*, vol. 28, p. 132, 1889.

G. Richard—Details of Glow Lamps.—*Lum. El.*, vol. 33, p. 10, 1889.

G. Richard—Arc Lamps.—*Lum. El.*, vol. 33, p. 61, 1889.

E. Dieudonné—Clerc's Electric Meter.—*Lum. El.*, vol. 33, p. 155, 1889.

A. Palaz—Photometry.—*Lum. El.*, vol. 33, p. 255, 1889.

E. Dieudonné—The Electric Light at the St. Lazare Railway Station.—*Lum. El.*, vol. 34, p. 7, 1889.

A. Palaz—New Form of Violle's Standard of Light.—*Lum. El.*, vol. 34, p. 51, 1889.

H. Kayser and C. Runge—Channel Spectrum of Carbon in the Electric Arc.—*Ann.*, vol. 38, p. 80, 1889.

E. v. Gothard—Photographing the Electric Spark.—*Beiblätter*, vol. 13, p. 422, 1889.

— Slotte—Dependence of Candle-Power of Glow Lamps on the Current Strength.—*Beiblätter*, vol. 13, p. 821, 1889.

A. Hillairet—Illumination produced by Arc Lamps in a Straight Line.—*Bull. Soc. Int.*, vol. 6, p. 327, 1889.

W. Wedding—Photometry of Arc Lamps.—*El. Zeit.*, vol. 10, p. 337, 1889.

K. Wiesner—Laying Underground Cables.—*El. Zeit.*, vol. 10, p. 357, 1889.

Dr. T. Erhard—Bunsen Photometer.—*El. Zeit.*, vol. 10, p. 377, 1889.

R. Rühlmann—Alternate Currents *v.* Continuous Currents for Central Stations.—*El. Zeit.*, vol. 10, p. 397, 1889.

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## V.—ELECTRIC POWER.

M. Leblanc—Transmission of Power by Alternate Currents.—*C. R.*, vol. 109, p. 172, 1889.

M. Deprez—Results obtained at the Bourganeuf Installation.—*C. R.*, vol. 109, pp. 394, 455, 1889.

M. Leblanc—Distribution of Energy by Electricity.—*Lum. El.*, vol. 33, pp. 101, 220, 263, 1889.

P. Marcillac—Electric Despatch Boat.—*Lum. El.*, vol. 33, p. 268, 1889.

M. Leblanc—Transmission of Power by Alternate Currents.—*Lum. El.*, vol. 33, pp. 358, 551.

E. Dieudonné—Electric Hoisting Gear.—*Lum. El.*, vol. 33, p. 451, 1889.

G. Richard—Electric Railways and Tramways.—*Lum. El.*, vol. 33, p. 465, 1889.

## VI.—MAGNETISM AND ELECTRO-MAGNETISM.

J. J. THOMSON—Magnetic Effects produced by Motion in an Electric Field.—*Phil. Mag.*, vol. 28, p. 1, 1889.

A. TANAKADATÉ—Thermal Effects produced by Reversals of Magnetisation in Soft Iron.—*Phil. Mag.*, vol. 28, p. 207, 1889.

L. KUSMINSKI-LEDOCHOWSKI—Action of a Uniform Field on a Magnetic Body.—*J. de Ph.*, vol. 8, p. 319, 1889.

P. JANET—Heat of Combustion of Iron in a Magnetic Field, and Thermo-magnetic Phenomena.—*J. de Ph.*, vol. 8, p. 312, 1889.

L. KÜLP—Magnetic Coercive Force.—*Beiblätter*, vol. 13, p. 551, 1889.

P. JOUBIN—Dispersion of the Magnetic Rotation of the Plane of Polarisation of Light.—*Beiblätter*, vol. 13, p. 554, 1889.

W. NEGBAUR—Permeability of various Kinds of Iron and Steel.—*El. Zeit.*, vol. 10, p. 348, 1889.

## VII.—MEASUREMENTS AND MEASURING INSTRUMENTS.

C. V. BOYS—Quartz as an Insulator.—*Phil. Mag.*, vol. 28, p. 14, 1889.

C. T. HUTCHINSON and G. WILKES—Comparison of the Mercury Unit with the B.A. Unit.—*Phil. Mag.*, vol. 28, p. 17, 1889.

L. DUNCAN, G. WILKES, and C. T. HUTCHINSON — Determination of the B.A. Unit in Absolute Measure by Lorenz's Method.—*Phil. Mag.*, vol. 28, p. 98, 1889.

E. VAN AUBEL—Electrical Resistance of Bismuth.—*Phil. Mag.*, vol. 28, p. 332, 1889.

T. MOUREAUX—Causes of certain Errors of Magnetographs.—*C. R.*, vol. 109, p. 272, 1889.

E. BOUTY and L. POINCARRÉ—New Method of Measuring the Resistance of Molten Salts.—*J. de Ph.*, vol. 8, p. 368, 1889.

— PELLISSIER—Electrometers.—*Lum. El.*, vol. 33, pp. 16, 73, 111, 178, 1889.

W. DE FONVIELLE—Registering Instruments on the Eiffel Tower.—*Lum. El.*, vol. 33, p. 422, 1889.

C. DECHARME—New Galvanometer.—*Lum. El.*, vol. 33, p. 456, 1889.

E. DIEUDONNÉ—Miot's Magnetic Inductometer.—*Lum. El.*, vol. 33, p. 510, 1889.

B. KOLBE—Simple Form of Electrometer.—*Beiblätter*, vol. 13, p. 521, 1889.

## VIII.—RAILWAY APPLIANCES.

M. COSSMANN—Railway Appliances at the Paris Exhibition.—*Lum. El.*, vol. 33, pp. 201, 365, 501, 1889.

G. DUMONT and — POSTEL-VINAY—Motor for Working Railway Signals.—*Bull. Soc. Int.*, vol. 6, p. 332, 1889.

Dr. A. TOBLER—Siemens & Halske's Improved Block System.—*El. Zeit.*, vol. 10, pp. 405, 428, 1889.

## IX.—STATIC AND ATMOSPHERIC ELECTRICITY.

G. FULLER—Water-Spray Influence Machine.—*Phil. Mag.*, vol. 28, p. 42, 1889.

H. H. HOPPERT—Intermittent Lightning Flashes.—*Phil. Mag.*, vol. 28, p. 106, 1889.

D. COLLADON—Duration of Lightning Flashes.—*C. R.*, vol. 109, p. 12, 1889.

E. L. TROUVELOT—Duration of Lightning Flashes.—*Lum. El.*, vol. 33, p. 70, 1889.

J. FREYBERG—Determination of the Potential Difference necessary to Produce Sparks in Air.—*Ann.*, vol. 38, p. 231, 1889.

G. JAUMANN—Silent Discharge in Air at Normal Pressure.—*Beiblätter*, vol. 13, p. 417, 1889.

F. EXNER—Atmospheric Electricity.—*Beiblätter*, vol. 13, p. 427, 1889.

K. A. BRANDER—Earth Currents.—*Beiblätter*, vol. 13, p. 734, 1889.

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Dr. L. WEBER—Experiments on Atmospheric Electricity.—*El. Zeit.*, vol. 10, p. 387, 1889.

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MSM—Storms on German Land Lines in 1888.— *El. Zeit.*, vol. 10, pp. 463, 482, 1889.

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## X.—TELEGRAPHY AND TELEPHONY.

E. MERCADIER—Intensity of Telephonic Effects.—*Ann. Tel.*, vol. 16, p. 115, 1889.

— LAGARDE—Wires of an Alloy of Copper and Magnesium.—*Ann. Tel.*, vol. 16, p. 124, 1889.

— VASCHY—Propagation of a Current in a Telegraph Line.—*Ann. Tel.*, vol. 16, p. 135, 1889.

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E. ZETSCHE—Enzmann's Telephone Relay.—*Lum. El.*, vol. 33, p. 326, 1889.

P. SAMUEL—New Multiple Printing Telegraph.—*Lum. El.*, vol. 33, pp. 558, 611, 1889.

C. JACQUIN—Acceleration of Telegraphic Transmission by Using a Condenser.—*Lum. El.*, vol. 34, pp. 27, 66, 1889.

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— HIERONIMUS—Synchronous Printing Telegraph.—*El. Zeit.*, vol. 10, p. 335, 1889.

Dr. V. WIETLISBACH—The Induction Coils of Microphones.—*El. Zeit.*, vol. 10, p. 378, 1889.

R. PETSCH—Telephonic Cables.—*El. Zeit.*, vol. 10, p. 381, 1889.

K. WIESMER—v. Rysselberghe's Multiple Telegraph.—*El. Zeit.*, vol. 10, p. 430, 1889.

H. DISCHER—Theory of Duplex Telegraphy.—*El. Zeit.*, vol. 10, p. 448, 1889.

## XI.—THEORY.

O. J. Lodge and J. L. Howard—Electric Radiation and its Concentration by means of Lenses.—*Phil. Mag.*, vol. 28, p. 48, 1889.

J. T. Bottomley—Expansion with Rise of Temperature of Wires under Pulling Stress.—*Phil. Mag.*, vol. 28, p. 94, 1889.

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E. B. Rose—Relation of Electrostatic to Electro-magnetic System.—*Phil. Mag.*, vol. 28, p. 315, 1889.

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— Krouchkoll—Electro-capillary Phenomena.—*J. de Ph.*, vol. 8, p. 472, 1889.

F. Larroque—Residual Charge.—*Lum. El.*, vol. 33, p. 67, 1889.

M. Leblanc and P. H. Ledeboer—General Equations of the Motion of Electricity.—*Lum. El.*, vol. 33, pp. 157, 204, 273, 417, 504, 614, 1889.

H. Righi—Electrical Phenomena produced by Radiation.—*Lum. El.*, vol. 33, p. 160, 1889.

J. Elster and H. Geitel—Motion of Electricity in Rarefied Gases.—*Ann.*, vol. 38, p. 27, 1889.

E. Cohn—Specific Inductive Capacity of Water.—*Ann.*, vol. 38, p. 42, 1889.

F. Braun—Currents of Deformation.—*Ann.*, vol. 38, p. 53, 1889.

A. Winkelmann—Determination of Specific Inductive Capacity by means of a Telephone.—*Ann.*, vol. 38, p. 161, 1889.

T. Homen—Conductivity of Gases.—*Ann.*, vol. 38, p. 172, 1889.

K. Wesendonck—The Differences of the Two Kinds of Electricity.—*Ann.*, vol. 38, p. 222, 1889.

O. Schumann—Cyclic Change of Electrical Conductivity.—*Ann.*, vol. 38, p. 256, 1889.

J. Debsaulx—Potential Difference of Dielectric Media along the Lines of Force.—*Beiblätter*, vol. 13, p. 390, 1889.

K. Steinmetz—Apparent Resistance of Conductors Traversed by a Current.—*Beiblätter*, vol. 13, p. 391, 1889.

H. Götz and A. Kurz—Volta's Fundamental Experiment.—*Beiblätter*, vol. 13, p. 399.

P. H. Dojes—The Equation for the Electric Current.—*Beiblätter*, vol. 13, p. 399 1889.

V. Dvorak—Action of Self-Induction in Electro-magnetic Current Interruption.—*Beiblätter*, vol. 13, p. 408, 1889.

— Naccari—Action of the Electric Spark on Conductors.—*Beiblätter*, vol. 13, p. 421, 1889.

M. Thoma—Dependence of the Electro-motive Position of Palladium on the Amount of Hydrogen Occluded.—*Beiblätter*, vol. 13, p. 529, 1889.

O. TROJE—Analysis of Surface Resistance.—*Beiblätter*, vol. 13, p. 543, 1889.

G. LEON—Equivalence of an Infinitely Small Current and a Small Magnet.—*Beiblätter*, vol. 13, p. 548, 1889.

L. BOLTZMANN—Theory of Hall's Electro-magnetic Phenomena.—*Beiblätter*, vol. 13, p. 548, 1889.

A. v. WALTENHOFEN—Meaning of various Formulæ of Magnetisation.—*Beiblätter*, vol. 13, p. 551, 1889.

P. CULMANN—Sparks on Breaking Circuit.—*Beiblätter*, vol. 13, p. 562, 1889.

K. ASPERN—Discharges from Flames and Points.—*Beiblätter*, vol. 13, p. 563, 1889.

A. RIGHI—Discharges produced by Radiation.—*Beiblätter*, vol. 13, p. 566, 1889.

F. UPPENBORN—Specific Inductive Capacity of some Kinds of Paper.—*Beiblätter*, vol. 13, p. 711, 1889.

G. LIEBEN—Experiments on Electrical Figures on Plates Rendered Sensitive to Light.—*Beiblätter*, vol. 13, p. 370, 1889.

M. HOOR—Effect of Ultra-Violet Rays.—*Beiblätter*, vol. 13, p. 731, 1889.

— JOUBERT—Hertz's Experiments.—*Beiblätter*, vol. 6, p. 318.

C. GRAWINKEL—Geometric Solutions of some Problems on Coupling up Batteries.—*El. Zeit.*, vol. 13, p. 333, 1889.

O. FRÖLICH—New Method of Observing Curves of Oscillation.—*El. Zeit.*, vol. 13, pp. 345, 369, 1889.

R. SAUER—The Relation between the Resistance of Crossed Electric Waves in Conducting Surfaces.—*El. Zeit.*, vol. 10, p. 351, 1889.

## XII.—VARIOUS APPLIANCES.

E. DIEUDONNÉ—Electrically Driven Tools, &c.—*Lum. El.*, vol. 33, p. 301, 1889.

G. DUMONT and A. LEPAUTE—Synchronising Clocks.—*Bull. Soc. Int.*, vol. 6, p. 310, 1889.

E. MERCADIER—The Phonograph and Graphophone.—*Bull. Soc. Int.*, vol. 6, p. 356, 1889.

# NOTICE.

1. The Society's Library is open to members of all Scientific Bodies, and (on application to the Secretary) to the Public generally.

2. The Library is open (except from the 14th August to the 16th September) daily between the hours of 11.0 a.m. and 8.0 p.m., except on Thursdays, and on Saturdays, when it closes at 2.0 p.m.

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*An Index, compiled by the late Librarian, to the first ten volumes of the Journal can be had on application to the Secretary, or to Messrs. E. and F. N. Spon, 125, Strand, W.C. Price Two Shillings and Sixpence.*

JOURNAL

OF THE

# Institution of Electrical Engineers.

*Founded* 1871. *Incorporated* 1883.

| VOL. XVIII. | 1889. | No. 83. |
|---|---|---|

The Eighteenth Annual General Meeting of the Institution was held at the Institution of Civil Engineers, 25, Great George Street, Westminster, on Thursday evening, December 12th, 1889—Dr. JOHN HOPKINSON, M.A., F.R.S., Vice-President, in the Chair.

The minutes of the Ordinary General Meeting of November 28th were read and confirmed.

Donations to the Library were announced as having been received since the last meeting from Messrs. Macmillan & Co.; Dr. J. Hopkinson, Member, V.P.; Mr. G. W. de Tunzelmann; and Sir William Thomson, President; to whom the thanks of the meeting were unanimously accorded.

The SECRETARY stated that, in addition to the books that had been presented to the Library, he had to announce that Lady Bright had been good enough to present to the Institution a cast of the bust of the late Sir Charles Bright, executed by Count Gleichen (now Prince Victor Hohenlohe), R.A., and which was exhibited in the Academy some years ago.

Upon the motion of the CHAIRMAN, the thanks of the meeting were unanimously voted to Lady Bright for her kind donation.

The CHAIRMAN announced that the ballot boxes would remain open till 8.30 p.m.

Mr. T. Buckney, Mr. G. Driver, Mr. C. T. Fleetwood, and Dr. R. M. Walmsley were appointed Scrutineers.

The SECRETARY then read the following Report of the Council:—

## REPORT OF THE COUNCIL.

The Council have the satisfaction of reporting that the number of new members elected into the Institution during the present year exceeds that of last year, which was already much beyond the average.

12 Foreign Members, 31 Members, 109 Associates, and 31 Students, making a total of 183, have been added to the register, and 39 candidates have been approved for ballot at the first meeting in January next.

19 Associates have been transferred to the class of Members, and 24 Students to the class of Associates during the year.

Our losses by death have fortunately been fewer than those of last year, comprising 1 Foreign Member—Mr. Holst, of the Great Northern Telegraph Company; 10 Members, among whom are included Mr. C. H. B. Patey, one of the secretaries to the Post Office; Dr. Warren De la Rue, whose valuable experimental researches in electrical science are so well known; Major-General Murray, R.A.; Mr. Walter Hall; and Mr. Henry Sach, Superintendent of Telegraphs to the Great Eastern Railway—the two last named having been among the oldest members of the Institution; and 5 Associates.

The resignations, which have also been considerably fewer than those of last year, include 3 Foreign Members, 5 Members, 10 Associates, and 1 Student.

The Institution continues to enjoy the great privilege of holding its General Meetings in the lecture hall of the Institution of Civil Engineers, an advantage which is all the more valuable in that the attendance at these meetings continues to increase very considerably, averaging nearly 200, and in some cases having reached over 300 persons.

The papers read during the session have been mostly of a practical nature, and have extended over different branches of electrical engineering, as will be seen by the following list. Several of these have given rise to important and valuable discussions.

## LIST OF PAPERS READ BEFORE THE INSTITUTION DURING THE YEAR 1889.

| Date. | Title. | Author. |
|---|---|---|
| Jan. 24. | The Insulation Resistance of Electric Light Installations ... ... ... | Prof. A. Jamieson, F.R.S.E., M.Inst.C.E., Member. |
| Feb. 21. | On certain Phenomena connected with Imperfect Earth in Telegraph Circuits | A. E. Kennelly, Associate. |
| ,, 28. | Some Electric Lighting Central Stations in Europe, and their Lessons ... ... | Prof. Geo. Forbes, M.A., F.R.SS. (L.&E.), Member. |
| Mar. 28. | Laboratory Notes on Alternate-Current Circuits ... ... ... ... ... | Prof. W. E. Ayrton, F.R.S., V.P., and Prof. John Perry, F.R.S., Member. |
| ,, 28. | On the Disturbances arising from the use of "Earth" for Electric Lighting Purposes ... ... ... ... ... | W. H. Preece, F.R.S., Past-President. |
| April 11. | Underground Conduits and Electrical Conductors ... ... ... ... | John B. Verity, Member. |
| ,, 25. | On Lightning, Lightning Conductors, and Lightning Protectors ... ... | Dr. Oliver J. Lodge, F.R.S., Member. |
| May 23. | On the Security against Disturbances of Ships' Compasses by Electric Lighting Appliances ... ... ... ... | Sir William Thomson, D.C.L., LL.D., F.R.SS. (L. & E.), President. |
| ,, 23. | On Alternate-Current Working ... | W. M. Mordey, Member. |
| Nov. 4. | On the Lighting of the Melbourne Centennial International Exhibition ... | K. L. Murray, Member. |
| ,, 28. | Electrical Engineering in America ... | G. L. Addenbrooke, Associate. |

The Council have awarded the annual premiums in respect to papers read during the twelve months ending the 31st of May last, as follows:—

*The Institution Premium*, value £10, to W. M. Mordey, Member, for his paper on "Alternate-Current Working."

*The Paris Electrical Exhibition Premium*, value £5, to Dr. Oliver Lodge, F.R.S., Member, for his paper on "Lightning, "Lightning Conductors, and Lightning Protectors."

*The Fahie Premium*, value £5, to A. E. Kennelly, Associate, for his paper on "Certain Phenomena connected with Imperfect "Earth in Telegraph Circuits."

### The Board of Trade.

At the commencement of the year the Council were consulted by the Board of Trade on several technical points in connection with the regulation of the supply of electric energy.

A Committee was at once appointed, who carefully considered the several matters in question, and their recommendations, which were fully approved by the Council, were duly forwarded to the Board of Trade, and have substantially been adopted by that department of the Government.

The most important matter discussed was the question of regulations for aerial conductors, which alone occupied no less than nine meetings of the Committee.

Your Council being convinced that the establishment of a standardising laboratory by the Board of Trade would be found indispensable for the proper administration of the Electric Lighting Act, and having received a communication from the Electrical Trades Section of the London Chamber of Commerce, calling attention to the importance, from an industrial point of view, of the standardising of electric measurements, reappointed the Standardising Committee, for the purpose of considering the matter, and of drawing up a scheme for such a laboratory, with estimate of the cost.

After this had been done, two joint meetings of the Committee and a consultative Committee of the Electrical Trades Section of the London Chamber of Commerce were held, and the draft scheme, as then finally approved, was adopted by the Council, and forwarded by them to the Board of Trade.

A joint deputation of the Institution and of the London Chamber of Commerce subsequently had an interview with the President of the Board, to urge the adoption of the scheme.

There is every reason to hope that this recommendation, which has already been partially adopted, will eventually be fully carried out, and thus render unnecessary any other standardising establishment, which, however well organised, and under whatever influential auspices it might be created, could scarcely expect to meet with the general recognition which a Government establishment must necessarily command.

## Paris International Exhibition.

The Council having been urgently requested by Colonel Laussedat, President of the Organising Committee of the Exhibition of Historical Apparatus at the Paris Exhibition (Exposition Retrospective du Travail), to obtain for the electrical section the loan of apparatus representative of the progress of applied electrical science in Great Britain, took measures accordingly, and by the kindness of the Postmaster-General, the Managers of the Royal Institution, Sir William Thomson (President), Mr. Latimer Clark (Past-President), and the School of Electrical Engineering, were enabled to send for exhibition, in the name of the Institution, a small but interesting collection of instruments and apparatus.

Your President has received from Colonel Laussedat a letter warmly thanking the Institution for the valuable addition made by them to the Exhibition of which he had charge.

## Paris Meeting.

Knowing that a considerable number of the members would be visiting Paris during the time appointed for the sitting of the Electrical Congress, the Council gladly accepted the courteous invitation of the Société Internationale des Électriciens, communicated by their President, Colonel Sèbert, to hold a joint meeting with the Société.

The meeting took place on the 23rd August, your President being, by the request of the Société, in the chair, and addresses of warm welcome to our members were delivered by Colonel Sèbert, the President of the Société, and by Monsieur E. Mascart, the President of the Electrical Congress, to which Sir William Thomson replied in an interesting speech.

Monsieur De Chardonnet subsequently explained his mode of manufacturing artificial silk, and Monsieur J. Carpentier his electrical apparatus, called respectively the "Mélographe," the "Mélotrope," and the "Batteur de Mésure." The phonograph and a variety of interesting electrical exhibits were on view.

During the ensuing week, the members were invited to visit the following establishments:—

The Works of Messrs. Sautter Lemonier,

The Works of La Société Générale des Téléphones,

The Laboratoire Centrale de l'Électricité.

*Electric Lighting Stations*—Central Station Palais Royal, Grand Opera.

The following highly-interesting lectures were also most kindly given, viz.:—"On, and repetition of Hertz's Experiments," by Professor Joubert, in the Laboratoire Centrale de l'Électricité;

"On, and repetition of Elihu Thomson's Experiments," by Monsieur Abdank Abakanowicz, in the Industrial Court of the American Section of the Exhibition.

By the great kindness of Monsieur E. Mascart, a special ascent of the Eiffel Tower was arranged for the members.

The members were also most hospitably entertained at an evening reception by the members then in Paris of the British Executive Commission, viz., Messrs. Dredge, Preece, and Trueman Wood, given at the elegant pavilion of the Commission in the Exhibition grounds.

The success of the Paris meeting was largely due to the kind exertions of Mr. John Aylmer, our Local Honorary Secretary.

## EDINBURGH EXHIBITION, 1890.

The Council lately received a deputation of gentlemen officially connected with the Exhibition of Electrical Engineering and Inventions to be held in Edinburgh next year; and, as there appears to be every probability of there being much to interest our members, it has been decided that a meeting of the Institution shall be held in Edinburgh, either shortly before or shortly after the meeting of the British Association at Leeds.

The Council have, at the request of the Executive Committee of the Exhibition, given their views on the subject of the system of presenting awards, and on other points affecting the practical advantages to be gained by such Exhibitions.

### Reporting of Discussions.

In the belief that the verbatim reporting of the proceedings of the Institution's Ordinary General Meetings was tending to encourage discursiveness on the part of many of the speakers—which was neither desirable in itself nor in accordance with the wishes of the majority, as evidenced by the ready adoption of the ten minutes rule—the Council decided to prohibit the reporting of the discussions in the technical journals, and to furnish to the latter an official abstract of each evening's discussion, prepared by a gentleman specially engaged for the purpose.

This arrangement has been loyally accepted and adopted by the Technical Press.

### Annual Conversazione.

The Conversazione of the President, on the 24th May, took place in the galleries of the Royal Institute of Painters in Waters Colours, and was very largely attended.

### Annual Dinner.

The success which attended the Institution Dinner held on the 4th November, when the members were honoured by the presence of the Prime Minister and other distinguished guests, has decided the Council to consider it as the first *annual* Dinner of the Institution of Electrical Engineers.

### Mode of Electing the Council.

As already made known to you by the Secretary's circular of the 3rd instant, the result of the Council's invitation to all Members and Associates, both at home and abroad, has been such as to lead them to revert to the original system of annually nominating for election on the Council only a sufficient number of Members and Associates to fill the occurring vacancies. They

trust that this decision, being that of the majority, will be accepted without further demur.

With the view of rendering the rule which requires the annual retirement of three members of the Council more effective in its intended purpose, viz., a complete although gradual change in the composition of that body, it has been resolved, upon the initiation of Professor George Forbes, that two at least of such retiring members each year shall be those who have served during the longest time on the Council. Professor Forbes and Dr. Fleming have accordingly retired. Mr. Stroh, having the same object in view, had already pressed his resignation on the Council, which has been accepted.

### Financial Position.

The finances of the Institution continue in a sound condition, the increase in the number of members more than compensating for the increased expenditure incurred in several respects by the wider field of its operations.

## THE LIBRARY.

### REPORT OF THE SECRETARY.

I beg to report that the accessions to the Library during the year, as reported from time to time in the Journal of the Institution, number 81, by far the greater majority of which have been presentations, for I have again the pleasure of recording the liberality of authors and publishers in responding to my applications; in fact, I have not yet in any one case been met with a refusal.

The Institution continues to receive, by the kindness of H.M. Commissioners of Patents, the specifications of all electrical patents. The number of patents applied for this year, up to the 30th November, was 19,295, of which 1,109, or nearly 5·75 per cent., were electrical.

The number of periodicals and transactions received regularly by the Institution is slightly increased since last year. A list of these is appended.

The number of visitors to the Library during the year has been 238, of whom 34 were non-members.

F. H. WEBB,
*Secretary and Librarian.*

*12th December*, 1889.

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## *APPENDIX TO SECRETARY'S REPORT.*

### TRANSACTIONS, PROCEEDINGS, &c., RECEIVED BY THE SOCIETY.

**ENGLISH.**

Asiatic Society of Bengal, Journal and Proceedings.
Cambridge Philosophical Society, Proceedings.
Greenwich Magnetical and Meteorological Observations.
Institute of Patent Agents, Transactions.
Institution of Civil Engineers, Proceedings.
Institution of Mechanical Engineers, Proceedings.
Iron and Steel Institute, Proceedings.
Liverpool Engineering Society, Proceedings.
Physical Society, Proceedings.
Royal Dublin Society, Transactions and Proceedings.
Royal Engineers' Institute, Proceedings.
Royal Institution, Proceedings.
Royal Meteorological Society, Proceedings.
*Royal Society, Philosophical Transactions of.
Royal United Service Institution, Proceedings.
Society of Arts, Journal.
Society of Chemical Industry, Journal.
Society of Engineers, Proceedings.
University College Calendar.

**AMERICAN.**

American Academy of Science and Arts, Proceedings.
American Institute of Electrical Engineers, Transactions.
Canadian Society of Civil Engineers, Transactions.
Franklin Institute, Journal of.
John Hopkins University Circulars.
Library Bulletin of Cornell University.
Ordnance Department of the United States, Notes.
Smithsonian Institution Reports.

**FRENCH.**

L'Académie des Sciences, Comptes Rendus Hebdomadaires des Séances de.
Société Belge d'Électriciens, Bulletin de la.
Société Française de Physique, Séances de la.
Société des Ingénieurs Civils, Mémoires.
Société Internationale des Électriciens, Bulletin de la.
Société Scientifique Industrielle de Marseille, Bulletin de la.



* Presented by Professor D. E. Hughes, F.R.S. (Past-President).

# LIST OF PERIODICALS RECEIVED BY THE SOCIETY.

## ENGLISH.

Electrical Engineer.
Electrical Plant.
Electrician.
Engineer.
Engineering.
English Mechanic and World of Science.
Illustrated Official Journal, Patents.
Indian Engineer.
Industries.
Invention.
Mechanical Progress.
Military Telegraph Bulletin.
Nature.
Philosophical Magazine.
Telegraphic Journal and Electrical Review.
Telephone, The.

## AMERICAN.

Electrical Engineer.
Electrical Review.
Electrical World.
Gas and Water Review and Journal of Electric Lighting.
Journal of the Telegraph.
Science.
Scientific American.
United States Patent Office, Official Gazette of.

## FRENCH.

Annales Télégraphiques.
L'Électricité.
Journal de Physique.
Journal Télégraphique.
La Lumière Électrique.
L'Électricien.
Revue Internationale de l'Électricité et de ses Applications

## GERMAN.

Annalen der Physik und Chemie.
Beiblätter zu den Annalen der Physik und Chemie.
Centralblatt für Elektrotechnik.
Electrotechnischer Anzeiger.
Electrotechnische Zeitschrift.
Verhandlungen des Vereins zur Beförderung des Gewerbfleisses.
Zeitschrift für Elektrotechnik.
Zeitschrift für Instrumentkunde.

**ITALIAN.**

Giornale del Genio Civile.
Il Telegrafista.

**SPANISH.**

Ingeniero y Ferretero Español y sud Americano.

The CHAIRMAN: Gentlemen,—I think you will all agree that the past year has been a successful one—the first year of the "Institution of Electrical Engineers." It has been remarkably so in one or two respects. The past year has been the first in which we have had an Annual Dinner, and the success of that Dinner was such that no doubt it will be repeated in future years. Then, again, the Board of Trade have consulted your Council with regard to the establishment of an electrical laboratory, and other matters, and a good deal of the time of the Council has been devoted to the consideration of those matters.

As regards papers, we have had a pretty busy session, and have had many interesting discussions. I therefore have great pleasure in moving—"That the Report of the Council, just now read, be "received and adopted, and that it be printed in the Journal of "the Institution."

The motion, having been duly seconded, was carried unanimously.

Major S. FLOOD PAGE: I am sure, Sir, that the Report of the Council which we have just heard is very satisfactory, but it seems to me that steps may be taken which may still further improve the position of the Institution. It is only fair to members to state that I had, in ignorance of the rules of the Institution, given notice of two resolutions to be proposed to-night. But I have been informed, in a most courteous manner, by the Council, that the business to-night is strictly confined to the following, viz.:—"To receive and deliberate upon the "Report of the Council on the state of the Institution, and to "elect officers for the ensuing year;" and it has been pointed out to me that resolutions of the kind of which I had given notice should be brought before a special general meeting convened by the Council, or called for by 10 members signing a requisition according to the rules. I do not wish to put the Council to the

trouble, expense, and inconvenience of calling a special meeting, neither do I wish to trouble 10 members to sign a requisition calling upon the Council to call a meeting; but I wish to suggest to the Council that they should take into consideration whether the time has not come when it would be very greatly for the advantage of the engineering profession if the main Institutions connected with engineering could be brought together into closer relations than exist now. I mean especially the Institution of Civil Engineers, that of Naval Architects, the Mechanical Engineers, and the Iron and Steel Institute, with ourselves. Now I do not think there is a single member of this Institution but will say that we should gain by such an arrangement. I do not for a moment suggest that there should be any amalgamation or any absorption: it is clear that such an Institution as the Civil Engineers could neither absorb us nor amalgamate with us, the conditions of membership are so different; but if we had a General Council on something like the lines of the British Association, each of the Institutions retaining its autonomy, I think that the report of the year following such an alliance would tell us that the Institution of Electrical Engineers was more successful than ever. It would result, amongst other things, perhaps, in some such arrangement as now exists at Burlington House, where, you are aware, the Royal Society, the Royal Academy, the Chemical Society, and other Societies are lodged under one roof. I think that if we were to draw closer together we should find that all branches of the engineering profession would gain, while we as the youngest would certainly not gain the least. I therefore beg to ask the Council whether they will take this into their consideration, with the view of increasing the value and usefulness of this Institution even more than it has been improved, as we are all glad to hear, in the last two years. It is only natural it should improve: the industry and the science with which we are connected is the most engrossing branch of science, and the coming industry of the age, and it is but natural that the Institution should improve.

I have another suggestion to make, and it is this: The great development of electricity, although we all hope that it will

soon show itself in motive power, and in other branches, still, at this present moment, the chief development is in the direction of electric lighting; and I am sure that as we walk about London and see the large number of houses that have notices on them that the electric lighting is being done by this or that firm, we are gratified when we see the name of well-known electrical engineers on that board: we feel that the electricity will probably be conducted into that house safely, and that the workmanship will be sound and good. But I think the time has come when it is for the interest of the Institution to exercise some influence in the subject of wiring houses, specially when we read the names of some non-electrical firms that are putting the electric light into houses. I suppose we all agree that it would be a great blow to this Institution, and to electricity in general, if a considerable number of fires were to take place in connection with electricity; and I suppose we also all agree that if electricity is installed in a house safely and well, it diminishes immensely the chance of fire, but that if it is done carelessly and on unsound principles, and by inexperienced persons, it positively increases the chance of fire. We know what has taken in New York, and the large amount of excitement that has been aroused whenever any accident is ascribed to electricity. There was a remark the other day in one of the New York papers bearing very forcibly on this—viz., that the papers were so entirely taken up with the one accident that had happened in connection with electricity, that no newspaper had a single line to give to the 35 accidents that had, during the same time, taken place in consequence of the use of gas. I think that it is immensely for the interest of all who are connected with electricity that we take some step towards ensuring that the wiring of houses is done on sound principles; and it seems to me that this Institution is the natural place for us to come to, to ask the Council to consider whether they cannot introduce something in the shape of an examination. We know that lately examinations have been introduced in the Surveyors' Institution, and in the Plumbers' Company; and the Institution of Civil Engineers itself has lately introduced a system of examination before men

can be elected members of that Institution. I think that in the interests of the science and industry of electricity it is very important that these should be arranged; and I ask the Council to consider whether it will not give a great fillip to the work of this Institution if they can commence some system of examination, so that men—wiremen and others—shall be able to say when they are called upon, "I have the certificate of the "Institution of Electrical Engineers." I am quite sure that men who had such a certificate would be able to get much more work than those who had not. I would therefore ask the Council whether they will take into their consideration the two questions that I have ventured to bring before you, with the view of improving still further the position of this Institution—the one, whether they can place themselves into communication with the Institution of Civil Engineers, and the other Institutes I have named, to see if we cannot draw the whole of the five Institutions closer together, without in any case giving up our autonomy; and, also, whether they can see their way to institute something in the shape of examinations, with the view of preventing any prejudice being created against electric lighting in consequence of fires taking place by houses being badly wired.

I have very great pleasure in supporting the Report which has been brought before us: it is most encouraging. My only object in addressing you here to-night is that I think we ought to march with the times; and surely it is a very good opportunity to take some such steps as I have suggested. We are on the verge of an electrical session of Parliament: since the great railway session of 1846 there has been no Parliamentary session occupied so entirely and monopolised by any one branch of industry, as the session which is now about to commence will be occupied by electric Bills. You are all aware that there are no less than from 430 to 450 different electric lighting schemes that will be before Parliament during next session; and I can conceive no moment in which the Institution of Electrical Engineers can do more good to the general public, and can more clearly show its right to speak for the electrical industry,

than at the commencement of the "electrical session of "Parliament," which is just about to commence.

The ballot-boxes were withdrawn.

The CHAIRMAN: Gentlemen,—The Council, I am sure, will be exceedingly glad to give very careful consideration to the important proposals which Major Flood Page has made. They will take his suggestion and do so at once, and thus avoid the necessity of calling a special general meeting of the Institution.

Professor SILVANUS P. THOMPSON: We are, indebted, Sir, to the Institution of Civil Engineers for the courtesy which they extend to us, year by year, in allowing us the use of their rooms and premises for our meetings, and we certainly cannot do less than give them a vote of thanks for the facilities which they give to us. They save us, no doubt, what would be a very much larger expense if we had to engage rooms of our own, and an establishment as commodious as that in which we hold our meetings. I have therefore very much pleasure in moving—"That the cordial "thanks of the Institution be presented to the President, Council, "and Members of the Institution of Civil Engineers for the "continuance of their kindness and liberality in permitting this "Institution to hold its general meetings in their lecture hall."

The motion, having been seconded by Mr. W. LANT CARPENTER, was carried by acclamation.

Mr. ALEXANDER SIEMENS: I have very much pleasure in moving—"That the thanks of the Institution are due to the "members who so ably represent us abroad as Local Honorary "Secretaries and Treasurers, for their continued kind services in "that capacity." You all know that there are quite a number of gentlemen in various parts of the world who undertake the very invidious task of getting the subscriptions of foreign members together and sending them on here to our Secretary. You will also have observed that at nearly every meeting a number of foreign members are proposed, and they have mostly been induced to send in their names by our Local Honorary Secretaries.

In making this motion it is perhaps invidious to particularise, but in this year I think it is natural that I should mention the name of Mr. John Aylmer specially, because we are all very

grateful for the very kind way in which he managed the affairs at the meeting in Paris.

The motion, seconded by Mr. SYDNEY EVERSHED, was carried unanimously.

Sir JAMES N. DOUGLASS: I have very great pleasure in proposing—"That the thanks of the Institution are due to Mr. "Edward Graves, Past-President, for his kind and valuable services "as Honorary Treasurer." The name of Mr. Graves is so well known that it requires no words of mine to present this motion before you for your hearty acceptance; but you will all be sorry to learn that Mr. Graves is prevented by indisposition from being hêre to-night.

The motion, having been seconded by Professor JOHN PERRY, was heartily carried.

Mr. C. E. SPAGNOLETTI: In all Institutions and Societies the financial position is an important one, and I am sure you will all agree with me in moving—"That our best thanks are due to Mr. "J. Wagstaff Blundell and Mr. Fred. C. Danvers for their kind "services as Honorary Auditors during the current year."

The motion, seconded by Professor S. P. THOMPSON, was carried unanimously.

Sir DAVID SALOMONS: We all know that with civilisation we do not get the advantages that one would expect. One necessary consequence of civilisation is the employment of solicitors, followed by their bills. In our case we occupy a position half-way between the most civilised and the savage state, in having a solicitor, but no solicitors' bills; I think, therefore, that the least we can do is to return our sincere thanks to our Honorary Solicitors for their services. As a rule, these gentlemen have had nothing to do, but on a recent occasion, when we changed our name, they gave their services most willingly, and for that alone our thanks are due, as well as for the duties which will no doubt devolve upon them in the future. I therefore beg to move—"That the thanks of the Institution are due to Messrs. Wilson, "Bristows, & Carpmael for their kind services as Honorary "Solicitors."

The motion, seconded by Mr. GISBERT KAPP, was heartily carried.

The CHAIRMAN: The next business before us is to continue the discussion on Mr. Addenbrooke's paper on "Electrical Engineering in America."

Mr. W. LANT CARPENTER: I was prevented by an engagement out of London from being present to hear Mr. Addenbrooke's paper and the discussion upon it last meeting, but I have had an opportunity of reading it since; and as it fell to my lot to make a very hurried journey to Vancouver's Island and back during August and September last, I was able to see some of the points that Mr. Addenbrooke has alluded to in his paper, and hence I should like to take the opportunity of saying how heartily I concur in all that he said with regard to the temporary and unæsthetic character of the fittings for electric lighting in the Western towns, both in the United States and Canada. I had not the opportunity of going over any of the central stations at all, and my journey being exceedingly hurried, I was only able mainly to observe what one can see in passing rapidly through these cities, and just to converse with various people on the general position of things. One or two points struck me which appear, perhaps, to have escaped Mr. Addenbrooke's notice, or possibly may have escaped my attention when reading his paper, or in the interpretation of it. One was the very extensive use made of water-power in the towns in the mountainous districts, especially on the Pacific slope of the Rocky Mountains; and it certainly was very remarkable to see how in mushroom towns three or five years old, every one of them was lighted electrically, in most cases by the water-power obtained from the streams and falls from the Rocky Mountains and their tributary ranges, and used not only for arc lighting, but for electric traction work. There were some towns that I saw where they had not yet properly formed the streets, but had levelled the place simply, and on that they had put down rough rails and carried out their locomotion entirely by electricity, using overhead conductors, the power being almost entirely derived from water in the neighbourhood. That was very notably the case at the town of Spokane Falls, in Washington Territory, which was burnt down last July: the whole place was lighted by arc lighting in a few hours after the

Mr. Carpenter.

Mr. Carpenter. catastrophe. The same thing applies to a town between Lake Huron and Lake Superior—Sault Ste. Marie—you see exactly the same thing; the power there being taken from the rapids running between the two lakes, the total vertical fall being about 50 feet. But the point that specially forced itself upon my notice was the very small amount of incandescence lamp lighting which was employed in any of these Western towns; and as far as I could make out from conversation with many people, the one great reason for it was that the thing was so badly managed that, as a rule, the life of the lamp was exceedingly short, and in fact they considered that 300 hours was quite a phenomenal life for a lamp. The reason, so far as I could ascertain, appeared to be that there was not that skilled supervision with regard both to the laying down of the installations and their management that we are accustomed to; and so it was that in many towns—I am speaking of these extreme Western towns, both in the United States and in Canada, to which the same remark applies—glow lamps were scarcely employed, and where they had been employed they were frequently given up, the reason being, as far as I could make out, the want of any proper arrangements whatever in the beginning, and also in the running of the installation afterwards.

With regard to the use of water-power,—it is probably within the knowledge of most present that three large towns in Canada—Montreal, Ottawa, and Quebec—are lighted by water-power, in the case of Quebec from the Montmorency Falls, eight miles off; and I heard a great deal about the probability of electric traction being derived from the same source. In conversation with people at Ottawa I was told, although I did not see it, that the alternate currents which were used so very much there for lighting, which were derived from the Chaudière Falls, were also used for power in a good many instances by the Tesla motor; I do not state this of my own authority, because I had not time to see for myself, but I was told that a large number of Tesla motors were employed commercially in Ottawa with these alternating currents.

The only other point on which I would trouble the meeting is the large number of wires used for long-distance telephone

working which were running out of New York—my attention was specially called to them—running to Buffalo and a large number of other towns. Long-distance telephony seems now to be a very easy thing, and largely used. Mr. Carpenter.

There has been a great reduction in the overhead wires in New York, Chicago, and other large towns during the last four or five years, compared to the Western towns of which I have been speaking, and there seems to be very much more underground work done. I am here contrasting what I saw in the streets of large cities in 1884 and 1889.

Mr. G. S. RAM: I have lived a year and a half in the States, and can therefore endorse a good many of the things that Mr. Addenbrooke has said. I do not altogether agree with his remarks about the glare of the arc lighting when done on the poles in the ordinary way. In New York there is no arc lighting on the tower system; there were two towers of great height in Union Square about two years ago, but they were abandoned in favour of the arc lights on ordinary poles; and it may be that one gets used to it, but I certainly very much prefer the "blazing light" in New York to the comparative "utter darkness" in London. Mr. Ram.

They have in America many ways of doing things which appear strange to the Britisher. For instance, I have seen in one of the largest hotels in New York, lighted by its own Edison plant, the large Edison dynamo being run with large blocks of ice on the bearings to keep them cool. This, the engineer told me, was the way he always ran the machine. It was probably not done at the direction of the Edison Company, but on the sole responsibility of the engineer, who had developed a theory that ice was cheaper than oil.

Mr. Addenbrooke's remarks on the overhead work might be thought by anyone who has not seen it for himself to be possibly somewhat overdrawn and exaggerated; but Mr. Addenbrooke, I think, was strictly speaking within the limits when he said that everything has a temporary and half-done look about it which is very offensive to English eyes. In order that those who have not seen the work may be able to more fully appreciate Mr. Addenbrooke's description, I have brought here a photograph of a bit

Mr. Ram. of Broadway, New York, which I took in the beginning of this year. When I took the photograph it was my intention to get a picture of the building, not the wires; but the result is such that one may say it is a picture of wires, with a building in the background. Only the thicker and nearer wires can be distinguished in the photograph, but in a transparency I have made, by the use of a magnifying glass hundreds of wires can be distinguished, while with a more powerful glass the details of the way in which they are fixed to the poles can be seen. There is one post with 17 cross-bars, each of which carries 10 green glass insulators, besides one on the top of the pole, and every insulator appears to carry a wire. A number of wires seem to run up and down and round this pole, I do not see for what purpose, and a number of ends are coiled up and left hanging. There are also a number of thickish insulated wires, which are probably arc light wires covered with "underwriters' insulation," which are fastened to the pole itself, and not to the insulators at all; and a number of these are seen to rise to a much higher pole, with its cross-bars at right angles to the first, about 10 feet off, and to which they are apparently tied below the cross-bar. On this pole one of the cross-bars is twisted round, and appears to be waiting for a gust of wind to blow it down. Between these two poles there is a third pole, from which wires radiate in all directions. With the aid of a glass the insulation of many of the wires may be seen to be hanging in shreds. This spot was not selected for the purpose of showing the wires, but I think it gives a fair average sample of electrical engineering out of doors on Broadway, New York. [*The photograph and transparency were handed round for examination.*]

Mr. Spagnoletti. Mr. C. E. SPAGNOLETTI: The necessities for insulation in the two countries, England and America, are so widely different that that may account for the difference in the appearance of the style of work in each to a very great extent. It will, perhaps, be remembered by many here that when Mr. Cromwell Varley went to America, many years ago, he was very much struck with the careless way in which the insulation was attended to there, and there was rather an animated correspondence in the papers

on that subject. Here, in England, we have, from our own experience, a knowledge as to how the insulation of the wires varies. On the Great Western Railway, in the South Wales district, one week our wires will test exceedingly high—some 8,000,000 or 10,000,000—and perhaps the next week, with continuous showery or drizzling rain, we may get insulation results on the same wires below 200,000 ohms. Some years ago Mr. Edison was over here with a new instrument—a modification of Bain's chemical printing telegraph—and he was trying some experiments at the Post Office. The instrument was a very sensitive one, and the induction in the underground wires through London was such as to very much interfere with it—in fact, to render working impossible. Upon the request of Mr. Culley, who was then the Engineer-in-Chief of the Post Office, we arranged to give Mr. Edison a room at our Ealing Station, into which one of the Post Office wires from Liverpool was led. Mr. Edison was in that room making experiments for about a fortnight, and he told me that in New York, without the slightest difficulty, he could get 2,000 words a minute out of this machine, but the most he could do under the best conditions that he could get over here was only at the rate of 614 words a minute. This will show the great difference of the two atmospheres; and, as Mr. Varley then said after his experience there, he thought that in America they might work a very long line with very inferior insulation, as the atmosphere there was totally different from that of this country. Mr. Spagnoletti.

Mr. W. H. Preece, F.R.S.: I should just like to add a rather interesting fact to what Mr. Spagnoletti has just communicated about Mr. Edison. When he came over here with the electro-magnetic shunt business, he thought he was going to do great things, and before he came to the Post Office he made some experiments on circuits belonging to the Eastern Telegraph Company, who gave him the use of a cable that was lying in the tanks of the Telegraph Construction Company at Greenwich. Mr. Edison wanted to study the effect of his automatic system on cables, and they gave him the use of this cable for a whole night. He told me this himself, and he said, "Now the first thing Mr. Preece.

Mr. Preece. "that I wanted to know was the length of a dot." The length of the cable he was experimenting with was some 800 or 1,000 miles—I forget the exact length—and he said, "When I put a "dot into that cable, what do you think was the length of it "when it came out at the other end?" I said, "I do not know; "perhaps six inches." He said, "Six inches! it was 28 feet: I "thought that dot was never going to end." That is a very interesting anecdote, because it showed that at that moment few of us really knew what the effect of self-induction in a coiled cable was. On this point it is also interesting to know—and I think I have mentioned it in this room before—that the first Atlantic cable, when coiled in the tanks at Greenwich, gave a speed of about 1½ words per minute; and Sir William Thomson, Varley, and Fleeming Jenkin brought out their curb key, which was going to increase the rate of working of the Atlantic cable from 1½ words per minute to about 4 or 6 words per minute. It was in 1866. The cable was laid; the key was brought down to Valentia. To their surprise they found that, with the ordinary single key, they were able to get 15 words a minute through the Atlantic cable; the reason being simply that they had experimented with a coiled cable in tanks in the first instance, and then with a cable laid straight along the bottom of the Atlantic, when the effects of self-induction disappeared, and there remained only the effect of electrostatic induction to retard working.

To go back to another question referred to by Mr. Spagnoletti, I have previously pointed out here that the great reason why there is such a difference between the insulation in the United States and the insulation in this country is that in the United States the prevailing winds from the ocean are from a cold region to a warm one: the shores of the northern straits of America are washed by the Arctic current, and the prevailing winds are cold and dry winds. In this country it is the reverse: we have our shores washed by the Gulf Stream, and the prevailing winds are the S.W. winds, which come upon our shores colder than the ocean, laden with moisture, and the result is that we have dampness; and thus there are difficulties that render insulation with us extremely difficult.

I do not feel at all able to-night to say anything about the paper of Mr. Addenbrooke, nor to take part in this discussion. I have twice visited the United States, and on each occasion I have given the result of my visit there to this Institution. But I have never come from the States without feeling benefited; and I have no hesitation whatever in saying this—that if any marked improvement has taken place in the management or in the arrangement of the working of the telegraphs of this country through my exertions, I believe that it has been greatly due to the contact that I have made with the energy and the go-a-headedness of our friends on the other side of the water. They have difficulties to surmount that we do not meet with here; we have difficulties to surmount that they do not know of there; and the tendency is that we are rather induced to criticise their doings from our point of view, while they criticise our doings from their point of view. I have never yet met an American electrician who has come over to this side of the water who has not admired something that we do, and gone back feeling a wiser man; and I am quite sure that there is not a single member of this Institution who would go to the other side of the Atlantic who would not come back again and say that he had learned a lot, and felt after it a much better man. Mr. Preece.

Mr. R. E. CROMPTON: The points I notice as specially interesting are the device which the author mentions as having been adopted in America for preventing the vibration of moving machinery being communicated to adjoining premises. This appears to consist in placing the dynamos on a wooden platform supported on wooden posts, and the platform not being connected at the dynamo level to the walls of the building. The experience that I have had in England has shown me that such an arrangement in some cases may actually increase instead of diminishing the vibration, with the result that the increased vibration communicated down through the ground to the adjoining walls is actually greater than if the moving machinery had been placed on more rigid foundations. I note that at the Edison station in Chicago a considerable number of resistances and indicators are used for the various feeder circuits. This appears to me as if Mr. Crompton.

Mr. Crompton. they were there ignorant of later practice, as in the best-managed modern systems which employ feeders it has been found quite possible, by careful design of the network and feeder system, to avoid the use of such variable resistances. The hatchet-shaped switch described by Mr. Addenbrooke is very largely used in England and on the Continent, as well as in America. He is quite right in saying that it is one of the cheapest and best switches that have been hitherto designed. I note also that Mr. Addenbrooke re-states the fact which I have always insisted upon, viz., that up to the present date alternating-current machines are not as a matter of practice run in parallel in the American central stations. This has been over and over contradicted in this Institution, and it is interesting to see Mr. Addenbrooke confirm my views.

The vertical boiler described in the paper is interesting to a mechanical engineer from the fact that apparently, in this case, it gives no trouble. The use of such boilers has been frequently attempted in this country, but there are so many disadvantages and dangers connected with it that it has in most cases been given up. The upper part of the tubes which pass through the steam space above the surface of the water is liable to get so hot that they consequently collapse and give great trouble. I can also state from personal experience that it is very difficult to make the upper ends of these tubes tight in the upper tube plate.

I regret much that we have little chance of availing ourselves of the interesting information Mr. Addenbrooke gives us of the various methods of overhead connections for electric tramways. I have no doubt that this method of supplying the electrical energy to tramways is the best and cheapest of all; but we have to overcome the æsthetic prejudice of the municipal authorities, and I am sure that this will be so strong that we have hardly a chance of using the overhead system.

The whole of Mr. Addenbrooke's paper is very interesting, as it shows what very great advantages American electrical engineering has enjoyed in the license that has been given them to carry out pioneer work in the roughest possible manner. I do not think it right to severely criticise such work, but consider that it

has been a great service in popularising the demand for electricity. Mr. Crompton.

Mr. A. RECKENZAUN: I quite agree with Mr. Preece's remarks, which were to the effect that those of us who visit the United States cannot return without having learnt something, and *vice versa* for American electrical engineers visiting this country. Mr. A. Reckenzaun

I am very sorry to notice that Mr. Addenbrooke uses some hard words with reference to electrical engineering practice in America. He seems greatly troubled about posts, and says that "when posts are used they are of rough pine, never painted, "often out of straight and warped and bent." I have seen a number of these posts: they are unsightly certainly, whatever shape they may be; but there are some posts in the States, especially in larger cities, which are straight, smooth, and painted, and there are also posts which are quite ornamental, made of cast or wrought iron.

Mr. Addenbrooke's paper seems to be a criticism upon the bad work done in America, and he says so little about the good work that is done there; therefore one hardly knows where to commence discussing his comments, and where to leave off. He says little or nothing about central stations and isolated plants in which secondary batteries are utilised. There has been a considerable development in this direction, and some mention ought to have been made, if only superficially, in a paper of such pretentious title. There are numerous central stations and isolated plants which are worked with storage batteries, as, for instance, at Detroit, feeding 1,600 lamps; Jersey City, 1,000 lamps; Honesdale, 500 lamps; Haverford College, 500 lamps; Cherryfield, 500 lamps; Watsontown, 500 lamps; Allentown, 500 lamps; the Metropolitan Telegraph and Telephone Company, New York, 1,100 lamps; Mill's Buildings, New York, 1,000 lamps; and the American Express Company, 250 lamps. These are only a few which my brother, who is here from San Francisco, mentioned to me at random.

There is one very important subject—a great and thriving industry in the United States—and that is electric railway engineering. Concerning this Mr. Addenbrooke says nothing

Mr. A. Reckenzaun beyond a few general statistics which have been published in the newspapers, and one is inclined to think that he has either not seen any electric railways, or if he has seen some he has not taken the slightest notice of them. Such matters would have been far more interesting to us than a list of faults which his critical eye happened to discover.

Mr. F. Reckenzaun Mr. FREDERICK RECKENZAUN: I think my brother has well covered the ground on which I should raise a discussion. It is true, electrical engineering practice in America differs somewhat from European practice. While overhead wiring is done to some extent in a "slipshod" way, to use Mr. Addenbrooke's expression, there is, on the other hand, a considerable amount of skill displayed in this class of work by the larger companies, such as the Brush, Thomson-Houston, United States, and others, who treat it as a special department. Overhead wiring, properly done, combines economy with efficiency and simplicity. As to the construction of arc lamps, I might say that we certainly do not see such ornamental lamps in America as we see here and on the Continent. The American arc lamps are plain and simple, but answer the purpose; what is wanted is not so much ornaments as illumination. It is certain that cheap construction reduces the cost of the electric light and facilitates its introduction. In New York, for instance, the arc light prices are so low that many small storekeepers could afford to use it, both inside and out. This has added to the illumination of the streets, which otherwise was in many places defective.

In regard to incandescent lighting, Mr. Lant Carpenter gave as the reason why it is not extensively introduced in the West, that such plants have been badly managed. This is true, perhaps, to some extent. On the other hand, Western people—who, as a rule, jump at new things, and want the biggest—often have in their towns an arc lighting plant before they have a gasworks; they find the arc light not only the biggest, but the most economical for their purposes. Incandescent lighting is rather looked upon as a refinement to be added later on. It costs a great deal more in construction as well as for operation, per unit of light, than arc lighting; and in many places where in

Europe they would not use arc lights, such as restaurants or offices, they use it there, and do not mind the little flickering and the glare which it brings with it. Mr. F. Reckenzaun

Mr. G. L. ADDENBROOKE, in reply, said: I think that as such a large number of points have been raised by the various speakers, I had better rather take them together than attempt to reply to every one separately. Mr. Addenbrooke.

Referring, first of all, to what Mr. F. Reckenzaun said about the use of arc lights, I must say that I was struck with the way in which they were employed in little saloons and places 12 or 14 feet square, and not much more than 10 feet high; an arc lamp often being suspended in the middle, when, of course, the light seems really brighter than daylight.

I would say that I *do* mention in my paper the *good* construction of the Thomson-Houston Company—at any rate, in New York. I specially mention that, and I also may state that they ran their lighting circuits during the time that the other companies suspended their operations in New York owing to the extent of the difficulties which had arisen with the Town Council.

There is no doubt that the tramway companies are bringing in the use of a better class of posts. First of all, they appear to have started with an even poorer class of posts than were being used for other things. Most of their posts were arranged one on each side of the street, with a cross-wire carrying the conductor in the middle; but they are now largely giving up that class of structure for neat wrought-iron posts. These posts are arranged in the centre of the two tracks, with an arm on each side, if the track is a double one; or, if the track is single, the posts are at the side of the road, with one arm projecting. It is easy to put lamps on these posts, which are useful to prevent vehicles running against them. The electro-motive force is, as a rule, 500 or 546 volts. Five lamps are put in series on every other, or third, pole, bridging across the mains: the lamps burn very well, and, of course, do for lighting the streets. I may say that the same method of using five lamps in series is used for lighting the cars.

With respect to what Mr. Crompton said about the construc-

Mr. Addenbrooke. tion of the flooring of central stations, which I described as separated from the building, and only steadied from the sides: I did not intend to imply in my paper that this was to reduce the vibration of the machinery, but it was to prevent the vibration of the machinery affecting the building. The upright boiler that I described as used by the Thomson-Houston Company may be subject to the defects which Mr. Crompton mentioned; all that I can say is, that there were three of them in use in the Thomson-Houston station in New York, and that Mr. Foster, the superintendent there, told me that they proposed increasing the number.

A remark was made by Mr. Lant Carpenter as to long-distance telephony. I had the pleasure of speaking from New York to Boston, through one translator coil, and I must say that the talking was excellent. The distance is 250 miles. I was in the central office at New York, and I spoke to one of their officials who was in an office situated at some distance from the Exchange in Boston.

At the last meeting Sir William Thomson alluded to the rough-and-ready methods which the Americans employ. There is no doubt that they have enjoyed, and do enjoy, enormous facilities from this way of going to work; and although I have strongly criticised it, yet I do not wish to criticise it entirely in a spirit of condemnation.

Turning now to the difference between the Americans and ourselves, we are accustomed to talk of English engineers and American engineers. I was rather surprised when I was there at the number of engineers I met in America who were born in England. They might have been out there twenty years or less, but really the proportion I met was quite large. From all I observed I do not think that there is really very much difference between American engineers and English engineers. It is the conditions of the country that are different, and the engineers over there have adapted themselves to it. When I was in Australia I saw very much the same class of thing there. Englishmen out in Australia adapt themselves to the country. Electric lighting has not developed in Australia to the same

extent that it has here; but still, out there, in engineering matters they do adapt themselves to the necessities of the country in the same way that they have done in America. Mr. Addenbrooke.

Then there is another point I noticed, not exactly in connection with electric lighting. It is that in America the Town Councils, Mayors, and the authorities of the towns seem to be stronger than they are here. Of course it is a democratic community; but when they put a man in a place, he seems to have a great deal of authority in that place; and I think that the American corporations of the towns perhaps have allowed all this overhead work to go ahead in a sort of good-humoured spirit to let people have a chance, with the feeling that they could put their foot down, and that they would put their foot down, and stop it whenever they thought fit to do so. You see frequent references and cases in the papers of the Mayors speaking on all sorts of subjects in an authoritative manner which I do not think would be used in this country.

I was at the last meeting asked some questions about motors. I did not see much of motors on arc light circuits—or, rather, run from arc light machines—because, of course, there would be some difficulty in running motors in the day on arc circuits without the lamps burning. I think they are mostly series motors, with arrangements for cutting out more or less of the field-magnet coils, and with a switch for short-circuiting the motor when not required to be used.

No doubt the climate in America is very different from what it is here. To take an instance, I think that the rainfall in the region about Salt Lake City is not much above 9 or 10 inches a year, and a great part of it occurs in the winter when there is snow. Even when the rain does come, it comes quickly; they do not get the long, drizzling rains that we get here.

The question of alternating-current motors has been alluded to. I was told that the Westinghouse Company have expended an enormous sum on experiments in that direction, and a fair number are in use, but I was unable to get any original statistics that I could bring forward as to their efficiency or the kind of satisfaction they were giving.

Mr. Addenbrooke.

With regard to meters it was much the same thing. I was told that the Westinghouse Company—and they are advertising it—have got 8,000 meters in use. The Americans are not long in saying that they have a result when they have got it, and, no doubt, experimenting on this scale with an apparatus which in many ways fulfils the requisite conditions, the natural progress and improvements which inevitably follow when anything once gets into extended use will enable a good working meter to be completed.

I do not know that there is anything more that I can usefully add, except that I might perhaps say how much the hospitality which was extended to the American electrical engineers over here during last summer was appreciated. I found that everyone in America was in a very good temper with everybody here, and disposed to be exceedingly courteous, while the opinion was universally expressed of how much kindness they had met from their English brethren.

Dr. Hopkinson.

The CHAIRMAN: I am sure you will all join in giving a hearty vote of thanks to Mr. Addenbrooke for his paper. It is an interesting paper in itself, and not only that, but it has given rise to an interesting discussion.

A hearty vote of thanks was unanimously accorded to Mr. Addenbrooke for his paper.

rofessor hompson.

Professor S. P. THOMPSON: May I be allowed to draw the attention of the meeting to the blue glass insulators which have been placed on the table? I brought them from America five years ago, and I have now brought them down here to illustrate Mr. Addenbrooke's paper. I happened to catch sight this week of a book newly published by a Chicago engineer of great ability, which is now being circulated in this country as a guide to those who have to deal with dynamo machines. In that book the blue glass insulator with the wooden pin is specially recommended as the proper thing for running arc lamp circuits!

Mr. Addenbrooke.

Mr. G. L. ADDENBROOKE: I would mention that I forgot to allude to the fact that I have laid on the table specimens of the class of cable which is now very largely being put into the con-duits in New York. The insulation is very hard, and is more like onite than india-rubber.

The Scrutineers handed in their report of the result of the ballot for Council and Officers for the year 1890, which the SECRETARY announced to be as follows:—

*President:*

J. HOPKINSON, M.A., D.Sc., F.R.S.

*Vice-Presidents:*

WILLIAM CROOKES, F.R.S.
Professor W. E. AYRTON, F.R.S.
ALEXANDER SIEMENS, Assoc. M. Inst. C.E.
R. E. CROMPTON, M. Inst. C.E.

*Ordinary Members of Council:*

Sir JAMES ANDERSON.
Sir ALBERT J. L. CAPPEL, K.C.I.E.
Major PHILIP CARDEW, R.E.
W. LANT CARPENTER, B.A., B.Sc.
Sir JAMES DOUGLASS, F.R.S.
Captain Sir DOUGLAS GALTON, K.C.B., D.C.L., LL.D., F.R.S.
Colonel R. RAYNSFORD JACKSON.
GISBERT KAPP, Assoc.M.Inst.C.E.
Sir HENRY MANCE, C.I.E., M.Inst. C.E.
Professor JOHN PERRY, M.E., D.Sc., F.R.S.
Sir DAVID SALOMONS, Bart., M.A.
Professor SILVANUS P. THOMPSON, B.A., D.Sc., F.R.A.S.

*Associate Members of Council:*

AUGUSTUS EDEN.
FRANCIS H. NALDER.
SIDNEY SHARP.

*Honorary Auditors:*

FREDERICK C. DANVERS.
AUGUSTUS STROH.

*Honorary Treasurer:*

EDWARD GRAVES, Past-President.

*Honorary Solicitors:*

Messrs. WILSON, BRISTOWS, & CARPMAEL, 1, Copthall Buildings, E.C.

The PRESIDENT proposed a vote of thanks to the Scrutineers for their labour in the examination of the ballot lists, which was carried unanimously.

The CHAIRMAN: Gentlemen,—I thank you very heartily for the great honour you have done me in electing me as your President for the coming year. I take it as a great honour to be elected to preside over such an Institution as this—an Institution which has already attained to great celebrity and importance in the industries with which it is connected, and which, I trust, in the future will attain to still greater importance, and still greater utility. I take it to be an honour, too, to succeed in this chair so distinguished a man as Sir William Thomson, the most distinguished electrician living. I can only say, gentlemen, that I will do my very best to maintain the honour of the position to which you have been so very kind as to elect me.

A ballot took place, at which the following candidate was elected:—

*Associate:*

Septimus Felix Beevor.

The proceedings then terminated.

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# LIST OF ARTICLES

RELATING TO

# ELECTRICITY AND MAGNETISM

**Appearing in some of the principal Technical Journals during the Months of NOVEMBER and DECEMBER, 1889.**

## I.—BATTERIES AND ACCUMULATORS.

— Krouchkoll—Polarisation of Metals by Immersion in a Liquid, by Motion in the Liquid, and by Withdrawal from the Liquid.—*Jour. de Phys.*, vol. 8, p. 519, 1889.

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P. Hahn—Relation of E.M.F. to Speed.—*Lum. El.*, vol. 38, pp. 374, 423, 1889.

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P. H. Ledeboer—Electro-Metallurgy of Aluminium.—*Lum. El.*, vol. 34, p. 159, 1889.

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— MASSIN—Elimination of Induction on Two Parallel Telephone Wires.—*Lum. El.*, vol. 34, p. 130, 1889.

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# APPENDIX.

## COMMUNICATION.

THE UNIVERSITY, GLASGOW,

*February* 11*th*, 1890.

With regard to the remarks upon submarine telegraphy by Mr. Preece, at the meeting of the 12th December, 1889,* which have just come under my notice, the following may be of interest to members:—

The first Atlantic cable was never coiled in tanks at Greenwich. It was made, half at Birkenhead, and half at Greenwich. The two parts were joined in one circuit for the first time on board the "Niagara" and "Agamemnon," at Queenstown, at the end of July, 1857, and I then got signals through it at something less than one word per minute with the special instruments which had been prepared by Mr. Whitehouse. When the cable was laid, in 1858, we obtained about three words per minute by hand-signalling, with my mirror galvanometer as receiver, during the three weeks of successful working of the cable. In this connection I may quote the following extract from a letter of mine to the *Athenæum*, dated October 24th, 1856, and published November 1st, 1856:—"A mode of operating so as to clear the "wire rapidly of residual electricity, which I have worked out "from theory, and a plan for telegraphic receiving instruments to "take the most full advantage of it, which has recently occurred "to me, allow me now to feel confident of the possibility of "sending a distinct letter every 3½ seconds by such a cable,"† or about 3½ words per minute.

* *Journal*, p. 844, above.

† "Collected Mathematical and Physical Papers," article lxxvi., vol. ii., p. 101.

It was the second (1865), and not the first (1857), Atlantic cable that was coiled in tanks at Greenwich, and it was in connection with this cable that Mr. Varley and Mr. Jenkin acted along with me.

Varley and Jenkin and I were perfectly aware of electro-magnetic induction,* and we were more pleased than surprised when we found that, after having promised to the company eight words per minute, we actually obtained, by hand-signalling, with mirror-galvanometer as receiver, 15 words per minute through each of the two cables (the second and third Atlantic cables), when the laying of both across the Atlantic was completed in 1866.

WILLIAM THOMSON.

* See "Remarks on the Discharge of a Coiled Electric Cable," British Association Report, 1859, part 2; or my "Collected Mathematical and Physical "Papers," vol. ii., article lxxx.

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